



UPPSALA UNIVERSITET

MASTER THESIS

Does Refugee Migration Make Right-wing Populists More Popular? Evidence from a Swedish Refugee Dispersal Program.

Author:
Viggo BARMEN

Supervisor:
Linna MARTÉN

UPPSALA UNIVERSITY

Department of Economics

June 7, 2019

UPPSALA UNIVERSITY

*Abstract*Faculty of Social Sciences
Department of Economics

Master of Science

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by Viggo BARMEN

Explaining the rising support for right-wing populist parties in Europe during the last decade is an issue that interests both economists, sociologists and political scientists. A number of theories suggest that the rising inflow of migrants to Europe has had an important causal effect on right-wing populist support.

However, as migration patterns generally are not exogenous to right-wing populist support, it is difficult to interpret the estimates of a correlation study causally. In this paper, I exploit a Swedish refugee dispersal program as a natural experiment to estimate the effect of refugee inflow on the support for the right-wing populist party the Sweden Democrats using an instrumental variable strategy. Despite detailed institutional knowledge, I am not able to find support for any short-term effects of refugee inflow on the self-reported preferences for the Sweden Democrats. This goes against the findings of most previous studies.

However, the multicollinearity of some of my covariates are high. In addition, as the program was introduced in 2016, there are few years available for identification. Thus, the precision of the estimates is relatively low and the study would benefit from adding more years to the panel data set.

Acknowledgements

I would like to thank my thesis supervisor Linna Martén for her detailed and helpful comments on the drafts of my thesis, for sharing her econometric and computational expertise with me, as well as for the interesting discussions we have had throughout the semester.

The idea to this thesis was first developed as a term paper in the course "Political Economics" in the fall of 2018. I would like to thank the course teacher Mounir Karadja for our valuable discussions about the early stages of my paper.

This thesis would not have been possible without the support from the experts of the Swedish Migration Agency, the Swedish Public Employment Service and the County Administrative Boards of Sweden. I would especially like to thank Jan Sundqvist and Ulla Perman at the Swedish Public Employment Service, Sofia Papanikolaou and Jens Carlander at the Swedish Migration Agency and Annelie Rosstedt at the County Administrative Board of Stockholm for generously sharing their time and expertise with me.

Throughout the entire thesis writing period, my work has benefited a lot from inspiring and frequent discussions with fellow master student Agnes Ortman. Thank you for the long and frequent breaks without which the thesis would not be as good as it is today.

I would also like to direct my warm thanks to the other fellow master students at the Department of Economics for your valuable comments during the two seminars at which I presented the drafts of this thesis. I particularly want to thank Malin Backman, who did an excellent job as the discussant of my thesis at the final seminar.

Lastly, I would like to thank Elin for her unwavering emotional and practical support. I apologize for being so absent this semester. Thanks also to Gerhard, who taught me the importance of writing well in an academic setting, and to Ingrid who first aroused my interest for migration policy through her long-term commitment to refugee and integration issues. I am always impressed and inspired by the integrity, the dignity and the stubbornness with which you handle challenges in life.

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1 Introduction

Over the last years, the increased popularity of right-wing populist parties all over Europe has arguably been the most important change in the political landscape of the continent. Parties like the National Front in France and the Alternative for Germany in Germany have increased their vote shares rapidly. They are now steadily rooted in both regional and national parliaments. In countries like Hungary, Poland, Italy and Austria, right-wing populists govern in different forms of coalitions. Sweden has its own right-wing populist party, the Sweden Democrats (SD), who won their first seats in the Swedish Riksdag (the national parliament) in the general elections of 2010. Since then, they have steadily increased their vote share, reaching 17.53 percent in the 2018 elections (SCB, 2019).

A common characteristic for most of these movements is their anti-immigrant sentiments and their commitment to stop or decrease the inflow of migrants from non-OECD countries to Europe. They also emphasize the importance of the national state, national identity and national sovereignty and many regard supranational organizations with suspicion. For the Sweden Democrats, an important political issue is to reduce the number of migrants and refugees that come to and stay in Sweden.

The discussions about the reasons behind this change in the political landscape have been intense, both in the popular debate and in academia. There are two main groups of theories that aim to explain the development of right-wing populist party support. The first group of theories relates the increased support for right-wing populists to changes in socioeconomic conditions, such as rising trade competition, economic inequality, unemployment and economic insecurity (for instance Autor et al. (2016), Bó et al. (2018), Facchini and Mayda (2009) and Dehdari (2018)).

The other group of theories instead emphasize the importance of immigration in itself. The idea is that the increasing right-wing populist support can be explained by natives' fear of losing control over the community or by xenophobia triggered by competition over jobs, housing and social welfare (for example Hjerm (2007), Rydgren and Tyrberg (2016), McLaren (2003) and Bowyer (2008)). In this paper, I focus on testing theories that make predictions about the effects of immigration in itself on right-wing populist support. However, I will also attempt to study whether socioeconomic conditions might be important to understand heterogeneous reactions to immigration.

In this thesis, I try to estimate the causal effect of refugee inflow on the support for the Sweden Democrats. Following the bulk of previous literature, my hypothesis is that there is a positive effect of refugee inflow on the support for the Sweden Democrats. In order to test the theories in the research field, it is crucial to identify the underlying mechanisms behind an observed effect and to find out whether the results are driven by certain groups. For this reason, I estimate my specifications for a number of subgroups in order to identify heterogeneous effects. In order to identify a causal effect, I exploit a 2016 reform that redistributed refugees between Swedish municipalities, effectively creating exogenous variation in migration between municipalities. Using an IV strategy on individual level data with municipality and time fixed effects, I consider a causal interpretation of my results credible.

There has been a number of studies (such as Barone et al. (2016), Edo et al. (2019), Hangartner et al. (2019) and Harmon (2018)) trying to estimate the effect of changes in refugee migration on the preferences of voters, most of which have found a positive effect of refugee migration on right-wing populist support. However, to the best of my knowledge, I am among the first to evaluate the effect of a refugee dispersal reform on the support for right-wing populists (the only previous studies I know about are Dustmann, Vasiljeva, and Piil Damm (2018) and Mehic (2019)). The issue is highly policy relevant as many governments may need to consider forcing localities to take on refugees as global migration flows are expected to increase over the coming decades. There are also ongoing discussions about implementing a similar system on a European level. To collect more knowledge about how such reforms affect the support for extreme right-wing parties is therefore important.

I argue that there is hardly any other study of refugee dispersal reforms that introduces so detailed knowledge about the allocation mechanism at work. Therefore, I believe that my claim of instrument exogeneity is more credible than that of previous papers. Hence, my study is able to test how robust previous estimates are to more detailed models of how refugees are reallocated through the program (in particular, I am able to test if previous model specifications fail to control for covariates that are correlated with the error term).

In addition, as I use individual level survey data in my outcome variable, I am also able to delve deeper into the mechanisms through which the causal effect is mediated than previous studies (such as Mehic (2019)). In particular, I am better able to identify heterogeneous effects. Thus, I can test several of the most common theories about the mechanisms behind the observed effects of refugee migration on right-wing populist support (such as the *group position theory*, the *ethnic competition theory* or the *contact hypothesis*). Furthermore, the survey data enables me to measure the outcome each year. Thus, I am able to study the short term reactions to refugee migration more carefully than previous papers who predominantly focus on the effect on election results (that are only measured once per election cycle).

In order to perform the study, I use panel data on refugee migration as well as political, economic and demographic characteristics at the level of Swedish municipalities. This data comes from the Swedish Migration Agency, Statistics Sweden and the Swedish Association of Local Authorities and Regions, respectively. In addition, I use repeated cross-sectional individual-level survey data from the National SOM Surveys.

I am not able to find any support for the hypothesis that there is a positive effect of refugee migration on right-wing populist support. However, my study is limited to short-run effects on the propensity to state a preference for the Sweden Democrats. In addition, the standard errors of my estimates are relatively large, probably due to multicollinearity between some of my covariates as well as because of the few years of panel data available. Interestingly, I find that the results are very sensitive to variable definitions and changes in the included covariates. In particular, some specifications that are similar to the most common specifications in the previous literature perform poorly in my placebo tests.

The remainder of the paper proceeds as follows. Chapter 2 presents the theoretical framework, Chapter 3 provides a review of relevant literature, while Chapter 4 explains the institutional background. Chapter 5 presents the characteristics of the data and Chapter 6 discusses the empirical method. In Chapter 7 I present my results. Chapter 8 concludes.

2 Theoretical framework

In this chapter, I discuss the dominating theoretical models that have been suggested to explain the increased support for right-wing populist movements in Europe.

There is no theoretical consensus about what factors that are likely to have caused the rise in extreme right-wing party support over the last decades. Instead, a number of different theoretical explanations have been proposed. Most of these theories can be attributed to one of two broad categories. The first group of theories claim that changes in socioeconomic conditions have caused the observed increase in popular support for extreme right-wing parties. The second group of theoretical models instead rely on sociological theories about how individuals use subjective perceptions of identity when deciding how to react to changes in their community. According to these theories, natives' support for anti-immigrant right-wing populists emerges as a reaction to immigration. Below, I focus on theories relating immigration to right-wing populist support as these are the theories I am able to test in this thesis.

2.1 Theories about socioeconomic factors causing right-wing populist support

There are a number of theories that relate the increased support for right-wing populists to changes in socioeconomic conditions. Dehdari (2018) provides an excellent overview of the theories in this group of explanatory models. Some authors in this strand of literature claim that changes in socioeconomic conditions have caused the increased right-wing populist support directly (voters support these parties in order to express their protectionist or anti-globalization sentiments or as a protest towards a perceived "establishment"). Others claim that socioeconomic conditions instead determine how natives react to immigration. However, a common trait of these theories is that they claim that the increased right-wing populist support in Europe can be explained by changes in socio-economic conditions rather than by immigration in itself.

According to the neo-classical labor market model of migration developed by Borjas, Freeman, and Katz (1996), the effects of migrant inflow on natives depend on the human capital of the migrants. If the average human capital level among refugee migrants is lower than that of the natives, then refugee migration is likely to benefit high-skilled natives while hurting low-skilled native labor (as low-skilled refugee migrant labor is a complement to the former, but a substitute to the latter). Assuming rational agents, we would therefore expect low-skilled natives to resent immigration but high-skilled natives to support it.

Facchini and Mayda (2009) develop a neo-classical model that instead focuses on the burden of immigration on taxes and transfers. In a European welfare state, with a tax system that redistributes resources through a publicly financed education and health care system as well as generous unemployment benefits, refugees are likely to be net recipients of social welfare (at least during the first years in their new country). Accordingly, one would expect that net payers to the social welfare

system (generally high-skilled individuals) would be less supportive of refugee immigration than their low-skilled fellow native citizens (that are not net payers to the same extent).

Another famous model is the *social marginalization hypothesis* discussed by Rydgren and Tyrberg (2016). According to this hypothesis, voters in marginalized areas may feel let down by the "established" parties, causing them to support radical right-wing populists in protest. The social marginalization hypothesis could also transform into *welfare chauvinism*. According to this theory, natives support right-wing populists in order to protect their access to social welfare and other benefits. The idea is that natives fear that the resources used to provide those benefits will be spent on refugees instead (if the country pursues a generous immigration and refugee policy). Voting for an austere refugee policy might thus be rational for these individuals.

Autor et al. (2016) proposes yet another theory, where voters support right-wing populists because of rising trade competition. The idea is that right-wing populists normally are not only against migration, but also against globalization. If voters believe that international trade competition threatens their jobs, voting for extreme right-wing parties in order to secure protective trade policies might be the rational thing to do.

2.2 Theories about immigration causing right-wing populist support

Another group of theories relate the increased support for right-wing populists in Europe to an increased inflow of immigrants and refugees. The main difference from the theories about socioeconomic factors is that the theories in this section explain the support for right-wing populist parties as an expression of anti-immigrant sentiments, (often) aroused by an inflow of refugees or migrants.

2.2.1 Group position theory

One influential model is the *group position theory*, originally proposed by Blumer (1958) and well explained by Hjerm (2007). This theory was first developed to explain racial prejudice in an American setting. It relies on the idea that individuals are prone to identify themselves with a certain group of other individuals (sometimes called the in-group). This identity is partly formed in contrast to individuals that are not members of the in-group (sometimes called the out-group). According to the theory, individuals often attribute certain stereotypical characteristics to members of the out-group. Hostility towards the out-group arises when individuals perceive that the position and power of the in-group is threatened. The *group position theory* suggests that, under majority rule, inter-group tensions are expected to increase when the (perceived) size of the out-group rises (as this threatens the ability of the in-group to control the development of the community). In particular, the increase in tensions will be exacerbated if boundaries between the groups are rigid, as this makes it difficult for individuals to migrate between groups in a later stage.

In a Swedish setting, natives' increased support for right-wing populists can be interpreted as an expression of anti-immigrant sentiments. The *group position theory* would suggest that natives identify with an in-group consisting of native Swedes and consider immigrants part of the out-group. Natives' increased anti-immigrant sentiments would then be a reaction to a perceived threat to their social position.

The prediction of the *group position theory* is thus that an increased arrival of immigrants to the community in itself should increase support for right-wing populists. However, as immigrants integrate into local communities, get jobs and learn the language, the boundaries between in- and out-groups cannot be considered absolutely rigid. Instead, the ability of individuals to migrate between the groups is likely to depend on the perceived size of cultural and ethnic differences between the groups. Refugees coming to Sweden are more likely than the average immigrant to come from countries with large differences to Sweden when it comes to culture and ethnicity. Previous experiences and human capital formation are also likely to differ more between native Swedes and refugees than between native Swedes and the average immigrant. Therefore, according to the *group position theory*, it is likely that the effect of refugee inflow on right-wing populist support would be greater than the effect of immigrant inflow.

2.2.2 Ethnic competition theory

Another theory in the same family is the *ethnic competition theory* which was originally proposed by Pettigrew (1957) and is explained by Rydgren and Tyrberg (2016). It is also based on the idea that natives are identifying themselves with an in-group in contrast to an immigrant out-group. However, according to the *ethnic competition theory*, it is not immigration per se or the struggle over power and social positions in society that causes anti-immigrant sentiments to rise. Instead, the theory suggests that as individuals compete over scarce resources in the economy, individuals will only become hostile towards the out-group if they perceive that the presence of out-group individuals threatens in-group access to key resources. In other words, it suggests that native voters turn to extreme right-wing parties if they perceive that their access to resources such as jobs, housing and social welfare is threatened by immigrants or refugees. The theory's empirical prediction is that we would expect to see a stronger effect of refugee inflow on right-wing populist support if jobs and housing are scarce and/or social welfare payments are high. The *ethnic competition theory* is closely related to the idea of *welfare chauvinism*, that I discuss in Section 2.1 above. Although subtle, the difference between the two lies in the attitude towards migrants and refugees. According to *ethnic competition theory*, natives start resenting migrants or refugees because of the competition over resources. *Welfare chauvinism*, on the other hand, does not require natives to resent migrants. Instead, opposing migration is simply a rational economic decision. Empirically separating these two effects, however, lies far beyond the scope of this thesis.

