Master Thesis Economics & Business - Policy Economics
ERASMUS SCHOOL OF ECONOMICS

Exposure to Refugees and Voting for the Far-Right: Evidence from The Netherlands

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Submission	: July 2018

Abstract

In my thesis I attempt to estimate the causal impact of micro-level exposure to refugees on the support for right-wing populism in the Netherlands. I exploit the recent European refugee crisis (2013-2016), during which some Dutch municipalities opened refugee shelter locations and some did not, in a difference-in-difference model, using the local vote share of the main Dutch right-wing populist party as an outcome variable. Results indicate that the opening of a shelter location in a municipality during the refugee crisis had no statistically significant impact on local support for right-wing populism.

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1 Introduction

The political climate of the current decade is characterised by a rise of right-wing populist parties in almost all Western nations. In e.g. France (Front National), Germany (Alternative für Deutschland), Austria (FPÖ) and Belgium (Vlaams Belang), right-wing populism has conquered a prominent role on the political stage, even more so in the wake of the European refugee crisis (2013-2016), during which anti-immigrant sentiment skyrocketed among European citizens (Greven, 2016; Mudde, 2016). In my thesis I attempt to make a contribution to the literature that examines the link between the refugee crisis and right-wing populist parties' results in elections. More specifically, I focus on how the support for right-wing populism in the Netherlands, most notably represented by the PVV party[1], was affected by increased exposure to refugees at the micro-level.

While the effect of *macro*-level exposure to immigrants can be defined as the 'general' impact of immigration, the effect of *micro*-level exposure is purely about the impact of housing immigrants in one's local community. It can be regarded as the isolated effect of changing levels of xenophobia in response to direct encounters with immigrants (Steinmayr, 2016).

In October 2015, the Netherlands experienced an unprecedented inflow of non-European asylum seekers (figure 1, figure 2). Simultaneously, the performance of the PVV in the polls increased steeply (figure 3), rising from 13% to well over 20%, a level that was maintained for more than half a year. These trends suggest that increased macro-level exposure to refugee immigration had a positive effect on the support for right-wing populism in the Netherlands. But was there also an effect of micro-level exposure?

During the refugee crisis, some Dutch municipalities opened a shelter location for asylum seekers, some did not. This means that the increase in micro-level exposure to refugees varied across the population. I exploit this variation in a difference-in-difference design, with the municipality-level vote share of the PVV as an outcome variable. I use the last general elections before the refugee crisis (2012) as the pre-treatment moment and the first general elections after the refugee crisis (2017) as the post-treatment moment. As a treatment group, I use all municipalities that opened a (extra) refugee shelter location in between the two elections. As a control group, I use all remaining municipalities. The

[1] As of 2018, fellow right-wing populist party FvD are equally successful in poll results. However, my thesis only considers the period until the 2017 elections, at which the PVV party were still ten times as large as FvD (see the Kiesraad database mentioned in appendix 6.2). Hence I focus on the PVV and ignore FvD.

difference-in-difference estimator then measures the causal impact of opening a (extra) shelter location on the local PVV vote share, where the opening of a (extra) shelter location is used as a proxy for increased micro-level exposure to refugees.

Some municipalities already housed a shelter in 2012. Hence the addition of (extra): it must not be the case that the new shelter was opened *after* an initial one was closed. The opening must have led to an increase in the number of shelters as compared to 2012. Only then, there has been increased micro-level exposure to refugees in the period leading up to the 2017 elections as compared to the period leading up to the 2012 elections.

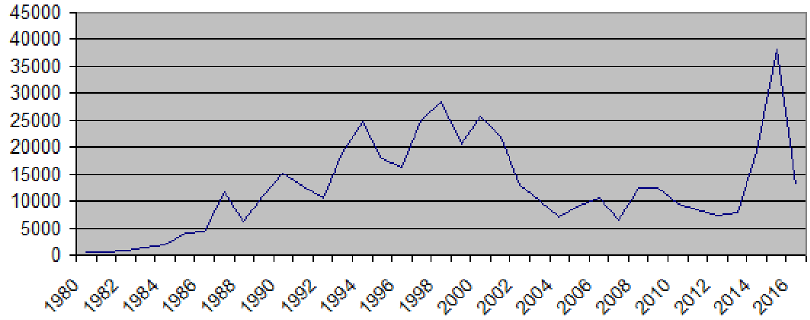

Figure 1: yearly number of first asylum requests made in the Netherlands by non-European people. Source: CBS. URL in appendix 6.2.