2.2.3 The contact hypothesis

A contradictory theory to group position theory and ethnic competition theory is the *contact hypothesis* first proposed by Williams (1947), developed by Allport (1954) and well explained by McLaren (2003). The idea in Williams (1947) was that if members of a perceived in-group interacted with members of the out-group, they would update their stereotypes about out-group individuals with the new information they had acquired. Thereby, the rigid boundaries between in-group and out-group would dissolve and, thus, the groups would merge. Allport (1954), again with the segregated America of the 1950's in mind, suggested that it is not sufficient for individuals of the two groups to inhabit the same community. Instead, certain other criteria are necessary. For interaction to decrease stereotyping, Allport (1954) for instance claimed that it was necessary that the groups had equal social status and cooperated

towards common goals in order for them to have a meaningful interaction. Since then, a number of researchers have suggested different necessary conditions that are required for interaction to decrease prejudice (McLaren, 2003).

Even though there is no certain answer to what circumstances are optimal for reducing prejudice, it is reasonable to assume that a necessary condition for meaningful interaction between natives and refugees is that the groups both actually live in the same local community. It is unclear to what extent workplaces and residential areas in Sweden are segregated. However, Sweden is a well-developed European welfare state with publicly provided education, childcare, health care and elderly care. As both refugees and natives are equally qualified for these welfare services, such institutions might provide arenas for meaningful interaction. One might therefore believe that the circumstances for contact between immigrants and natives might be more beneficial than in states without a similar system. In this setting, the *contact hypothesis* would suggest that immigration to a local community should increase the probability of meaningful interaction, thereby decreasing anti-immigrant sentiments. If the *contact hypothesis* holds, we would therefore expect an increased refugee inflow to a local community to cause lower levels of support for right-wing populists.

2.2.4 The halo effect hypothesis

A recent addition to these theories is the *halo effect hypothesis* (proposed by Bowyer, 2008) that combines ethnic competition theory with the contact hypothesis. It states that anti-immigrant sentiments are expected to increase among natives in areas neighbouring communities with extensive immigration, but remain constant or decrease in the areas subject to immigration. The idea is that natives' propensity of meaningful interaction with refugees rises in the areas where refugees actually settle, but remain constant in neighbouring areas. Instead, natives inhabiting those areas will only be affected by the effects of ethnic competition and/or group position threat. In a Swedish setting, this would mean that the effect of refugee inflow on Sweden Democrat support is expected to be negligible in municipalities where refugees actually are allocated, but positive in neighbouring municipalities. Even though it would theoretically be possible to test the *halo effect hypothesis* using the data I have at hand, this unfortunately is beyond the scope of this paper.

3 Literature Review

There have been a number of papers trying to empirically identify the reasons behind the increased support for right-wing populists. In this chapter, I give an overview of recent and important papers related to my study as well as elaborate on my contribution to the literature.

3.1 Studies of socioeconomic factors and right-wing populism

One strand of literature tries to empirically test the hypotheses discussed in Section 2.1. These papers investigate whether social and economic circumstances can explain the support for right-wing populists. A large part of this literature study the relationship between unemployment and extreme right-wing support (thereby testing the *social marginalization hypothesis*). An early example is Jackman and Volpert (1996) who find a positive and significant relationship in a cross-country comparison. Their findings have been corroborated, for instance in a study using cross-country European survey data (Arzheimer, 2009). However, other authors such as Coffé, Heyndels, and Vermeir (2007) and Knigge (1998) are not able to replicate these results. Instead, they find ambiguous or negative effects of unemployment on right-wing populist support.

A recent and important study is Dehdari (2018) who uses the sectoral composition of local labor markets in combination with national sectoral trends in layoffs as an instrument to estimate the effect of layoff notices on the support for the Sweden democrats. He finds that there is a strong and significant increase in the support for SD caused by an increase in layoff notices. In particular, he is able to identify an even stronger effect in areas with a high level of low-skilled immigrants, thereby lending some support to the theory of welfare chauvinism.

There have also been a few papers relating the support for right-wing populists to voters that suffer from increased inequality and economic insecurity. Autor et al. (2016) find that increased trade competition results in US voters electing more extreme candidates both on the right and left wing. The mechanism would be that voters choose candidates with protectionist attitudes in order to decrease competition (discussed above in Section 2.1). Malgouyres (2017) are able to replicate these results in a French setting. Bó et al. (2018) find that support for the Sweden Democrats is strongest among citizens suffering from increased economic insecurity as well as among those losing from rising inequality in Sweden.

Most of the papers I discuss above suggest that right-wing populist voters are selected from the lower part of the income distribution. However, in an influential paper Facchini and Mayda (2009) find that high-income individuals are negative to the inflow of low-skilled migrants but supportive of the inflow of high-skilled migrants (lending support to their theory of high-income natives trying to avoid welfare expenditures).

Although an empirical consensus about the effects of socioeconomic factors on right-wing populist support has yet to be established, most studies (including recent research exploiting natural experiments) suggest that there is a positive effect

of increased economic insecurity on the support for extreme right-wing parties. The mechanisms behind this effect are yet to be determined. In addition, little empirical research has studied other social factors and their impact on right-wing populist support.

3.2 Studies of immigration and right-wing populism

Even though the socio-economic explanatory models have been popular in academia, there is also a vast literature that studies the connection between right-wing populist support and immigration itself. Indeed, Card, Dustmann, and Preston (2012) exploit data from the European Social Survey and conclude that the increase in right-wing populist support to a large extent seems to be driven by non-economic factors. The studies in this strand of literature have primarily been conducted by sociologists and political scientists, but in recent years political economists have shown an increasing interest in the topic. There has been a number of papers employing quasi-experimental methods coming out in recent years. These studies often try to test the hypotheses discussed in Section 2.2.

One of the most notable of these papers is Steinmayr (2016). Steinmayr exploits variation in housing availability in Austria as an instrument to estimate the effect of immigration on the support for a right-wing populist party. During 2015, a large number of refugees passed through Austria (often by foot) on their way north towards Germany and Sweden. Some refugees also applied for asylum in Austria and were assigned to communities with available housing. Using availability of housing as an instrument, Steinmayr concludes that anti-immigrant sentiments increased in border communities immigrants passed, but that they decreased in communities that actually received and hosted immigrants. Thus, his paper lends support to the contact hypothesis. Support for the contact hypothesis is also provided by a study using data from the Eurobarometer survey (McLaren, 2003) as well as by a meta study of sociological papers (Pettigrew and Tropp, 2006).

As I discuss above in Section 2.2, the halo effect hypothesis could be considered to constitute a combination of the contact hypothesis and ethnic competition theory. Bowyer (2008) both introduces this hypothesis and finds evidence for it in an empirical study of neighbourhoods in England. His results are reaffirmed by Biggs and Knauss (2012). Valdez (2014) finds further support for the halo effect hypothesis when using a Swedish program for the construction of high-rise apartment blocks as an instrument to find exogenous variation in immigration.

However, the vast majority of empirical evidence lends support to group position theory or the ethnic competition theory. Rydgren and Tyrberg (2016), Rink, Phalet, and Swyngedouw (2009), Schneider (2008) and Lubbers, Gijsberts, and Scheepers (2002) all find that the number of immigrants in the community is correlated with the support for extreme right-wing parties, suggesting that the arrival of migrants increases anti-immigrant sentiments. Gerdes and Wadensjö (2010) use a "selection on observables"-strategy to study how migration effects right-wing populist support in Denmark. They find a strong positive association between immigration and the vote share for right-wing populists.

Migration patterns are unlikely to be exogenous with regard to right-wing populist support (there could be reverse causality if migrants avoid places with strong anti-immigrant attitudes or omitted variable bias if there are unobserved covariates affecting both migration and the support for right-wing populists). It is therefore difficult to argue for a causal interpretation of studies solely based on multivariate

regressions. Instead, quasi-experimental methods provide a better fit if we are interested in identifying the causal effect of migration on extreme right-wing support. To the best of my knowledge, Otto and Steinhardt (2014) was the first paper to exploit a natural experiment in this strand of literature. They use a peak in the number of immigrants to city districts in the German city of Hamburg in the 1980's to estimate the effect on extreme right-wing parties using a "difference-in-differences"-strategy. They find a positive effect of immigration on right-wing populist support.

There have also been a number of papers using previous immigrant settlement patterns as instruments for later migration flows. The idea is that previous settlement is not related to current political preferences but that migrants choose to settle close to their fellow countrymen. Hence, previous settlement is argued to provide exogenous variation in immigration flows. Studies using this method has been conducted in Italy (Barone et al., 2016), Austria (Halla, Wagner, and Zweimüller, 2017) and France (Edo et al., 2019). All have found positive effects of immigration on the support for far right-wing candidates. However, Halla, Wagner, and Zweimüller (2017) find that the effect is accentuated among low-skilled natives (which lends support for the ethnic competition theory). Strömblad and Malmberg (2016) lend further support to ethnic competition when they find that right-wing populist support in Sweden only increases when immigration rises in areas suffering from high unemployment.

The instrumental variable strategy evidence for a positive effect of immigration on right-wing populism is not restricted to previous immigration patterns. Hangartner et al. (2019) use distance to Turkey as an instrument to estimate the effect of refugee flows in the Aegean Sea during the so called "European Refugee Crisis" of 2015. Using data collected through a specifically designed survey directed to the natives of a number of Greek islands, they are able to identify a strong positive effect on the support for right-wing extremists among those most heavily affected by the refugee flows. Harmon (2018) uses the existence of high-rises as an instrument to estimate the effect of immigration in Danish municipalities between 1981 and 2001. He is able to conclude that immigration has had a positive effect on the voting for far right candidates.

3.2.1 Studies with similar methodologies

In a study with an identification strategy that resembles mine, Dustmann, Vasiljeva, and Piil Damm (2018) exploit quasi-experimental variation in refugee flows stemming from a refugee dispersal program in Denmark. Using placement of refugees as an instrument for immigration, they find that refugee inflow has a strong positive effect on the vote shares for anti-immigration parties. Interestingly, however, they find that these effects are concentrated in small and rural municipalities (conditional on observables). They cannot observe any effects in large and urban municipalities. Barone et al. (2016) find similar results in their study of Italy.

Maybe most important for the methodology of my study is Dahlberg, Edmark, and Lundqvist (2012) who use a Swedish refugee dispersal program from the 1990's to estimate the impact of migration on the preferences for redistribution. The reform redistributed refugees over most of Sweden's municipalities following a spike in migration in the wake of the Balkan wars. Dahlberg, Edmark, and Lundqvist (2012) claim that the variation in immigration is exogenous, conditional on observables. Using an IV strategy on an individual-level panel survey data set collected by the Swedish National Election Studies Program, they are then able to identify

a negative effect of migration on preferences for redistribution (which they interpret causally). Dahlberg, Edmark, and Lundqvist (2012) have received criticism by Nekby and Pettersson-Lidbom (2017). I believe that my empirical strategy avoids the pitfalls pointed out in their comment¹. Thus, I believe that my study might contribute to the insights from Dahlberg, Edmark, and Lundqvist (2012) by providing an arguably even cleaner identification. In addition, I study the preferences for right-wing populists rather than the support for redistribution, thus further adding to the understanding of natives' reactions to refugee migration.

Inspired by Dahlberg, Edmark, and Lundqvist (2012), Mehic (2019) evaluates the same reform as I do. We are, to the best of my knowledge, the first to evaluate the effect of migrant dispersal reforms on the support for right-wing populists in Sweden. Mehic (2019) is using an identification strategy which is very similar to mine (an IV strategy assuming exogenous variation in refugee inflow conditional on a set of covariates) to estimate the effect of immigration on Sweden Democrat election results in the Swedish parliamentary election of 2018. Mehic (2019) finds a strong and significant positive effect of refugee immigration on the support for the Sweden Democrats in the election of 2018. The effect is weaker if the refugees are minors and exacerbated in areas who strongly supported the extreme right-wing party New Democracy in the 1990's.

Both Mehic (2019) and I use a similar IV method as Dahlberg, Edmark, and Lundqvist (2012) and Dustmann, Vasiljeva, and Piil Damm (2018) to answer the intriguing question of how refugee immigration affect the support for right-wing populists in a Swedish setting. The reform we exploit is arguably even more suitable for the purpose than the one used by Dahlberg, Edmark, and Lundqvist (2012). However, as discussed in Chapter 1, I add to the insights provided by Mehic (2019) by testing the robustness of the estimates for the inclusion of additional institutional knowledge. As I use individual-level survey data in my outcome variable, I am also able to study detailed heterogeneous effects among groups that can be expected to drive my results.

¹Nekby and Pettersson-Lidbom (2017) criticize the identification strategy proposed by Dahlberg, Edmark, and Lundqvist (2012) in three different ways. First, Nekby and Pettersson-Lidbom (2017) question the choice of what they deem is an arbitrary measurement of preferences. In my paper, I have chosen to present all available measures of SD preferences (a more detailed explanation of the measures can be found in Section 5.3). Secondly, they criticize the use of panel-structure survey data (with possible endogenous sample selection). As I use repeated cross-sectional data and cluster my standard errors on municipal level this should not be an issue for me. Lastly, Nekby and Pettersson-Lidbom (2017) argue that the refugee placement program was measured with systematic error because Dahlberg, Edmark, and Lundqvist (2012) measured it by the refugees the municipalities were remunerated for (instead of measuring the refugees supposed to be placed in the municipality according to the contract). As municipalities could refuse to follow the contracts, this left room for endogeneity. In this study, I use the planned allocation of refugees as my instrument.

4 Institutional background

In this chapter, I provide some background information about migration to Sweden and historical institutions in the Swedish migration system (in the time before the reform). I then describe the refugee redistribution program from 2016 in detail. Lastly, I provide some brief information about right-wing populism in Sweden and the parties that have become the main vehicles of these opinions.

4.1 Migration to Sweden

4.1.1 The inflow of migrants to Sweden.

Sweden was for a long time mainly a country of emigrants, with a considerable net outflow of migrants during the 19th century, primarily to the USA. After the Second World War, Sweden became a country with a net inflow of migrants. After accommodating a large number of German, Nordic and Baltic refugees in the wake of the war, Sweden started taking on a considerable number of labor migrants (i. a. from Finland, Italy, Greece, Yugoslavia and Turkey). During the 1970's, labor migration decreased gradually due to stricter regulation. In the 1980's, the primary source of immigration to Sweden instead became non-Nordic asylum seekers (Migrationsverket, 2019b).

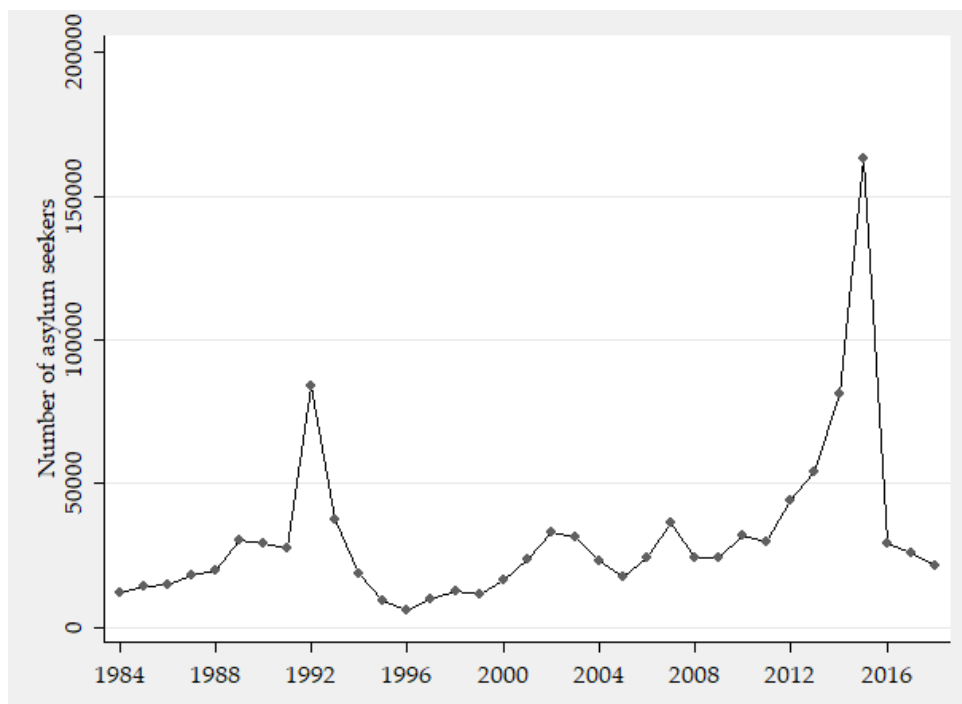


FIGURE 4.1: Inflow of asylum seekers to Sweden 1984-2018

Figure 4.1 shows the inflow of asylum seekers to Sweden during the period 1984-2018. There has been a slow but steady increase in the number of asylum seekers over the entire period, with some notable spikes. The average number of asylum seekers per year in Sweden in the period 1984-2017 was 31 500.

During the 1980's, most of the asylum seekers came from countries in the Middle East and on the horn of Africa such as Iran, Iraq and Eritrea. In the early 1990's, the Yugoslav wars released a wave of refugees over Europe. A considerable share of these refugees, primarily Bosnians came to Sweden. Thus, the inflow of asylum seekers in Sweden rose sharply, peaking just above 84 000 in 1992. During the late 1990's the numbers fell back to the levels from before the war. It gradually rose again during the 2000's reaching approximately 30 000 in 2011. After the onset of the war in Syria, the number of asylum seekers again rose sharply and during the so called "European Migration Crisis" in 2015 the number of asylum seekers reached almost 163 000. To illustrate how exceptional this inflow was, one could observe in Figure 4.1 that there have been only four years since 1984 during which the number of asylum seekers have surpassed 50 000. As a reaction to the high number of asylum seekers, the Swedish parliament decided to change the Swedish refugee migration regulation. Sweden went from having the most generous refugee migration regulation in Europe to instead adapt to the minimum level required for a member state of the European Union. As a consequence, the number of asylum seekers fell drastically again during 2016-2018. However, as the time for the application process for a residence permit increased drastically during the same time, many of the asylum seekers that came to Sweden in 2015 did not obtain their residence permits until during 2016 or 2017 (Mottagandeföreskrifterna, 2018). Of the asylum seekers that came during the migration wave of 2015, 78% came from Syria, Afghanistan, Iraq, Eritrea or Somalia. In fact, 47 % of the asylum seekers coming to Sweden between 2000 and 2017 came from Syria, Iraq, Afghanistan and Somalia (Mehic, 2019).

4.1.2 The structure of the Swedish migration system.

Today, immigration to Sweden is regulated by the Swedish Migration Agency (SMA). However, much of the practical work is conducted by the 290 municipalities (which are spread over the country's 21 counties). In each county, there is a county administrative board (*länsstyrelse*). The county administrative boards act as the local representatives of the central government and cooperate closely with the Swedish Migration Agency with implementing national immigration policies locally.

Immigrants to Sweden today can be categorized into three broad groups. The first is migrants from inside the Schengen area of the European Union. The second consists of non-EU citizens who are not applying for asylum (i.a. students and guest workers). The third consists of migrants who apply for asylum in order to obtain refugee status (Migrationsverket, 2018). This third group is at the center of public debate and is the focus of the reform in this study.

Refugee immigrants can in turn be subdivided into three broad categories: asylum seekers, quota refugees and relatives of previous refugees. Asylum seekers and quota refugees are most directly relevant for my thesis as they are most affected by the reform². Asylum seekers are individuals who come to Sweden and apply for asylum. During their residence permit application process, the asylum seekers can

²Relatives of previous refugees are directly assigned to the same municipality in which their relative resides (except in the case when their relative has been granted a residence permit but has not yet been assigned to a municipality. In this case, the relatives are also affected by the reform).

either organize accommodation on their own or stay in housing provided by the Migration Agency. When their application has been processed, those who are granted asylum are presented with a choice. Again they can either find their own housing, or they can ask the Migration Agency for help. In the latter case, the Migration Agency will assign them to a certain municipality.

Quota refugees, on the other hand, are individuals selected by the UNHCR among the most needing in refugee camps around the world. They are directly assigned to a municipality before coming to Sweden.

Regardless of category, the municipality has the responsibility to provide the refugee with housing and social services after assignment. However, the municipalities are remunerated by the central government (through the SMA) for taking on refugees (Perols, 2018).

4.2 The refugee dispersal program

Until March 1st 2016, the assignment of refugees to municipalities was made through voluntary contracts between municipalities and the central government (Arbetsmarknadsdepartementet, 2015). However, in the wake of the 2015 wave of asylum seekers, a new law was introduced that required municipalities to accommodate refugees assigned to them by the county administrative boards. The number of refugees per county was in turn decided by the central government. The distribution was proposed by the Migration Agency. The law asks the Migration Agency to consider local labor market conditions, population, previous reception of refugees, available housing and the number of asylum seekers in the municipality³ when deciding upon the distribution (SFS 2016:38, SFS 2016:39). There were 8 municipalities that were forced to start taking on refugees as a consequence of the reform (all other municipalities had voluntary agreements before the reform)⁴.

Through discussions with officials at the SMA, the Swedish Public Employment Service and the county administrative boards, I have been able to map out the process of refugee allocation after the reform. In July the year before the refugee allocation takes place, the Migration Agency presents a prognosis over the number of refugees of different categories that are expected to be granted a residence permit during the coming year. Using retrospective data on the shares of the different categories that have requested assistance with finding housing during previous years, the Migration Agency also computes a prognosis over how many refugees they will need to assign to a municipality. For instance, quota refugees will always be directly assigned to a municipality. Every refugee in this category is therefore treated by the reform. On the other hand, refugees that already during the asylum process found housing of their own are very unlikely to request the Migration Agency to assist them with housing after they obtain a residence permit.

The Migration Agency then proposes a tentative distribution of these refugees over counties and municipalities using an allocation mechanism including six different variables. Three of the variables are connected to labor market conditions⁵, whereas the remaining variables are related to the share of refugees already present

³It is unclear if the law refers to current or historical asylum seekers.

⁴The forced municipalities were Burlöv, Helsingborg, Höganäs, Skurup, Staffanstorps, Vellinge, Trollhättan and Hallsberg.

⁵The number of professions in the municipality, local unemployment and the share of unemployed born outside of Europe that became employed during the previous year, respectively.

in the municipality⁶. The government decides on the distribution of refugees over counties and assigns the task to decide the distribution over municipalities to the county administrative boards. The county administrative boards in turn sets the distribution of refugees (so called *kommuntal*) in dialogue with the municipalities themselves. The starting point of the discussion is the distribution of refugees proposed by the Migration Agency, but the distribution within the county can be subject to change. The only allowed reason for changing the distribution over municipalities is the availability of housing. During the studied period 8 – 10 of the 290 municipalities in Sweden had their distribution changed from the one proposed by the Migration Agency each year⁷.

The number of refugees assigned to each municipality is decided in late autumn. Throughout the following year, the municipal quotas are implemented by the Migration Agency. When refugees with a residence permit request assistance with housing the SMA assigns them to a municipality. The primary allocation criterion of individual refugees is the extent to which a municipality has fulfilled its quota. However, in a second stage a large number of secondary criteria are used to match the refugee to a municipality (such as housing availability, need for specialized healthcare and school availability).

As refugees residing in Sweden self-select into the program (and quota refugees from the start are a selected group of vulnerable refugees), the group of refugees affected by the redistribution scheme are likely to be systematically different from those who find housing on their own. One might for instance expect that refugees who have better labor market opportunities and better social networks in Sweden are less likely to be in the program. Thus, if I use the redistribution program as an instrument, I might not estimate the effect from the inflow of average refugees on Sweden Democrat support. Instead, I will be estimating the effect of a selected group of refugees (that are likely to have more difficulties to integrate into the labor and housing markets) on Sweden Democrat preferences. This is not necessarily a problem for my study, as the type of programs I try to evaluate are designed to redistribute the burden of accommodating refugees in need of support between municipalities. However, when reading this study it is crucial to remember that my estimates are likely to be upwards biased if we want to interpret the results as the causal effects of refugee immigration in general on the support for right-wing populists.

4.3 Right-wing populism in Sweden

The Sweden Democrats is the most successful right-wing populist party in Sweden. The party was formed in 1988 with the purpose of working against immigration to Sweden. Several of the founders came from extreme right-wing parties as well as from neo-nazi movements in Sweden. After the national election in 1994, the previously dominant right-wing populist party New Democracy was dissolved, thus leaving room for a new right-wing populist party. As the party grew, many members and local representatives of the party were excluded in an attempt to liberate

⁶The population of the municipality, the number of refugees expected to choose to settle in the municipality in the coming year (based on previous migration patterns) and the total number of days asylum seekers stayed in housing provided by the SMA in the municipality during the previous year (*boendedygn*).

⁷The changes were generally small as reducing the quota for one municipality requires that another municipality in the same county accepts to raise their quota with the same amount.

the party from accusations of being racist. Today, the party describes itself as "social-conservative and nationalist". Important issues for the Sweden Democrats are to decrease immigration and to fight "multiculturalism". The party has also become known for supporting a strict criminal justice policy and for having a skeptic attitude towards the European Union.

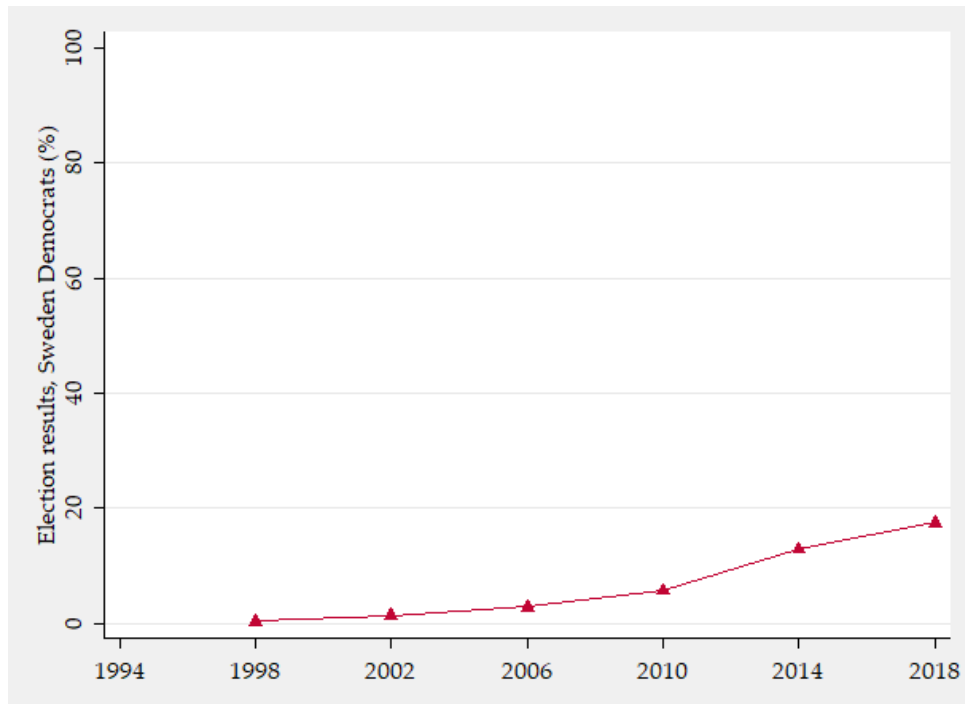


FIGURE 4.2: SD election results

As can be seen in Figure 4.2, the support for the Sweden Democrats has risen from a marginal support in the election of 1998 (the first year Statistics Sweden measured their national election results) to obtaining over 17.5 percent of the votes in the national elections of 2018. The support for the party is strongest in the southern counties of Skåne and Blekinge. The party has a weaker support in the large cities of Sweden (particularly in the capital Stockholm) as well as in the rural parts of northern Sweden (a traditional stronghold of the left-wing Social Democrats and the Left Party).

The party became the third largest Swedish party in the national elections of 2018. Despite this, the party has remained relatively isolated in the Swedish parliament. The reason for this is that the other parties of the parliament have tried to avoid cooperating with the Sweden Democrats, citing their strong preferences for a very restrictive refugee migration policy as the main reason for this (Nationalencyklopedin, 2019).

5 Data

In this chapter, I discuss the data sets I employ. I use data from the Swedish Migration Agency on the total refugee inflow to Swedish municipalities, as well as data on the refugees allocated to municipalities through the redistribution program. The Swedish Migration Agency (together with the Swedish Public Employment Service) also provided me with the computational model used to allocate refugees. In addition to the algorithm, it also contains a number of municipal characteristics that were fed into the computational model. Additional municipal level data are provided by Statistics Sweden and the Swedish Association of Local Authorities and Regions (SKL). Lastly, I employ survey data from the National SOM Surveys of Sweden. This data set is collected and managed by the SOM Institute at Gothenburg University.

5.1 Data on refugee immigration

As municipalities in Sweden get remunerated by the central government for taking on refugees, panel data on registered refugees on municipality level are available from the SMA (Migrationsverket, 2019a). This data set contains both the refugees that are allocated through the redistribution program and the refugees that move to the municipality on their own initiative. Ideally, I would have liked to have data on all refugees moving to Swedish municipalities. However, as municipalities have a strong economic incentive to register all refugees, my data is likely to be almost complete (the marginal group of refugees who are completely without any need of municipal support straight after obtaining their residence permits may be missing).

I have access to data on the total number of refugees with a residence permit that the municipality has been remunerated for by the Migration Agency since 2011. Because of the nature of the reform and other data available, I focus on the period 2015-2017. During this time span, the total inflow of refugees (both those allocated and those that moved voluntarily to the municipality) were 188 709 individuals. As seen in Table 5.1, the inflow varied substantially between municipalities. The smallest inflow to any municipality in a given year was one refugee and the largest inflow was 5401 refugees (the average was 868 refugees/municipality).