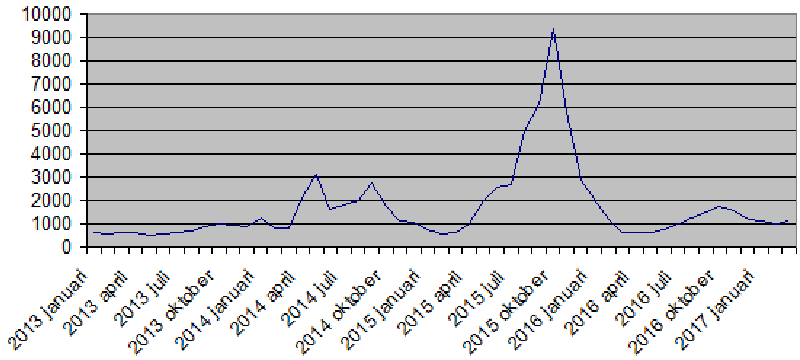

Figure 2: monthly number of first asylum requests made in the Netherlands by non-European people. Source: CBS. URL in appendix 6.2.

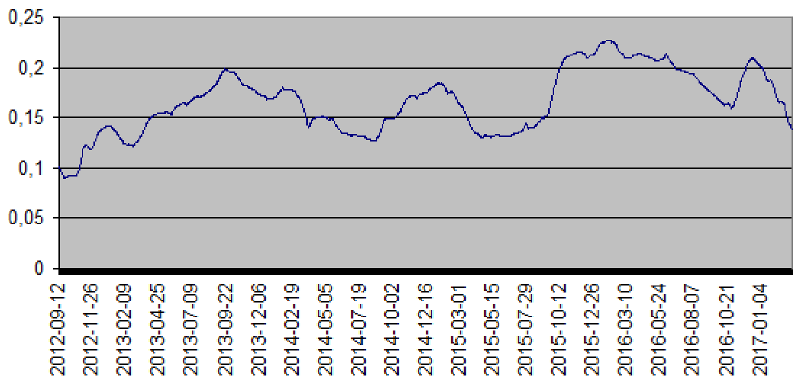

Figure 3: The PVV vote share in the polls. Source: Peilingwijzer. URL in appendix 6.2.

The internal validity of my approach mostly rests upon the common trend assumption, which states that in the absence of the treatment (the opening of (extra) shelter locations), the variable of interest (the local PVV vote share) should have evolved similarly in the treatment group and the control group. Unfortunately, to some extent municipalities were able to determine themselves whether they ended up in the treatment group or in the control group, which gives reason to doubt whether this assumption holds. One could argue that municipalities with a more left-wing climate were more likely to volunteer for a (extra) shelter location, and that the support for right-wing populism may be on a different trend in a left-wing climate than in a right-wing climate.

Placebo tests indicate that in between 2006 and 2012, the treatment group was on a different trend than the control group. This gives a strong indication that the common trend assumption is indeed violated. In order to circumvent this problem, I construct an adjusted control group for which the common trend assumption is less likely to fail. I do so by exploiting the unexpected drop in the inflow of refugees that occurred in 2016. This drop caused many approved plans for shelter locations to be cancelled. In fact, multiple buildings that were built or renovated to accommodate refugees, never housed one refugee in the end. This means that many municipalities that *wanted* to be in the treatment group ended up in the control group for reasons outside their control. I use these municipalities to construct my adjusted control group. They arguably form a better counterfactual than the initial control group, which includes many municipalities that *chose* not to open any (extra) shelter locations.

Placebo tests show that the treatment group and the adjusted control group were on the same trend between 2006 and 2012. This confirms my expectation that the common trend assumption is not violated anymore when the adjusted control group is used. The causal effect of having opened a (extra) shelter location on the local PVV vote share can now be measured without bias.

Theoretically, two outcomes of increased micro-level exposure to refugees can be expected. On the one hand, there is the realistic group conflict theory as referred to by Campbell (1965), which predicts that the arrival of a group of immigrants with a different cultural background leads to anxiety in a community. On the other hand, there is the contact hypothesis (Allport, 1954), which states that micro-level exposure to refugees may diminish anti-immigrant sentiment. Direct encounters with refugees may be positive, which makes the local community less concerned about immigration as compared to people that are only exposed at the macro-level.

My results are somewhat surprising. Contrary to all main related studies, I find a causal effect that is not statistically significant from zero. This result is highly robust to the type of municipalities included in the sample.

The remainder of my thesis is organised as follows. Chapter 2 sheds light on some related studies. In chapter 3, I outline my empirical strategy in more detail. In chapter 4, the results are shown. Chapter 5 concludes.

2 Related studies

An extensive body of economic literature has emerged, trying to explain the surge of right-wing populism in Europe. Several empirical studies focus specifically on the effect of refugee immigration and are therefore closely related to my thesis. I discuss them briefly in this chapter.

2.1 Austria

Steinmayr (2016) considers how the vote share of the FPÖ, the main right-wing populist party in Austria, was impacted by micro-level exposure to refugees, which varied on a local level during the European refugee crisis. In order to solve the endogeneity problem caused by the non-random allocation of refugee shelters to neighbourhoods, he uses pre-existing group accommodations as an instrumental variable. The intuition behind this approach is that neighbourhoods that happened to house large tenantless buildings were much more likely to end up providing shelter to refugees than neighbourhoods that were not, while the availability of vacant buildings is unlikely to be correlated with other factors that may influence the FPÖ vote share. Therefore, Steinmayr argues, such buildings generate exogenous variation in exposure to refugees. Steinmayr finds that the FPÖ vote share decreases by 4.4 percentage points if refugees are present in a local community.

Also Halla et al. (2017) focus on the effect the presence of immigrants has on the support for right-wing populism in Austria. Contrary to Steinmayr (2016), this paper finds that local exposure to immigrants affected the FPÖ vote share positively. First, Halla et al. (2017) use panel data in order to assess the impact of a percentage change in the share of immigrants in the local population on the percentage change of the local FPÖ vote. As a complement, the authors also use historical settlement patterns of immigrants as an instrumental variable. The idea behind this instrument is that areas in which immigrants have always chosen to settle should be more likely to host immigrants today than areas that have not, while the historical immigration patterns are supposed to be uncorrelated with other factors that may influence the local FPÖ vote in the current era. Both the panel data approach and the IV approach show that as the share of immigrants in the local population rises, the local support for the FPÖ increases.

How can these two studies yield such opposite results? First of all, note that Steinmayr looks purely at presence or absence of immigrants, whereas Halla et al. focus at the share of immigrants in the population. Also the type of immigration differs: Steinmayr considers refugee shelter locations, Halla et al. look at actual immigration.

Note that both the identification strategy of Steinmayr (2016) and (especially) the identification strategy of Halla et al. (2017) have their weaknesses. Steinmayr's instrument validity depends on the assumption that the presence or absence of empty buildings in a neighbourhood is not correlated with any other factors that may influence the local FPÖ vote. I would argue that, for example, areas that are experiencing economic downturn are more likely to house redundant spaces than booming cities. Simultaneously, people whose lives are deteriorating may be more likely to turn to right-wing populist parties. Therefore I fear the exclusion restriction may be violated in this IV approach.

In Halla et al. (2017), the instrument validity seems even weaker to me, as it can be argued that the FPÖ vote is definitely influenced when an area houses a lot of descendants of migrants (different mindset, culture, ...). Moreover, the first stage (the effect of historical migration patterns on the likelihood of welcoming immigrants) is weak, as the authors themselves admit. Finally, I feel that Halla et al.'s panel data approach is dodgy as well. The authors fail to eliminate the possibility of reverse causality (immigrants may choose to avoid areas in which the support for right-wing populism is rising) convincingly.

2.2 Germany

Otto & Steinhardt (2014) use a very similar IV approach as Halla et al. to estimate electoral outcomes of exposure to immigrants in the city of Hamburg, Germany. The study focuses on immigrants in the period 1987-1998, a large share of whom were asylum seekers. Otto & Steinhardt find that growing shares of foreigners in a city district have a positive impact on the popularity of right-wing populist parties and a negative impact on the pro-immigration Green party.