The data set also includes the planned allocation of refugees over municipalities (according to the original SMA proposal to the Swedish government) throughout the period as well as the number of refugees from that quota that have actually been placed in the municipality. This data set is not entirely consistent. As the reform was not implemented until 2016, municipalities were not forced to take on refugees in 2015. In order to have a pre-treatment year in my data set, I have added the year 2015 anyway, but I replaced the number of planned allocated refugees for that year with the number of refugees that were supposed to be taken on by municipalities through voluntary contracts with the Swedish Migration Agency (discussed in Section 4.2). I run all my main specifications both with and without the year 2015 in order to make sure that this does not affect my estimates.

In total, the Swedish Migration Agency suggested that municipalities were to take on 55 877 refugees throughout the period. In reality, 50196 individuals with a

residence permit were actually assigned to a municipality. The bulk of the difference between the number of refugees supposed to be allocated and those actually allocated comes from the year 2015, when municipalities were contracted to take on 10575 refugees, but only actually received 6648 (a difference of 3927 individuals). The remaining discrepancy of 1754 individuals stem from the first three months of 2016⁸. The register of allocated individuals was transferred from the Swedish Public Employment Service to the Swedish Migration Agency as part of the reform. In interviews with officials from the two authorities, I have understood that the transfer was not entirely smooth. Errors in the data could have occurred due to different bureaucratic traditions, miscommunication or lacking IT capacity.

TABLE 5.1: Summary statistics: Refugee migration 2015-2017

Variable	Mean	Std. Dev.	Min.	Max.
Total number of refugees to the municipality	868.363	1245.287	1	5401
Planned allocated refugees	327.993	683.525	0	2858
Refugee inflow rate	0.647	0.395	0.008	4.098
Planned allocated refugee inflow rate	0.197	0.136	0	1.59

The rates are expressed in percentage points of the local population 2015.

The literature provides no clear answer to whether it is better to use the planned or the actual allocation of refugees through the dispersal program as an instrument for refugee migration to a municipality. As pointed out by Nekby and Pettersson-Lidbom (2017) in a comment on Dahlberg, Edmark, and Lundqvist (2012), the planned allocation of refugees is arguably more exogenous to the political preferences of the municipality as municipalities are involved in a dialogue with the county administrative boards about the detailed distribution. The theoretical argument that planned allocation is likely to be more exogenous than actual allocation is confirmed by the placebo tests I conduct in Section 7.4 (the planned allocation hold up better to the placebo tests than the actual distribution).

However, it is the number of refugees actually placed in the municipality that I believe might affect the support for the Sweden Democrats. Moreover, as discussed in Chapter 4, the opportunities of municipalities to affect how many refugees they received through the program were limited, thus reducing the risk for endogeneity in actual refugee allocation (particularly when only including the post-reform period of 2016-2017).

I still choose to use the planned number of refugees received as my instrument (in my sensitivity analysis, I also employ the actual allocation as my instrument).

⁸Although the number of planned and actually allocated refugees differ very little when only looking at the post-reform period, there is a considerable lag in the allocation of refugees. For instance, a substantial number of refugees allocated according to the quota for 2016 were not actually placed in a municipality until several months into 2017. Through interviews with officials from the Swedish Migration Agency, I have understood that this depends on several factors. Partly, the lag is due to delays in the placement of refugees. Mainly, however, there is lag because of the time it takes between the arrival of a refugee and the time he/she enters the registers of the Migration Agency (in particular as the data is based on payments made to the municipality). This could take up to three months. Thus, refugees placed in late December may not be accounted for until March the year after.

As the SOM surveys (from which I collect my outcome variable data) are conducted throughout the fall each year, using data on refugee migration for the entire year might prove problematic. This is because a substantial part of the refugees may arrive to the municipality after the survey has been collected. In my sensitivity analysis, I therefore choose to use the number of refugees recorded to be placed in municipalities at the end of the year as my preferred measure of actually dispersed refugees. Considering the lag in the registration of refugees, I should then only include refugees who were placed before mid-fall (before the survey was conducted).

Thus, I will use the intention to treat municipalities as my instrumental variable. Henceforth, when I write allocated refugees, I mean those *planned* to be allocated. As seen in Table 5.1, the average number of refugees planned to be assigned to a municipality at the end of the year was 328 (with a minimum of 0 and a maximum of 2858).

Both relative inflow and the absolute number of refugees might affect local opinion. Following Dahlberg, Edmark, and Lundqvist (2012) and Mehic (2019), I choose to define my main inflow variable as refugees as a share of the local population. This definition makes it easy to interpret the effects and follows the logic of the *group position theory*. I thus define the total refugee inflow rate (RIR) in municipality m in year t as in Equation 5.1 and the allocated refugee inflow rate (ARIR) as in Equation 5.2⁹.

$$\text{RIR}_{mt} = \frac{\text{Refugee inflow}_{mt}}{\text{Population}_{m2015}} \quad (5.1)$$

$$\text{ARIR}_{mt} = \frac{\text{Allocated refugee inflow}_{mt}}{\text{Population}_{m2015}} \quad (5.2)$$

Although I use RIR and ARIR in my main specifications, I also test to use the absolute number of refugees as the instrument.

In Figure 5.1, I illustrate the distribution of the total inflow of refugees as a share of the local population. The refugee inflow rate varies from almost nothing to upwards of 4 % of the population, with most municipalities receiving just over 0.5 % (the average inflow rate is approximately 0.4 % as seen in Table 5.1). The refugee inflow rate is particularly high in rural municipalities in southern and central Sweden such as Högsby, Hylte, Lessebo and Avesta. The smallest share of refugees in relation to the population come to municipalities in the Stockholm and Gothenburg archipelagos such as Öckerö and Vaxholm, as well as to wealthy suburban municipalities such as Vellinge and Vallentuna.

Likewise, in Figure 5.2, I illustrate the distribution of allocated refugees as a share of the local population. This distribution is more skewed to left. In particular, there are many municipalities that get almost no refugees allocated to them. As seen in Table 5.1, the average municipality get an inflow of allocated refugees equivalent to roughly 0.2 % of the population, whereas a few of the municipalities get well over 1.5 %. The highest allocated refugee inflow rates are found in rural municipalities, predominantly in northern Sweden (such as Sorsele, Dorotea, Aneby, Uppvidinge and Malå). Rural municipalities in southern and central Sweden such as Arboga, Lessebo, Munkfors and Perstorp have the smallest amounts of planned allocated refugees.

5.2 Data on municipal characteristics

As thoroughly discussed in Chapter 4, refugees are distributed over municipalities according to a number of municipal characteristics. I have been able to access the distribution scheme for 2016-2019, including the panel data on municipal characteristics used by the Swedish Migration Agency. The data contains variables from the Swedish Public Employment Service on total unemployment, number of occupations and the turnover rate from unemployment to employment for migrants in the municipality (these variables are used to characterize how beneficial the local labor

⁹I use the population in the beginning of the measurement period (2015) to make sure that the population in itself does not change with the refugee inflow.

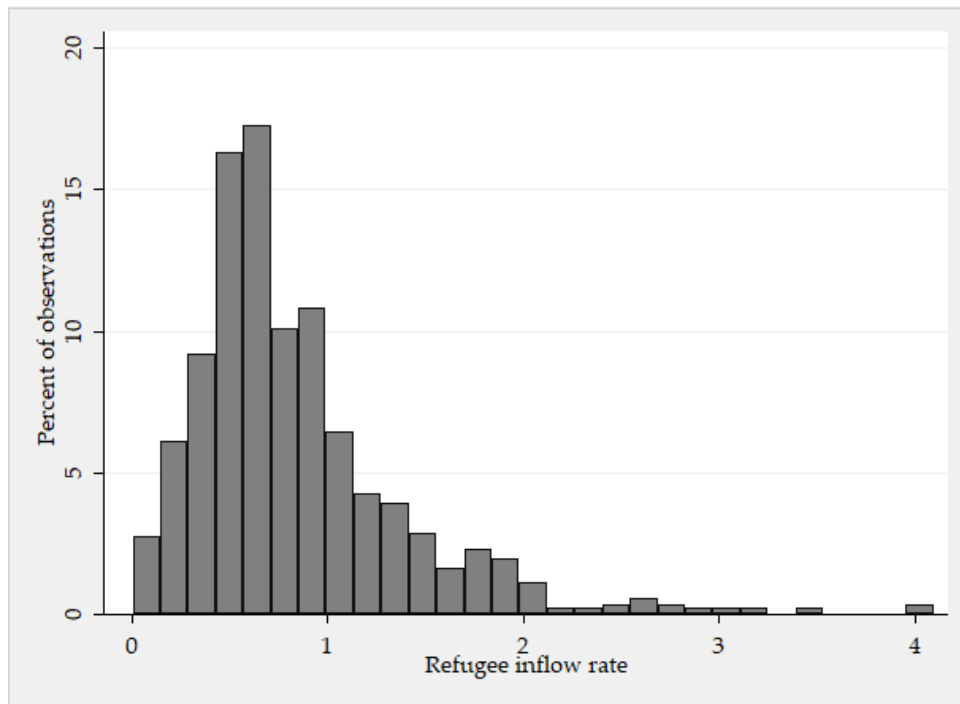


FIGURE 5.1: Histogram: Total refugee inflow rate

market can be expected to be for refugees). It also contains data from the Swedish Migration Agency in itself on the number of nights asylum seekers stayed in housing provided by the agency in the municipality.

In addition, the data file also contains the number of refugees prognosed to move voluntarily to the municipality in the coming year¹⁰. Adding this variable to my regressions introduces a lot of multicollinearity, because it is likely to covary closely with the other control variables of the reallocation algorithm (this can be verified in the covariance matrix in Figure A.2 in Appendix A). Introducing a highly multicollinear control may increase my standard errors and make my estimates sensitive to small changes in the data. I would therefore like to replace the prognosed inflow with some proxy variable. I believe that the most credible way the prognosed inflow could be expected to correlate with the error term of my regression is through the number of immigrants living in the municipality before. The prognosed inflow is based on the number of refugees who chose to settle in the municipality previous (which in turn is likely to covary with SD support).

In order to avoid the possible bias introduced by excluding this control, I instead proxy the variable with the number of refugees that came to the municipality in the period 2005-2014. As seen in Figure A.2, there is a strong correlation between the total refugee inflow 2005-2014 and some of my covariates (such as population) as well. Despite this, I still prefer this proxy because the correlation between the total refugee inflow and most of my other covariates is lower than the correlation between the prognosed refugee inflow and those same covariates. In fact, many of the covariates I include are correlated (which poses a problem to the precision of my estimates). However, none of the other covariates are as strongly correlated with all other covariates as the prognosed number of voluntarily moving refugees. Thus, I believe that the other variables will not cause as serious problems with multicollinearity.

¹⁰Based on previous migration patterns.

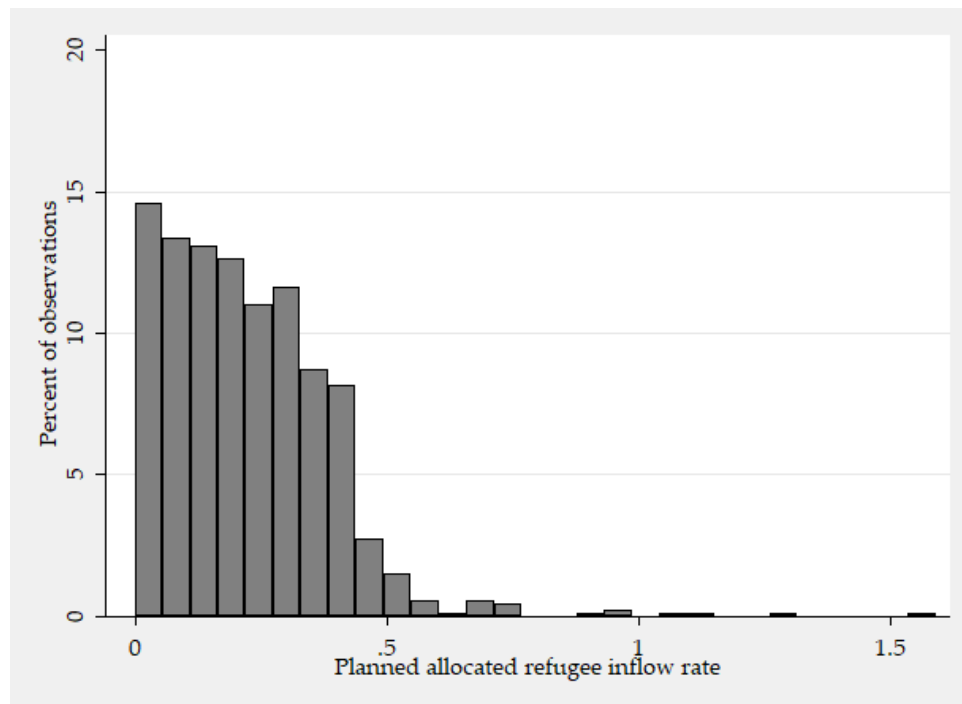


FIGURE 5.2: Histogram: Allocated refugee inflow rate

As discussed in Chapter 4, the availability of housing is also considered when allocating the refugees. Following Dahlberg, Edmark, and Lundqvist (2012), I proxy availability of housing with vacant apartments in housing owned by the municipality (this data is provided by Statistics Sweden). This is reasonable as municipalities when assigned with the task to find housing for refugees are likely to start by searching for available apartments in the public housing stock¹¹. In addition, available public housing should serve as a reasonably good measure for how easy it is for natives to find housing (which I suspect might affect the propensity to support the Sweden Democrats).

As mentioned in Section 5.1, I add the year 2015 to the analysis to have a pre-treatment year in the panel. As there was no computational model in 2015, no data is available from the Swedish Migration Agency on labor market and migration characteristics for that year. I therefore proxy the labor market variables from the Swedish Public Employment Service with municipal unemployment in the ages 16-64 from SKL, the Swedish Association of Local Authorities and Regions (SKL, 2019). The number of nights in Swedish Migration Agency housing is proxied by the number of asylum seekers in the municipality on the last day of the year¹². When I estimate my regressions including 2015, this means that I include a limited set of covariates. Furthermore, the SKL proxies are only available for 285 of the 290 municipalities in Sweden which limits my sample. Detailed descriptive statistics over

¹¹It is also likely that available public housing is used as a measure in the negotiations with the country administrative boards.

¹²The proxy would arguably have been better if I would have been able to multiply the number of asylum seekers with the median length of stay for an asylum seeker in unicity. However, only the average number of days is available. As the average number of days is much higher than the median (due to a small number of asylum seekers staying in Swedish Migration Agency housing for a very long time), I fear that using this number would make my proxy worse, rather than improving it. Therefore, I believe that the number of asylum seekers is the best available proxy.