2.3 Denmark

Denmark quasi-randomly allocates refugees to municipalities. This policy has been exploited by Dustman et al. (2016) to estimate the impact of refugee immigration on various electoral outcomes. The authors find convincing evidence that on average, the larger the share of refugees allocated to a municipality, the more the support for right-wing populist parties rose, and the more the support for centre-left parties dropped. This conclusion holds for all municipalities but those in the most urbanized areas, for which the impact is exactly the opposite. Barone et al. (2014) draw exactly the same conclusion when considering Italian data (although this study did not focus on refugee immigration but on immigration in general).

Harmon (2015) focuses on a Danish dataset ranging from 1989 to 2001. In this period, allocation of refugees did *not* occur quasi-randomly, but was often dependent on local availability of rental houses. Harmon exploits the historical (1970) stock of rental houses as an instrumental variable. This approach rests upon the assumption that this historical stock only affects decreasing or increasing support for right-wing populism through the refugee channel. Harmon finds that immigration positively influenced the vote shares of right-wing populist parties.

2.4 Greece

Greece held parliamentary elections in January 2015 and in September 2015. In between these elections an enormous flow of refugees arrived on various Greek islands. Seferis & Vasilakis (2016) exploit these events by studying the local change in the support for Golden Dawn, the main right-wing populist party, from the first to the second election. In order to deal with endogeneity concerns, Seferis and Vasilakis use an instrumental variable approach. The instrument used is how many refugees are predicted to settle on a certain island, based on the distance of the island to the nearest Turkish border. This prediction is supposed to be correlated to the actual immigration numbers, whereas it shouldn't influence any other factors that might affect the support for the right-wing populist party. The crucial assumption is that in between the elections, no events took place that might have influenced the islands that are close to the Turkish border in a different way than those that are a bit further away from that border. The authors find that the higher the share of refugees, the more the support for Golden Dawn rose from the first to the second election.

2.5 Conclusion

Most empirical studies tend to conclude that there is a positive impact of micro-level exposure to refugees on the local vote share of right-wing populist parties. Especially Dustman et al.'s identification strategy is convincing. It exploits quasi-random allocation of immigrants and does not rely on instrumental variable approaches of which the underlying assumptions may be violated.

3 Strategy and data

3.1 Identification strategy

Right-wing populist parties in Europe oppose refugee immigration, fearing an increasing share of Muslims in the population, and claiming that the increased supply of low-skilled labour comes at the expense of domestic working classes (Rydgren, 2008; Guiso et al, 2017). How has this particular type of immigration impacted the rise of right-wing populism in the Netherlands?

First of all, note that there probably exists no feasible 'ideal' experiment that can provide unbiased causal estimates of the electoral outcomes of refugee immigration within a country. Even if immigrants were randomly allocated to certain areas, people in other areas would still be affected due to e.g. the fiscal balance and labour market effects of immigration. Using other countries as a counterfactual would be problematic as well due to spill-over effects. People in those countries would learn about the macro-consequences of immigration in the countries that experienced refugee immigration, and adjust their voting behaviour accordingly.

What *can* be estimated without bias, are the electoral outcomes of exposure to refugees at the micro-level: housing refugees in one's local community. Its impact can be regarded as the isolated effect of changing levels of xenophobia in response to direct encounters with refugees. Clearly, when it comes down to micro-level exposure, it is much easier to distinguish between those that are affected by immigration and those that are not.

Still, simply regressing right-wing populist parties' local vote shares on the local population share of refugee immigrants is unlikely to yield unbiased causal estimates. Immigrants do not settle in areas randomly. They tend to choose those locations where they can find work and where they can live near fellow countrymen (Piil Damm, 2007), or where they are less likely to encounter racist and discriminatory practices (Logan et al., 2002). Say the latter factor would be at play, such that immigrants choose to settle in areas with a predominantly left-wing climate. Then, the cross-sectional outcome is likely to suggest that micro-level exposure to refugees leads to a loss of support for right-wing populism, whereas this need not be the case.