TABLE 5.2: Summary statistics: Municipal characteristics 2015-2017

Variable	Mean	Std. Dev.	Min.	Max.	Available years
Population	1759602	270000	2451	949761	2015-2017
Native-born population	135821.347	202735.575	2171	715058	2015-2017
Native-born population share	0.838	0.07	0.585	0.985	2015-2017
Total refugee inflow 2005-2014	4347.047	6143.865	5	22138	2015-2017
Total mun. unemp. (%)	7.724	2.834	2.319	15.184	2016-2017
Mun. unemp. 16-64 years (%)	8.4	3.219	2.2	18.6	2015-2017
Turnover rate to emp. for migrants	0.257	0.115	0	1.519	2016-2017
Number of occupations	70.709	37.388	1	142	2016-2017
Days in SMA housing/1000 inhab.	4397	4998	98	59693	2016-2017
Asylum seekers/1000 inv	17.758	19.472	1	231.4	2015-2017
Available municipal apartments	21.522	38.266	0	151	2015-2017
Election result SD 2018 (%)	17.6	5.522	8.6	39.2	2015-2017
Election result SD 2014 (%)	12.848	4.529	5.3	30	2015-2017

the municipal characteristics can be found in Table 5.2.

5.3 Data from the SOM surveys

The last data set that I use I get from the SOM Institute at Gothenburg University (SOM, 2019). The National SOM Surveys have been collected each year since 1986. The latest available survey data is from 2017. The surveys are sent out by post to a nationally representative sample of approximately 20 000 Swedes (randomly sampled from the population register of the Swedish Tax Agency). The respondents are also given the opportunity to answer the survey online. If the respondent does not answer the survey within the first weeks, they are reminded through phone calls and text messages in order to increase the response rate. In 2017, the SOM Institute introduced a minor monetary reward for answering the survey (they had previously conducted a survey experiment to confirm that introducing a reward did not affect the quality of the survey answers). In the years 2015-2017 the response rate varied between 51% and 55%. The SOM Institute concludes that an analysis of the response rates shows that the surveys are representative of the population in most dimensions. However, women and older persons are generally over-represented among the respondents (Andersson et al., 2018; Andersson et al., 2017; Ohlsson et al., 2016). This means that if the effect of refugee migration on right-wing populist support is differs between young and old as well as between men and women, this might bias my estimates due to sample selection.

My SOM data set thus includes individual reiterated cross-sectional survey data for approximately 10 000 Swedes over the age of 15 collected in the fall each year between 1986 to 2017. For my panel for 2015-2017 this means that I have a total of 28903 observations. When I use only 2016-2017, I instead have 20640 observations to work with. The data set includes a large number of variables such as party of preference, income and education. Summary statistics over the variables that I use in my estimations are found in Table A.1 in Appendix A.

The most important variables I use from the SOM surveys are the outcome variables included in my regressions. Ideally, I would like to observe what every survey respondent actually votes for. This is the golden standard I try to get as close to as possible to when I choose the survey questions to construct my outcome variable.

I prefer to use a question where the respondent is asked to state their most preferred party (see Figure A.1). I construct a binary variable that takes on the value 1 if the respondent stated that they preferred SD and 0 otherwise. This way, I do not need to worry for neither the interpretation of the coefficients (see Section 6.1) or the sample size (25556 observations for 2015-2017, 18177 observations for 2016-2017)

There is also one question in the SOM survey that asks the respondent to state their support for the Sweden Democrats on an ordinal scale from -5 to 5 (see Figure A.2 and/or Table A.1 for details). Although this question could be used to study the change in SD support, I worry that the ordinal scale will make it difficult to interpret the results. In addition, the question was only asked to a subsample of respondents, so I only have approximately 4500 observations¹³. Another possible measure, the support on an ordinal scale for Sweden Democrat leader Jimmie Åkesson is ruled out for the same reasons (see Figure A.3 and/or Table A.1 for details). Both these questions are, however, used in my sensitivity analysis (see Section 6.4).

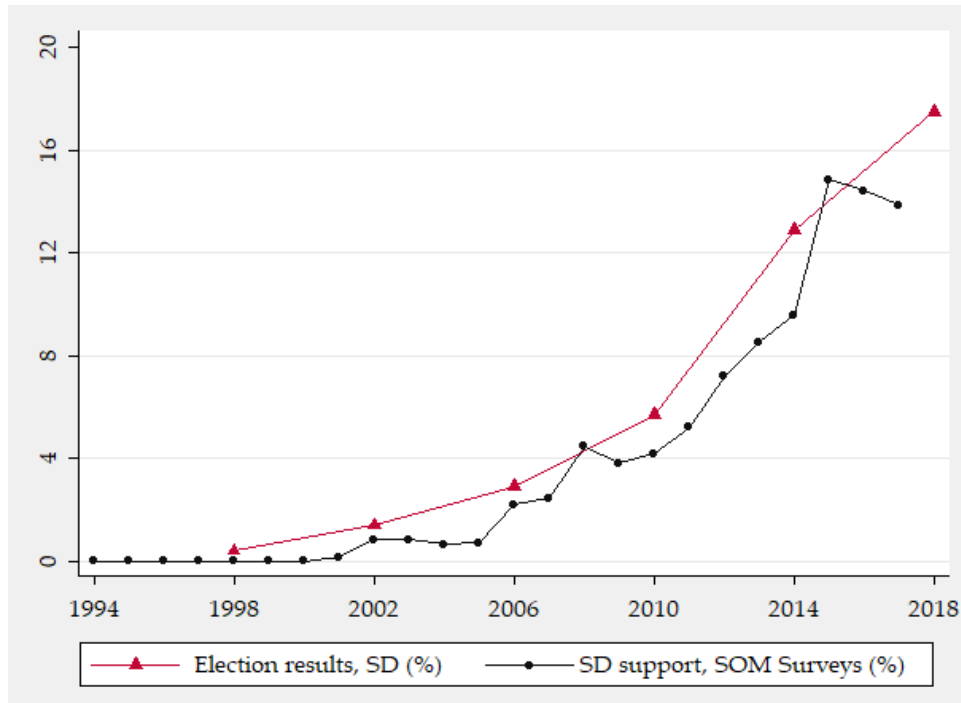


FIGURE 5.3: Sweden Democrat support over time

Above I discussed that there might be a systematic difference in the survey response rates between SD supporters and others (for example if SD voters are over-represented among men and young people). Indeed, it has been suggested that SD voters are less likely to answer polls and surveys than voters from other parties. However, there is also a risk that there could be a systematic difference in *how* SD voters answers the survey. In Figure 5.3, I plot the time trend of Sweden Democrat election results as well as the percentage of SOM respondents that state that the Sweden Democrats is their preferred party (note that the scale only shows a support of maximum 20 % of the population). The pattern is clear: the Sweden Democrats

¹³The SOM surveys are sent out in six survey waves. Some of the questions are the same in all waves whereas some differ. The question about the most preferred party is posed in every survey wave, but the questions about the support for the Sweden Democrats and Jimmie Åkesson are only posed in a few of the waves.

are generally underrepresented among SOM respondents (or they at least do not state their true party of preference). However, the trend in SOM respondent Sweden Democrat support follows the trend in national election results very closely (with a possible exception for the years since 2014).

If SD voters are less prone than others to answer surveys and state their real party of preference (maybe because of social stigma), my point estimates are likely to underestimate the real effect. This should, however, not be a large problem for neither the sign or the direction of my results as long as it is not related to the migrant redistribution patterns of the reform. I have no reason to believe it is.

There are a number of other survey variables from the SOM survey that I use in my analysis of heterogeneous effects as well as in my robustness checks. All the variables that I use are described in Appendix [A](#).

6 Empirical method

In the following, I describe the ideal experiment, my identification strategy and the identifying assumptions of my study. Furthermore, I discuss my efforts in identifying the mechanisms at work and possible heterogeneous effects. Then, I discuss the threats to identification as well as how I tackle them.

6.1 Identification strategy and the ideal experiment

In order to identify the causal effect of immigration on support for the Sweden Democrats, I would ideally like to conduct an experiment, randomly distributing refugees over Swedish municipalities. If I could observe individual-level changes in the propensity to vote for SD, the average difference in the change between treatment and control communities could be interpreted causally.

As this is impossible, I need to find another way to estimate the causal relationship. I cannot observe the propensity of an individual to vote for SD, but through my data from the SOM surveys I have access to individual-level self-stated party preferences. If I was to run a simple OLS regression of refugee migration on SD support, however, it would likely be biased as there could be omitted variables affecting both refugee inflow and SD support. For instance, refugees might avoid moving to municipalities with a weak labor market. At the same time, as we have seen in Section 2.1 and Section 3.1 above, there is both a theoretical and an empirical foundation for believing that unemployment in itself might increase right-wing populist support. In addition, reverse causality could also pose a problem for identification. It is reasonable to believe that immigrants might avoid municipalities with strong anti-immigrant sentiments (and thus, likely a high SD support). In both cases, an OLS regression would underestimate the true effect of immigration on SD support.

In order to remedy these problems, I instead use quasi-experimental variation in the number of refugees assigned to different Swedish municipalities to estimate the effect of refugee migration on SD support. Following the implementation of the 2016 refugee redistribution program, the Swedish Migration Agency considered six different variables when allocating refugees to counties. Thereafter, the county administrative boards considered availability of housing in addition to these six variables (see Chapter 4 for details about the reform and Chapter 5 for the covariate variable definitions). Conditional on these seven variables, the variation in refugee inflow at the municipal level in the period from 2016 onward is thus likely to be exogenous to municipal characteristics that also affect Sweden Democrat support (as discussed in Chapter 5 above I use proxies for the prognosed inflow of refugees moving voluntarily as for the availability of housing).

Following Dahlberg, Edmark, and Lundqvist (2012), I therefore propose an identification strategy using refugees assigned to the municipality as an instrument for the change in the number of immigrants living in the municipality. The first and second stage equations I estimate become:

$$RIR_{mt} = \alpha_m + \beta_t + \gamma_1 ARIR_{mt} + \gamma_2 Z_{mt} + u_{mt} \quad (6.1)$$

$$SD_{imt} = \alpha_m + \beta_t + \lambda_1 RIR_{mt} + \lambda_2 Z_{mt} + \epsilon_{mt} \quad (6.2)$$

In the equations above, SD_{imt} is a dummy that takes on the value 1 if the Sweden Democrats is the party preferred by individual i in municipality m in year t and 0 otherwise. Municipal and time fixed effects are given by α_m and β_t , respectively. $ARIR_{mt}$ is the inflow rate of refugees assigned to the municipality throughout the year and RIR_{mt} is the total inflow of refugees to the municipality during the same period. The main parameter of interest is λ_1 . It represents a change in the propensity of an individual to prefer SD if the refugee inflow grows with 1 percent of the municipality's population. The seven covariates (unemployment, the number of different occupations, the turnover rate to employment for migrants, lagged population, total refugee inflow 2005-2014, number of days asylum seekers stayed in housing provided by the Swedish Migration Agency and available municipal housing) are gathered in the vector Z_{mt} and the error terms are represented by u_{mt} and ϵ_{mt} . In my baseline specification, I use a linear regression (thus my estimation employs a linear probability model, a so called LPM), but as the dependent variable is binary I run probit regressions as a robustness check. Standard errors are clustered at the municipal level.

If the point estimate of λ_1 is positive and significant, this would lend support to the *group position theory* or the *ethnic competition theory*. If it is negative, this would be suggestive of the *contact hypothesis* being more credible. If there is no effect, this would suggest that researchers may need to look further into other theories (such as those of *social marginalization*) to explain the rising support for right-wing populists.

6.2 Identifying assumptions

There are four identifying assumptions that need to be fulfilled in order for me to interpret the effect causally. Firstly, assignment need to effect refugee migration rates. This is easily tested by regressing allocated refugee inflow on total inflow of refugees. Second, the assignment needs to be uncorrelated with the error term of my specifications, conditional on my covariates. Thanks to my in-depth institutional knowledge of the allocation mechanism used by Swedish authorities, I argue that this is the case conditional on the seven covariates discussed above. Third, assignment can only affect individual SD support through its effect on immigration. This could not be tested and might pose a problem to the strategy. However, it is unlikely that the number of assigned refugees in itself would affect SD support in any other way than through the total number of refugees coming to the municipality (as assignment is likely not salient to citizens). Lastly, there must be no municipalities getting a smaller inflow of immigrants because of the assignment. This is incredible as the smallest assignment possible is 0. There are no refugees moved from one municipality to another (that is, the number of refugees does not decrease in any municipality).

6.3 Mechanisms and heterogeneous effects

Previous studies with a research design similar to mine (see Section 3.2.1) have identified a positive effect of immigration on right-wing populist support and I expect to do so in my study as well. However, little is known about the mechanisms at work (whether the *group position theory*, *ethnic competition theory* or the *halo effect hypothesis* are best applicable). In my study, I would like to try and discern more about which mechanisms might actually play an important role.

The average effects might also hide interesting heterogeneity within the population. In particular, as I discussed in Section 2.1, theory might lead us to expect different responses from individuals in the tails of the income and skill distributions as well as from individuals suffering from different types of economic insecurity. The most common way to study these issues would be to estimate interaction effects, interacting refugee migration with variables on the individual characteristics of survey respondents.

However, I worry that there might be particular effects of each of the covariates in Z_{mt} in Equation 6.2 for respondents with different individual characteristics. For instance, unemployed individuals may react differently in their political preferences to a change in available housing than individuals with a job.

To avoid this problem, I will do a subsample analysis rather than an interaction model. For instance, I will rerun the estimation with only low-skilled individuals. This comes at the price of statistical power.

The coefficient λ_1 of Equation 6.2 for unemployed, low-skilled and low-income individuals is of particular interest. A larger point estimate than that for the entire population would lend support to the *ethnic competition theory* and/or *welfare chauvinism* at the cost of the *group position theory*. This is because individuals receiving social welfare or renting their apartment should be more likely to feel threatened by the competition over scarce resources from the incoming refugees. I also explore other eventual heterogeneous effects by rerunning the regressions separately for different groups based on age and sex. Dustmann, Vasiljeva, and Piil Damm (2018) find large differences between urban and rural municipalities in Denmark. I will conduct such an estimation in order to assess whether their conclusions hold in a Swedish setting as well.

6.4 Limitations

It is important to note that I am only able to capture short-term effects of refugee migration. Eventual dynamic effects of immigration are not observed. This probably makes it less likely for me to observe the effects of the *contact hypothesis* (as meaningful interaction may take some time). Another limitation is the stable unit treatment variable assumption (SUTVA). Refugees assigned to one municipality might affect SD support in neighbouring municipalities (not least through the *halo effect hypothesis* developed in Section 2.2.4). Even though it would be interesting to estimate this eventual cross-border effect, unless it is systematically related to refugee allocation, it should not bias my estimates.

In the specifications where I include the year 2015, I proxy all three labor market variables with the unemployment in the municipality. As discussed in Section 5.2, I also proxy the variable that captures the scope of accommodating asylum seekers in the municipality. Doing so, I particularly worry that the exclusion of the number of occupations and the turnover rate might bias my estimates. I suspect that they

might capture a dynamic local labor market or an urban environment, both of which might also correlate with the support for the Sweden Democrats (see for instance Dustmann, Vasiljeva, and Piil Damm, 2018).

Refugees are not forced to stay in the municipality they are assigned to. Instead, they can move elsewhere at any time. If the movements patterns are systematic, this might bias my estimates. As pointed out by Mehic (2019), refugees and other migrants indeed tend to resettle according to the same patterns as migrants that choose where to settle to begin with (away from small, rural municipalities and to large, urban cities). Both Dahlberg, Edmark, and Lundqvist (2012) and Mehic (2019) discuss this problem extensively. They conclude that resettlement is a minor problem in the short run (and that they are likely to be able to measure the effect before refugees move away). As I measure my outcome for three consecutive years (as opposed to Dahlberg, Edmark, and Lundqvist (2012) and Mehic (2019) who only measure it at the end of the election cycle), this reasoning is likely to be even more valid in my setting.

However, a problem that I have not been able to solve is that municipalities may rent housing for allocated refugees in other municipalities. If this is the case, I suspect that the estimated effect of refugee inflow on SD support will be downward biased. It is reasonable to assume that municipalities with populations that are negative to refugee migration are more prone to use this strategy than others (after all, the local politicians rely on popular support). Consider such a municipality. Assume that it is assigned a high number of refugees (maybe because it has taken on few refugees before) and that the municipality rents housing for the refugees in other municipalities. Then, Sweden Democrat support may stay roughly the same in the municipality as no refugees actually came there (even though the support would have increased a lot had the refugees actually come to the municipality). In my data, this would make it seem that there is a relatively little effect of the allocated refugees on Sweden Democrat support, even though the truth is actually the opposite.

Even though there have been reports about this kind of procedures, it is unclear whether they are common enough to threaten identification. The Swedish Association of Local Authorities and Regions (SKL) has data on the share of the refugee accommodation budget in each municipality that is used to rent housing for refugees elsewhere. I have investigated if I could somehow use this data to find out how large the bias might be. However, the cost of accommodating refugees varies much between different individuals (for instance, unaccompanied minors are much more expensive for municipalities than adult refugees). In addition, the SKL data only includes the municipalities' costs for renting housing from public providers. Many municipalities outsource the housing of refugees to private actors that may be situated in another municipality, but that are not present in the data. It is therefore difficult to assess the bias using this data.

It can however be said that an eventual bias would work in the opposite direction of my main hypothesis (if I still find a positive effect of refugee inflow on Sweden Democrat support, this will be despite, not because of, the fact that municipalities rent housing elsewhere).

7 Results

In this chapter, I present the main results of my thesis and discuss how to interpret them. I start by discussing the output of my first stage regressions (Section 7.1). Reassured by the power of my instrument, I then continue to discuss the results of my main specifications (Section 7.2). In Section 7.3, I discuss my estimates of heterogeneous effects. Lastly, in Section 7.4, I present the results from my placebo regressions and other robustness checks.

7.1 First stage estimates

As discussed in Section 6.2, a crucial identifying assumption of any IV strategy is a significant first stage. This means that the instrument needs to affect the endogenous variable. A common rule of thumb is that a F-test of the instrument in an estimation of the first stage equation should yield a F-value larger than 10 (Stock, Wright, and Yogo, 2002).

Table 7.1 shows the results of an estimation of my first stage equation (Equation 6.1). In specification (1)-(4) of Table 7.1 I have used the full panel including the years 2015-2017. In specification (1) and (2), I use my preferred variable definitions of the total refugee inflow rate (RIR) and allocated refugee inflow rate (ARIR) (see Equations 5.1 and 5.2 for details) as the dependent and the independent variable, respectively. In specification (3) and (4), I instead use the total refugee inflow and the total number of allocated refugees (not in relation to local population). I estimate the first stage both with and without covariates. As I am not able to use the full set of covariates in the panel 2015-2017, I use a limited set of covariates with proxies (see Section 5.2 for further details).

Specifications (5)-(8) of Table 7.1 show the first stage estimates with the panel limited to the years 2016-2017. I estimate the same specifications as in specifications (1)-(4), but instead using my preferred full set of covariates.

The relative inflow of allocated refugees indeed seems to affect the total inflow of refugees to the municipality. The point estimates in each of the four specifications (specification (1), (2), (5) and (6)) are positive and significant at the 1% level. The F-values are far higher than 10. However, the specifications with the absolute number of allocated refugees are not as significant and in the models where covariates are included, the F-value generally falls below 10 (specifications (4) and (8))¹⁴. I therefore choose to not use the specifications with the absolute inflow of refugees in my second stage estimations.

Moreover, I suspect that the raw estimates of specifications (1) and (5) may be endogenous (I have not included the covariates that I believe correlate with both the

¹⁴In specification (3) I actually obtain a F-value of 7610. This is suspiciously high. I have unsuccessfully tried to find out why this is the case (I suspect that it has something to do with multicollinearity in my control variables). However, as I do not use this specification in my second-stage regressions, this issue should not affect my results.

TABLE 7.1: First stage estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RIR^1	RIR^1	RI^2	RI^2	RIR^1	RIR^1	RI^2	RI^2
First panel year	2015	2015	2015	2015	2016	2016	2016	2016
$ARIR^3$	0.823*** (0.084)	0.753*** (0.082)			1.083*** (0.231)	0.820*** (0.172)		
ARI^4			0.937*** (0.011)	-0.533*** (0.202)			0.407 (1.724)	-1.089** (0.535)
Obs.	25,535	25,319	25,535	25,319	18,177	18,177	18,177	18,177
Adj. R^2	0.893	0.892	0.972	0.993	0.894	0.933	0.969	0.997
F-test	95.55	83.88	7610	6.984	21.92	22.78	0.0558	4.133
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Set of covar.	-	Limited	-	Limited	-	Full	-	Full

1. RIR = Refugee inflow rate (%), 2. RI = Refugee inflow (thousands)

3. $ARIR$ = Allocated refugee inflow rate (%), 4. ARI = Allocated refugee inflow (thousands)

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

instrument and the outcome variable in these specifications). Thus, in my main analysis, I use specifications (2) and (6) as my preferred first stage estimates. Reassured that I have a relevant instrument, I can move on to estimate my main specifications.

7.2 Second stage estimates

Table 7.2 shows the results of my main specifications. Specifications (1) and (2) display OLS regressions of the total refugee inflow rate on the propensity of a voter to state that their most preferred party is the Sweden Democrats. They use the full panel of 2015-2017 (specification (1) is the raw estimate of the refugee inflow rate on SD support, while specification (2) adds the limited covariates available for the year 2015). The point estimates indicate that a 1 % increase in the refugee inflow rate is associated with an 2.4 % and 2.7 % increased probability to prefer SD, respectively. However, the estimates are statistically indistinguishable from zero. I can therefore find no discernible pattern in the OLS estimates.

As discussed in Section 6.1 above, the OLS estimates are likely to be biased. In specification (3) of Table 7.2, I therefore present the 2SLS estimate with allocated refugee inflow as an instrument for total refugee inflow. The point estimate now becomes negative but remains indiscernible from zero.

Specification (4) and (5) again shows OLS regression estimates of refugee inflow as a share of population on SD support, but limits the panel to the years 2016-2017. Specification (5) employs the full set of covariates. The point estimates indicate that a 1 % increase in the refugee inflow rate is associated with an 5.6 % and 5.7 % increased probability to prefer SD, respectively. These results are significant on the 5 % level. However, when instead estimating the 2SLS results in specification (6) with the full model of covariates for the years 2016-2017, I obtain point estimates close to zero and no sign of statistical significance.

The 2SLS estimates of specifications (3) and (6) show point estimates that are statistically indistinguishable from zero. One interpretation of these results would be that there is no general effect of refugee migration on right-wing populist support. This would make my findings stand out in comparison to most of the previous literature on the topic.

TABLE 7.2: The effect of planned refugee migration on the propensity of voters to prefer SD

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
First panel year	2015	2015	2015	2016	2016	2016
Refugee inflow rate	0.024 (0.016)	0.027 (0.017)	-0.011 (0.047)	0.056** (0.023)	0.057** (0.028)	-0.0211 (0.102)
Observations	25,535	25,319	25,319	18,177	18,177	18,177
Adj. R ²	0.022	0.023	0.0225	0.023	0.023	0.0225
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
Set of covar.	-	Lim.	Lim.	-	Full	Full

The outcome is coded as 1 if SD is respondent's preferred party, 0 otherwise.

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

However, the small point estimates of specifications (3) and (6) could also arise, even in the case of a real effect, if there is a very low response rate from the very individuals who start voting for SD because of refugee migrations.

Another possibility that could explain the differences between my estimates and the results found by, for instance Mehic (2019) and Dustmann, Vasiljeva, and Piil Damm (2018), is that I measure the effect on SD support in the same year as the refugees are allocated. It is possible that an eventual effect of refugee migration on SD support is not immediate. Instead of changing in the short run, one could imagine that party preferences are inert and only change over a longer period (for example an election cycle). A safer interpretation of the estimates would thus be that I am not able to identify any short run effect of refugee migration on how much individuals state that they support SD.

In Table B.5 I instead use the actual allocation of refugees as the instrument to check the robustness of my results. Although the point estimates change slightly, the estimations of specifications (3) and (6) in Table B.5 confirm the zero results of Table 7.2. In specifications (4)-(6) of Table B.8 I re-estimate the main specifications from Table 7.2 using a probit model instead of a linear probability model. The table displays the marginal effect of refugee inflow at the average of the other covariates. The standard errors are rather large, but the marginal effects are indistinguishable from zero. If anything, the IV point estimates of specifications (5) and (6) of Table B.8 seems to indicate a negative effect of refugee inflow on Sweden Democrat support (which goes against my main hypothesis).

A problem with my main model is that it only captures the effect on individuals that start stating that the Sweden Democrats is their most preferred party as a consequence of refugee migration. There might also be a more indirect effect, namely that individuals instead change how *much* they like the Sweden Democrats because of refugee migration (even though they still have another party they like more). In

order to capture this effect, I re-estimate my main specifications with two alternative outcome variables. The first is the stated opinion about the party and the second is the stated opinion about the Sweden Democrat's leader Jimmie Åkesson (see Section 5.3 for details). These estimates are found in Table B.6 and Table B.7 of Appendix B. Whereas most point estimates are indiscernible from zero, the point estimates of specification (6) are large, and positive (they are however not significant as the standard errors are very large). Studying the first stage of specification (6) shows that the F-value has fallen below 10, which might be the reason why I observe these large point estimates. I am not able to derive any support for the hypothesis that refugee inflow has a positive effect on the support for the Sweden Democrats from the non-binary specifications of the outcome variables, either.

7.3 Heterogeneous effects

In this section, I present the results of the analysis of heterogeneous effects through the estimation of my main specifications on sub-samples of respondents depending on individual characteristics (discussed in Section 6.3). The goal is to find if the zero results of my main analysis might hide some interesting heterogeneous effects. My main focus is to study subsamples with different educational level, income and labor market status in order to test the *ethnic competition theory* and *welfare chauvinism*. For each of the subgroups, I have estimated three different specifications. The first is an OLS estimation of the model with the limited set of covariates in the period 2015-2017. Secondly, I run a 2SLS estimation of the panel 2015-2017, again with the limited set of covariates. Lastly, I run my preferred specification with the panel 2016-2017, but with the full set of covariates. For all of the specifications, both the endogenous variable and the instrument are defined as shares of the local population.

TABLE 7.3: Separate effects of refugee migration on SD support, depending on education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Education	Low	Low	Low	Med.	Med.	Med.	High	High	High
	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016	2015	2015	2016
<i>RIR</i> ¹	-0.034 (0.049)	0.027 (0.111)	-0.0725 (0.315)	0.064** (0.030)	-0.085 (0.077)	-0.0731 (0.163)	0.020 (0.021)	0.029 (0.052)	0.0134 (0.133)
Obs.	3,850	3,850	2,740	10,335	10,335	7,476	10,637	10,637	7,606
Adj. <i>R</i> ²	0.017	0.0166	0.0143	0.025	0.0216	0.0257	0.017	0.0173	0.0178
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full	Lim.	Lim.	Full

1. *RIR* = Refugee inflow rate (%).

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7.3 illustrates the results of these estimations divided on three subgroups

with varying levels of education ¹⁵. If *ethnic competition theory* or *welfare chauvinism* would be present, we would expect the point estimates for the group with low education to be large. However, most point estimates are relatively small and insignificant (the only exception is the OLS specification (4)). If anything, it seems like the groups with medium education are driving the large point estimate of specification (4) in Table B.5 (Section 7.2). However, I should be cautious when interpreting the coefficients as the number of observations in the group with low education is much smaller than the number of observations in the other groups. Nevertheless, I cannot find any support for neither *ethnic competition* nor *welfare chauvinism* based on education.

TABLE 7.4: Separate effects of refugee migration on SD support, depending on income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Income	Low	Low	Low	Med.	Med.	Med.	High	High	High
	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016	2015	2015	2016
<i>RIR</i> ¹	-0.005 (0.037)	0.139 (0.095)	0.162 (0.294)	0.049* (0.027)	-0.151* (0.082)	-0.173 (0.154)	0.022 (0.030)	0.006 (0.060)	0.138 (0.211)
Obs.	5,900	5,900	4,175	8,440	8,440	5,991	9,453	9,453	6,960
Adj. <i>R</i> ²	0.029	0.0256	0.0160	0.031	0.0249	0.0352	0.021	0.0215	0.0251
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full	Lim.	Lim.	Full

1. *RIR* = Refugee inflow rate (%).

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7.4 instead shows the results of similar regressions of subgroups based on income¹⁶. The results are quite similar. None of the estimates are statistically significant. The point estimates of the 2SLS estimations (specifications (2) and (3)) for the group with low income are considerable, but so is the estimate from specification (9) in the high income group. This could point to the fact that radical populist support is driven by individuals in the tails of the income distribution. However, it does not fit particularly well with the idea of *ethnic competition* or *welfare chauvinism* and I should be careful not to interpret the insignificant point estimates too much¹⁷.

In Table B.1 in Appendix B, I also try to test *ethnic competition theory* and *welfare chauvinism* by running regressions similar to those of Table 7.3 and Table 7.4 on subgroups depending on labor market situation. I code respondents into the category "insecure labor market situation" if they are unemployed, in labor market training or dependent on social welfare. I then compare the results from regressions with this

¹⁵ Respondents are coded as having low education if they have only completed grade 9 of comprehensive school or less. They are coded as medium if they have completed comprehensive school but have no university education. High education are respondents who have studied at, or have degrees from, university.

¹⁶ The variable divides respondents into three groups of roughly the equal size.

¹⁷ It could of course be the case that the point estimates of the low income group are driven by ethnic competition and the estimates for the high income group by an unwillingness to pay for social welfare (see Facchini and Mayda (2009)).

subgroup to regressions with respondents who have stated that they are "gainfully employed". The raw correlation between the inflow of refugees and SD support among those that are employed is positive and significant (a 1% increase in refugee inflow is associated with a 6.5 % increase in SD support). However, when introducing covariates and instrumenting for refugee inflow, the significance disappears. The point estimates for the instrumental variable specifications for those with an insecure labor market status are very large. However, the small sample size makes the standard errors so large that I cannot exclude even very large effects with the opposite sign). In general, I find very little support for the *ethnic competition theory* and *welfare chauvinism*.

Apart from my tests of *ethnic competition theory* and *welfare chauvinism*, I also run the main specifications separately for different age cohorts and genders. Of particular interest is my 2SLS specification with the full set of covariates, estimated separately for men and women (specifications (3) and (6) in Table B.2). Whereas the point estimate for men is large and negative, the point estimate for women is large and positive. Men are often said to be over-represented among SD voters, so these results are a little surprising. One possible interpretation would be that women may respond stronger in their SD preferences when exposed to refugee migration than men. However, I should be careful not to draw too strong conclusions as none of the point estimates are statistically significant.

The regressions for different age cohorts are presented in Table B.3. The coefficients for the 2SLS estimations do not differ substantially between the age cohorts. Most interesting is the OLS estimation for the youngest cohort. It is strongly positive and significant on the 1 %-level, thus indicating a strong association between municipalities with a large refugee inflow and strong SD support among young adults.

Previous studies such as Dustmann, Vasiljeva, and Piil Damm (2018) have suggested that urban and rural communities may show heterogeneous responses in the effect of immigration on the support for right-wing populists. In order to test whether this is the case in my study, I estimate my main specifications for urban and rural municipalities and respondents separately. Dustmann, Vasiljeva, and Piil Damm (2018) code the 5% largest municipalities as urban, and all others as rural. However, using this measure in my setting might be misleading. The measure was developed for a Danish context where urban areas are much more dominated by a few large cities. In Sweden, there are many more mid-sized towns that are not among the 5 % largest municipalities, yet could not be classified as rural areas in any common meaning of the word. In addition, the measure developed by Dustmann, Vasiljeva, and Piil Damm (2018) fails to capture that there might be both urban and rural areas within the same municipality.

In specifications (1)-(6) of Table 7.5, I have therefore instead used the respondents self-stated neighbourhood to construct a measure of urbanity. If respondents have stated that they live in a "village" or in a "rural area", I have coded them as living in a rural area. If they have stated that they live in a "town", a "city" or a "larger urban settlement", I have classified them as living in an urban area. Using this measure, I find no sign of heterogeneous effects between urban and rural communities.

In order to make my results more comparable, I also estimate the heterogeneous effects in urban and rural municipalities following the definition of Dustmann, Vasiljeva, and Piil Damm (2018). The estimates of my preferred specifications using these definitions are found in specifications (1)-(6) in Table B.4. Most point estimates are statistically indistinguishable from zero, with the notable exception of specification (3) which displays a large and highly significant negative point estimate. This result suggests a negative effect of refugee migration on Sweden Democrat support in

TABLE 7.5: Separate effects of refugee migration on SD support, depending on urban or rural respondent.

	(1)	(2)	(3)	(4)	(5)	(6)
	Urban	Urban	Urban	Rural	Rural	Rural
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016
Refugee inflow rate	0.027 (0.023)	-0.006 (0.058)	-0.116 (0.139)	0.031 (0.026)	-0.009 (0.075)	0.0145 (0.146)
Observations	16,504	16,504	11,818	8,290	8,290	5,977
Adj. R ²	0.018	0.0183	0.0177	0.012	0.0117	0.0106
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

Standard errors (in parentheses) are clustered on municipality level

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

urban areas (the estimate tells us that a 1% increase in refugee inflow as a share of population is associated with an almost 29% decrease in the support for the Sweden Democrats). This would lend support to the contact hypothesis in an urban setting. However, the analysis of urban municipalities includes only 15 clusters. When compensating for this using the wild bootstrap (as suggested by Cameron, Gelbach, and Miller (2008)), the adjusted p-value for specification (3) rises to over 0.18. Thus, the estimate is no longer statistically significant.

7.4 Sensitivity analysis

In this section, I present the results from my sensitivity analysis.

One potential threat to identification is sample selection. For instance, if natives with anti-immigrant preferences choose to move away from municipalities that are assigned many refugees, my estimates risk being downward biased. I can control for this by studying how the number of native-born adults in the population fluctuates following the reform. To test if native outflow is a problem for my estimates, I run my three preferred specifications with the native population as the outcome variable (remember that I control for municipal and time fixed effects as well as lagged population in all specifications). Unless the point estimates are negative, an outflow of natives should not be an urgent problem for my study. This is a strategy previously used by Andersson, Berg, and Dahlberg (2018). The results are reported in specifications (1)-(3) of Table 7.6.

Following Dustmann, Vasiljeva, and Piil Damm (2018), I also include native-born population as a share of total population in 2015 (the start of the measurement period) in specifications (4)-(6) of Table 7.6. If there is a significant leakage of natives from municipalities that take on many refugees, I would expect to see negative point estimates.

The OLS specifications (1) and (4) seem to show that there is a small but positive and significant association between the total refugee inflow rate and both the absolute and relative change in native population. The estimates (2) and (5) with limited covariates and the full panel 2015-2017 seem to tell the same story. However, when

adding all covariates and excluding the year 2015, the estimates (3) and (6) instead become negative and significant.

TABLE 7.6: Test of Native Flight

	(1)	(2)	(3)	(4)	(5)	(6)
First panel year	2015	2015	2016	2015	2015	2016
	NP ²	NP ²	NP ²	NPR ³	NPR ³	NPR ³
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
RIR ¹	294.553*	167.990	-1,271*	0.009***	0.029***	-0.0175***
	(159.814)	(425.985)	(676.7)	(0.001)	(0.004)	(0.00601)
Observations	28,634	28,634	20,632	28,634	28,634	20,632
Adj. R ²	1.000	1	1	0.997	0.996	0.998
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

1. RIR = Refugee inflow rate (%), 2. NP = Total native-born population.

3. NPR = Native-born population as a share of total population.

Standard errors (in parentheses) are clustered on municipality level.

*** p<0.01, ** p<0.05, * p<0.1

It seems unlikely that an increased refugee inflow to a municipality would cause natives to move there. Instead, I believe that there is endogeneity in the specification I use for the years 2015-2017. As mentioned in Section 6.2 above, I think it is possible that by dropping the variables for number of occupation and the turnover rate from unemployment to employment for migrants, I might miss important factors affecting both allocated refugee inflow and native population trends. For instance, I may miss to capture important factors about the functioning and dynamics of the local labor market. These factors may also co-vary with the propensity to support SD, thus biasing the estimates using the limited set of covariates.

It seems like there is a significant effect of refugee inflow on the outflow of the native population, particularly for the relative variable definition. The point estimate suggests that an increase of 1 % in the inflow of refugees as a share of municipal population is associated with a decrease in municipal native population of 1.75 % of total population. In addition, the estimate is significant on the 1 %-level. This might be one part of the explanation why I cannot find an effect of refugee migration on Sweden Democrat support. As my estimates in Table 7.6 seem to be very sensitive to changes in the specification, I should however avoid drawing too strong conclusions from this estimate (which intuitively seems large).

It is important to remember that the estimate I obtain from my main specifications are local average treatment effects. That is, they measure the effect of immigration on SD support among individuals in the municipalities that were most strongly affected by the reform. This can pose a problem in generalizing the result. It might also prove a problem for the exclusion restriction. Although the number of allocated refugees is probably not salient in the average municipality, it might be in the municipalities that are bound by the reform. That is, there might be an effect of being forced to accommodate refugees on SD support which is separate from the effect of immigration on SD support. However, even if I still would capture some of the effect from being forced rather than the pure effect of refugee migration, this full effect

would still be interesting from a policy perspective (for instance for a decision-maker trying to evaluate the consequences of a refugee redistribution program).

To make sure that this effect is not driving my results, I rerun the regressions without the eight municipalities that were forced to take on refugees. I use the same three specifications as the estimations of heterogeneous effects in Section 7.3. The results are presented in columns (1)-(3) of Table B.8. The estimates are very similar to their equivalents in Table 7.2 (specifications (2), (3) and (6)). Thus, it does not seem as if the municipalities that were forced are driving my estimates.

7.4.1 Placebo tests

A further threat to identification is the validity of the exclusion restriction. It could be violated if county administrative boards systematically assign refugees according to omitted variables affecting both assignment and SD support. If they consider SD support in their allocation decisions, reverse causality could be present.

This could be tested by running placebo tests using lagged municipal mean support for the Sweden Democrats as the outcome variable. If my instrument really is exogenous to all factors affecting both refugee migration and right-wing populist support, I should not be able to observe any effect of refugee migration on lagged Sweden Democrat support. The reason is that the lagged Sweden Democrat support was measured before the refugees were allocated. Thus, the allocated refugees should have had no chance to affect the Sweden Democrat preferences in the municipalities.

TABLE 7.7: Placebo tests

	(1) SD (lag.) 2015	(2) SD (lag.) 2015	(3) SD (lag.) 2016	(4) Cinema 2015	(5) Cinema 2015	(6) Cinema 2016
First panel year						
RIR ¹	-0.018 (0.019)	-0.088** (0.041)	-0.144 (0.130)	-0.030* (0.018)	-0.325*** (0.065)	-0.0485 (0.0724)
Obs.	25,319	25,319	18,177	25,319	25,319	18,177
Adj. R ²	0.568	0.557	0.657	0.156	0.140	0.00784
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

1. RIR = Refugee inflow rate (%)

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Another possible placebo test is to instead use answers to other questions in the SOM survey as outcome variables. The answer to these questions should not be affected by the inflow of refugees. For a question to be a credible candidate for a placebo test, it should moreover be asked in all waves of the SOM surveys and have no relationship to other political preferences or to socio-economic status.

The purpose of such a placebo test is to find out if the allocation of refugees really is exogenous to the support for the Sweden Democrats. I choose to use an indicator of whether the respondent has been to the cinema in the last year as placebo outcome. Going to the cinema is arguably a very widespread pastime in all parts of Sweden and has little connection to political or socio-economic characteristics.

Thus, I do not expect that the inflow of refugees in itself should affect the propensity of respondents going to the cinema. If there is an observed effect, I would suspect that there might be endogeneity in the allocation of refugees to other municipal characteristics (that correlate with cinema-going). Such endogeneity could indicate that refugee allocation is not exogenous to factors that in turn may affect SD support, thereby challenging the causal interpretation of my estimates. If the points estimates instead are close to zero, I will rest more assured of the exogeneity of my instruments.

The results of the placebo tests are presented in specifications (1)- (6) of Table 7.7. Unfortunately, it seems like my specification with the year 2015 and a limited set of covariates (which in many ways resemble specifications used in the previous literature) do not manage the placebo tests very well. Studying the placebo regression with lagged SD support (specification (2)), the point estimate is relatively large, strongly significant and negative.

When instead studying the placebo test with cinema-going as the outcome (specification (5) of Table 7.7), I find similar results. The estimate is large, negative and significant on the 1 %-level. In fact, it suggest that a 1 % increase in the refugee inflow rate is associated with an almost 33 % lower probability of the respondent having been to the cinema during the last year.

The estimates of the full model and the year 2016-2017 are however indiscernible from zero, both when I run the placebo tests on lagged SD support and on cinema visits (specifications (3) and (6), respectively).

These results suggest that my identifying assumption of instrument exogeneity does not hold up for the limited specification. Indeed, it seems like there might be factors correlated with both refugee allocation and Sweden Democrat support that bias my estimates.

These results show that the limited covariate model that in many ways resemble the models used by Dahlberg, Edmark, and Lundqvist (2012) and Mehic (2019) is likely to produce biased estimates in my setting.

8 Conclusion

In this paper, I have used quasi-experimental variation from a refugee redistribution program in Sweden to estimate the effect of refugee inflow on the support for the Swedish right-wing populist party the Sweden Democrats. I have exploited panel data on municipal characteristics and immigration on municipal level as well as individual-level repeated cross-sectional survey data in order to employ an instrumental variable identification strategy. In particular, I use in-depth institutional knowledge to make sure that my instrument really is exogenous to factors affecting support for the Sweden Democrats.

The estimates from most of my specifications are both economically and statistically insignificant. Thus, I am not able to find support for any effect of refugee migration on respondents' propensity to state that they support the Sweden Democrats, at least not in the short run. Thus, I cannot provide any support for neither the *group position theory*, nor the *contact hypothesis*. Analyzing heterogeneous effects, I am not able to find any support for the *ethnic competition theory* or *welfare chauvinism*, either.

I also show that a specification using rather coarse proxies (which is common in the literature) is likely to yield biased estimates in my setting. However, even though my main specifications hold up to the placebo tests, the resulting estimates have large standard errors which makes an economically relevant interpretation of my results difficult. This is probably partly due to the few years available in my panel data set as well as to the relatively high multicollinearity between some of the covariates. This study would benefit from being replicated in a few years when more data is available.

An additional limitation of the paper is it being dependent on survey data to measure the outcome variable. It thus becomes difficult to avoid sample selection, particularly as right-wing populist supporters by some are believed to systematically differ in their survey responses.

My main contribution to the literature on the determinants of right-wing populist support is to underline the importance of institutional knowledge when evaluating refugee redistribution programs. I show that estimates often are very sensitive to changes in covariates and variable definitions.

These findings emphasize the need for additional research on this important topic.

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A Descriptive statistics on, and questions from, the National SOM Surveys

A.1 Descriptive statistics SOM Surveys

TABLE A.1: Descriptive statistics: The SOM Surveys 2015-2017

Preferred party: SD		Freq.	Percent	Valid	Cum.
Valid answers	No	21897	75.76	85.68	85.68
	Yes	3659	12.66	14.32	100.00
	Total	25556	88.42	100.00	
Missing	.	3347	11.58		
Total		28903	100.00		
SD support: ordinal scale		Freq.	Percent	Valid	Cum.
Valid answers	-5	2167	7.50	48.37	48.37
	-4	286	0.99	6.38	54.75
	-3	193	0.67	4.31	59.06
	-2	136	0.47	3.04	62.10
	-1	98	0.34	2.19	64.29
	0	559	1.93	12.48	76.76
	1	184	0.64	4.11	80.87
	2	193	0.67	4.31	85.18
	3	242	0.84	5.40	90.58
	4	183	0.63	4.08	94.67
	5	239	0.83	5.33	100.00
	Total	4480	15.50	100.00	
Missing	.	24423	84.50		
Total		28903	100.00		
SD leader support: ordinal scale		Freq.	Percent	Valid	Cum.
Valid answers	-5	1784	6.17	40.92	40.92
	-4	301	1.04	6.90	47.82
	-3	226	0.78	5.18	53.00
	-2	152	0.53	3.49	56.49
	-1	137	0.47	3.14	59.63
	0	676	2.34	15.50	75.14
	1	208	0.72	4.77	79.91
	2	205	0.71	4.70	84.61
	3	204	0.71	4.68	89.29
	4	191	0.66	4.38	93.67
	5	276	0.95	6.33	100.00

Continued on next page

Table A.1 – Continued from previous page

	Total	4360	15.08	100.00	
Missing	.	24543	84.92		
Total		28903	100.00		
Gender		Freq.	Percent	Valid	Cum.
Valid answers	Woman	15158	52.44	52.45	52.45
	Man	13678	47.32	47.33	99.78
	-	64	0.22	0.22	100.00
	Total	28900	99.99	100.00	
Missing	.	3	0.01		
Total		28903	100.00		
Education		Freq.	Percent	Valid	Cum.
Valid answers	Low	4442	15.37	15.76	15.76
	Medium	11886	41.12	42.18	57.94
	High	11853	41.01	42.06	100.00
	Total	28181	97.50	100.00	
Missing	.	722	2.50		
Total		28903	100.00		
Household income		Freq.	Percent	Valid	Cum.
Valid answers	Low	6817	23.59	25.46	25.46
	Medium	9498	32.86	35.48	60.94
	High	10458	36.18	39.06	100.00
	Total	26773	92.63	100.00	
Missing	.	2130	7.37		
Total		28903	100.00		
Age		Freq.	Percent	Valid	Cum.
Valid answers	16-29	4219	14.60	14.60	14.60
	30-49	8141	28.17	28.17	42.77
	50-64	7557	26.15	26.15	68.92
	64-	8981	31.07	31.08	100.00
	Total	28898	99.98	100.00	
Missing	.	5	0.02		
Total		28903	100.00		
Labor market situation		Freq.	Percent	Valid	Cum.
Valid answers	Employed	14701	50.86	53.98	53.98
	Labor mark. train.	376	1.30	1.38	55.36
	Unemployed	608	2.10	2.23	57.60
	Old age pens.	8555	29.60	31.41	89.01
	Disability pens.	657	2.27	2.41	91.42
	Student	1985	6.87	7.29	98.71
	Other	351	1.21	1.29	100.00
	Total	27233	94.22	100.00	
Missing	.	1670	5.78		
Total		28903	100.00		

A.2 Covariance matrix and questions from SOM Surveys

Fråga 12 Vilket parti tycker du bäst om i dag?

<input type="checkbox"/> Vänsterpartiet	<input type="checkbox"/> Moderaterna	<input type="checkbox"/> Feministiskt initiativ
<input type="checkbox"/> Socialdemokraterna	<input type="checkbox"/> Kristdemokraterna	<input type="checkbox"/> Annat parti:
<input type="checkbox"/> Centerpartiet	<input type="checkbox"/> Miljöpartiet	
<input type="checkbox"/> Liberalerna	<input type="checkbox"/> Sverigedemokraterna	

Anser du dig vara en övertygad anhängare av detta parti?

<input type="checkbox"/> Ja, mycket övertygad	<input type="checkbox"/> Ja, något övertygad	<input type="checkbox"/> Nej
---	--	------------------------------

FIGURE A.1: SOM question: Used to construct binary variable for SD support.

Fråga 17 Var skulle du vilja placera de olika partierna på nedanstående skala?

	Ogillar starkt ↓		Varken gillar eller ogillar ↓		Gillar starkt ↓						
	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Centerpartiet	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Moderaterna	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Vänsterpartiet	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Liberalerna	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Socialdemokraterna	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Miljöpartiet	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Kristdemokraterna	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Sverigedemokraterna	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Feministiskt initiativ	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

FIGURE A.2: SOM question: Used to construct ordinal variable for SD support.

Fråga 18 Om du skulle använda samma skala för några partiledare, var skulle du placera:

		Ogillar starkt		Varken gillar eller ogillar					Gillar starkt			
	Personen är okänd för mig	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Annie Lööf	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Anna Kinberg Batra	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Jonas Sjöstedt	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Jan Björklund	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Stefan Löfven	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Gustav Fridolin	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Isabella Lövin	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Ebba Busch Thor	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Jimmie Åkesson	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Gudrun Schyman	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

FIGURE A.3: SOM question: Used to construct ordinal variable for support for SD leader Jimmie Åkesson.

Fråga 41 Är du:

☐ Kvinna ☐ Man ☐ Annat:

FIGURE A.4: SOM question: Used to construct gender variable.

Fråga 59 Vilken skolutbildning har du? Om du ännu inte avslutat din utbildning, markera den du genomgår för närvarande.

- ☐ Ej fullgjort grundskola (eller motsvarande obligatorisk skola)
- ☐ Grundskola (eller motsvarande obligatorisk skola)
- ☐ Studier vid gymnasium, folkhögskola (eller motsvarande)
- ☐ Examen från gymnasium, folkhögskola (eller motsvarande)
- ☐ Eftergymnasial utbildning, ej högskola/universitet
- ☐ Studier vid högskola/universitet
- ☐ Examen från högskola/universitet
- ☐ Studier vid/examen från forskarutbildning

FIGURE A.5: SOM question: Used to construct education variable.

Respondents are coded as having low education if they have only completed grade 9 of comprehensive school or less. They are coded as medium if they have completed comprehensive school but have no university education. High education are respondents who have studied at, or have degrees from, university.

TABLE A.2: Covariance matrix

	A.	B.	C.	D.	E.	F.	G.	H.	I.	J.	K.	L.
A. SD support	1.000											
B. Planned ARI	-0.286***	1.000										
C. ARI	-0.282***	0.977***	1.000									
D. Population	-0.319***	0.807***	0.792***	1.000								
E. Tot. ref. infl. 2005-2014	-0.297***	0.783***	0.790***	0.956***	1.000							
F. Prog. vol. ref. infl.	-0.299***	0.799***	0.799***	0.913***	0.973***	1.000						
G. Asyl. seek. (days/pop.)	0.177***	-0.208***	-0.198***	-0.208***	-0.164***	-0.157***	1.000					
H. Turnover to emp. (migr.)	-0.323***	0.433***	0.417***	0.388***	0.307***	0.252***	-0.346***	1.000				
I. No. of occupations	-0.370***	0.615***	0.652***	0.687***	0.749***	0.722***	-0.312***	0.300***	1.000			
J. Available housing	-0.253***	0.821***	0.808***	0.922***	0.879***	0.808***	-0.104***	0.352***	0.558***	1.000		
K. Total mun. unemp.	0.178***	-0.152***	-0.151***	-0.036***	0.135***	0.209***	0.318***	-0.385***	0.102***	0.058***	1.000	
L. Unemp. (16-64 years)	0.078***	-0.143***	-0.149***	-0.141***	-0.126***	-0.130***	0.162***	-0.191***	-0.169***	-0.073***	0.119***	1.000

Fråga 61 Vilken är den ungefärliga sammanlagda årsinkomsten i kronor för samtliga personer i ditt hushåll före skatt (pension, studiemedel etc. ska räknas in)?

- | | |
|---|--|
| <input type="checkbox"/> 100 000 eller mindre | <input type="checkbox"/> 601 000 – 700 000 |
| <input type="checkbox"/> 101 000 – 200 000 | <input type="checkbox"/> 701 000 – 800 000 |
| <input type="checkbox"/> 201 000 – 300 000 | <input type="checkbox"/> 801 000 – 900 000 |
| <input type="checkbox"/> 301 000 – 400 000 | <input type="checkbox"/> 901 000 – 1 000 000 |
| <input type="checkbox"/> 401 000 – 500 000 | <input type="checkbox"/> 1 001 000 – 1 100 000 |
| <input type="checkbox"/> 501 000 – 600 000 | <input type="checkbox"/> Mer än 1 100 000 |

FIGURE A.6: SOM question: Used to construct household income variable.

The variable divides respondents into three groups of roughly the equal size.

Fråga 42 Vilket år är du född?

Årtal:

FIGURE A.7: SOM question: Used to construct age variable

Fråga 41 Vilken av de här grupperna tillhör du för närvarande?

- | | |
|--|--|
| <input type="checkbox"/> Förvärsarbetande (även sjukskriven, föräldraledig) | <input type="checkbox"/> Ålderspensionär/avtalspensionär |
| <input type="checkbox"/> Har arbete i arbetsmarknadspolitiska åtgärder/
genomgår arbetsmarknadsutbildning | <input type="checkbox"/> Har sjuk-/aktivitetsersättning |
| <input type="checkbox"/> Arbetslös | <input type="checkbox"/> Studerande |
| | <input type="checkbox"/> Annat: |

FIGURE A.8: SOM question: Used to construct labor market situation variable.

Insecure labor market situation is coded as being unemployed, in labor market training or dependent on social welfare.

Fråga 56 I vilken typ av område bor du?

- | | |
|---|--|
| <input type="checkbox"/> Storstad: centralt | <input type="checkbox"/> Större tätort |
| <input type="checkbox"/> Storstad: ytterområde/förort | <input type="checkbox"/> Mindre tätort |
| <input type="checkbox"/> Stad: centralt | <input type="checkbox"/> Ren landsbygd |
| <input type="checkbox"/> Stad: ytterområde | |

FIGURE A.9: SOM question: Used to define urbanity.

Rural is coded as answering "Mindre tätort" (Village) or "Ren landsbygd" (rural area), while urban is coded as answering one of the other alternatives.

B Heterogeneous Effects and Sensitivity Analysis

B.1 Heterogeneous Effects

TABLE B.1: Separate effects of refugee migration on SD support, depending on labor market status

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp.	Emp.	Emp.	Insecure ²	Insecure ²	Insecure ²
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016
RIR ¹	0.065** (0.026)	0.045 (0.066)	0.115 (0.148)	-0.075 (0.104)	-0.178 (0.210)	0.610 (0.893)
Observations	13,087	13,087	9,374	1,381	1,381	990
Adj. R^2	0.032	0.0320	0.0324	0.040	0.0384	0.0385
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

1. RIR = Refugee inflow rate (%).

2. Insecure labor market status includes unemployed and social welfare recipients.

Standard errors (in parentheses) are clustered on municipal level

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE B.2: Separate effects of refugee migration on SD support, depending on sex

	(1)	(2)	(3)	(4)	(5)	(6)
	Women	Women	Women	Men	Men	Men
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016
<i>RIR</i> ¹	0.012 (0.023)	-0.041 (0.052)	0.170 (0.149)	0.037 (0.029)	-0.006 (0.076)	-0.170 (0.171)
Obs.	13,017	13,017	9,288	12,250	12,250	8,848
Adj. <i>R</i> ²	0.030	0.0292	0.0310	0.022	0.0221	0.0264
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

1. *RIR* = Refugee inflow rate (%).

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE B.3: Separate effects of refugee migration on SD support, depending on age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	16-29 2015	16-29 2015	16-29 2016	30-49 2015	30-49 2015	30-49 2016	50-64 2015	50-64 2015	50-64 2016	64- 2015	64- 2015	64- 2016
R/R^1	0.163*** (0.062)	0.001 (0.109)	-0.0542 (0.237)	-0.008 (0.033)	-0.013 (0.094)	-0.0262 (0.259)	0.054 (0.035)	-0.002 (0.085)	0.0779 (0.209)	-0.024 (0.026)	-0.028 (0.070)	0.0255 (0.178)
Observations	3,584	3,584	2,577	7,194	7,194	5,181	6,609	6,609	4,716	7,932	7,932	5,703
Adj. R^2	0.062	0.0591	0.0841	0.045	0.0451	0.0400	0.022	0.0216	0.0200	0.017	0.0166	0.0163
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full	Lim.	Lim.	Full	Lim.	Lim.	Full

1. R/R = Refugee inflow rate (%).

Standard errors (in parentheses) are clustered on municipal level

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE B.4: Heterogeneous effects over urban and rural communities
using the definition from Dustman et al. (2018)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
First panel year	2015	2015	2016	2015	2015	2016
Refugee inflow rate	0.002 (0.028)	-0.200 (0.333)	-0.288** (0.132)	0.016 (0.022)	-0.063 (0.077)	-0.0229 (0.107)
Bootstrapped p-value	0.949	0.525	0.181			
Observations	8,642	8,642	6,191	16,677	16,677	11,986
Adj. R ²	0.009	0.00751	0.00958	0.020	0.0190	0.0174
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

Standard errors (in parentheses) are clustered on municipal level.

*** p<0.01, ** p<0.05, * p<0.1

B.2 Sensitivity Analysis

TABLE B.5: The effect of refugee migration on the propensity of voters to prefer SD

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
First panel year	2015	2015	2015	2016	2016	2016
Refugee inflow rate	0.024 (0.016)	0.027 (0.017)	-0.000 (0.037)	0.056** (0.023)	0.057** (0.028)	0.0940 (0.0800)
Observations	25,535	25,319	25,319	18,177	18,177	18,177
Adj. R ²	0.022	0.023	0.0226	0.023	0.023	0.0228
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
Set of covar.	-	Lim.	Lim.	-	Full	Full

The outcome is coded as 1 if SD is respondent's preferred party, 0 otherwise.

Standard errors (in parentheses) are clustered on municipality level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE B.6: The effect of refugee migration on voters preferences for SD on an ordinal scale

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
First panel year	2015	2015	2015	2016	2016	2016
Refugee inflow rate	0.288 (0.314)	0.284 (0.320)	0.914 (0.837)	0.422 (0.586)	0.327 (0.759)	4.971 (4.270)
Observations	4,475	4,437	4,437	3,043	3,043	3,043
Adj. R ²	0.033	0.033	0.0324	0.040	0.040	0.0253
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
Set of covar.	-	Lim.	Lim.	-	Full	Full

The outcome variable, SD preference, varies between -5 and 5.

Standard errors (in parentheses) are clustered on municipal level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE B.7: The effect of refugee migration on the support for Jimmie Åkesson, the leader of SD

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
First panel year	2015	2015	2015	2016	2016	2016
Refugee inflow rate	0.612* (0.360)	0.626* (0.367)	0.989 (0.823)	0.783 (0.673)	0.039 (0.889)	4.313 (3.622)
Observations	4,356	4,319	4,319	2,966	2,966	2,966
Adjusted R-squared	0.030	0.031	0.0305	0.027	0.027	0.0148
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
Set of covar.	-	Lim.	Lim.	-	Full	Full

The outcome variable is trust in Jimmie Åkesson and varies between -5 and 5.

Standard errors (in parentheses) are clustered on municipal level.

*** p<0.01, ** p<0.05, * p<0.1

TABLE B.8: Second stage regressions with excluded forced municipalities and probit estimations of main specifications

	(1)	(2)	(3)	(4)	(5)	(6)
First panel year	2015	2015	2016	2015	2015	2016
	OLS	2SLS	2SLS	Probit	Probit (IV)	Probit (IV)
	Coef.	Coef.	Coef.	Marg. eff.	Marg. eff.	Marg. eff.
Refugee inflow rate	0.031* (0.018)	0.009 (0.046)	-0.00566 (0.0959)	0.024 (0.015)	-0.039 (0.200)	-0.154 (0.448)
Observations	24,455	24,455	17,573	25,254	25,254	18,081
Adj. R ²	0.023	0.0234	0.0237	-	-	-
FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Set of covar.	Lim.	Lim.	Full	Lim.	Lim.	Full

Standard errors (in parentheses) are clustered on municipality level.

*** p<0.01, ** p<0.05, * p<0.1