In order to deal with this *selection problem*, I exploit the recent European refugee crisis (2013-2016) in a difference-in-difference design. The Dutch asylum procedure generates local variation in micro-level exposure to refugees. Upon arrival, asylum seekers are housed in shelter locations until their asylum request has been accepted or turned down[2], which could take up to 15 months at the peak of the refugee crisis[3]. When turned down, the asylum seeker should leave the Netherlands. When accepted, the asylum seeker is no longer regarded as an asylum seeker but as a status holder. He stays in the shelter until he has been allocated a house in a Dutch municipality. Status holders are spread very evenly across municipalities[4].

The Netherlands use a system with three different types of shelter locations[5]. There are regular shelters, which are in use for a period of at least two years, there are temporary shelters, which are in use for a period of up to two years, and there are emergency shelters. These locations are usually in use for a period of up to six days. De facto, regular and temporary shelters are very similar in terms of capacity and impact on the local community[6].

In 2012, only regular shelters were in place. In order to cope with the large inflow of refugees in 2015, about 75 temporary shelters were opened and the number of regular shelters was more than doubled, to over 70[7]. In 2015, many 'emergency shelters' were opened as well[8]. These were used to house asylum seekers until a place had been found for them in a regular or temporary shelter. Most temporary shelters were closed by 2017, when the inflow of asylum seekers had dropped sufficiently such that the regular shelters were capable of doing the job on their own[9].

In my difference-in-difference model, I exploit the local variation in micro-level exposure to refugees that this asylum procedure generates. I use the last general elections before the refugee crisis (2012) as the pre-treatment moment and the first general elections after the refugee crisis (2017) as the post-treatment moment. I use the municipality-level vote share of right-wing populist party PVV as the dependent variable. I use all Dutch municipalities that opened a (extra) regular shelter in between 2012 and 2017 as a treatment group, together with all municipalities that opened a (extra) temporary shelter. I use all remaining Dutch municipalities as a control group. Hence, I use the opening of a (extra)

[2] See e.g. https://www.coa.nl/nl/asielopvang
[3] See e.g. https://www.volkskrant.nl/nieuws-achtergrond/asielprocedure-kan-nu-vijftien-in-plaats-van-zes-maanden-duren~b25501a8/
[4] See e.g. https://www.rijksoverheid.nl/onderwerpen/asielbeleid/huisvesting-asielzoekers-met-verblijfsvergunning
[5] See e.g. https://www.coa.nl/nl/actueel/veelgestelde-vragen/opvangvormen
[6] See e.g. https://www.nrc.nl/nieuws/2016/02/02/dit-zijn-de-feiten-over-asielzoekers-in-nederland-a1405200
[7] Source: see appendix 6.2.
[8] https://www.coa.nl/nl/actueel/nieuws/laatste-crisisnoodopvang-gesloten
[9] See e.g. https://nos.nl/artikel/2121283-coa-sluit-noodopvanglocaties-voor-asielzoekers.html

regular or temporary shelter as a proxy for increased micro-level exposure to refugees. I ignore emergency shelters as these have affected the local population for a very short period only. I also ignore so-called small-scale shelters that typically house just a few dozens of refugees[10]. I ignore exposure to status holders as well, since they are spread very evenly across municipalities. Finally, note that I allocate municipalities that housed one regular shelter both in 2012 and in 2017 and did not open a temporary shelter location are allocated to the control group. One could argue this is incorrect, as the local population may have experienced an increase in micro-level exposure to refugees due to a rising occupancy rate in these shelters during the refugee crisis. However, COA stick to a reserve capacity of just 15 to 20%[11]. This implies that rising occupancy rates during the refugee crisis have arguably resulted in just a tiny increase in micro-level exposure to refugees as compared to places where completely new (extra) shelter locations were opened. This justifies my choice to allocate municipalities with existing shelter locations (but no new ones) to the control group.

In summary, using the opening of a (extra) regular or temporary shelter location as a cut-off condition should ensure that only municipalities that experienced a substantial and prolonged increase in the number of refugees housed are included in the treatment group.

3.2 The model

The difference-in-difference model compares the average local PVV vote share at the 2012 general elections with the average local PVV vote share at the 2017 general elections for treated and untreated municipalities. The causal difference-in-difference estimator, represented by δ in the equation below, yields the impact of having opened a (extra) regular or temporary shelter location in a municipality in between 2012 and 2017 on the PVV vote share in that municipality, as compared to a municipality that did not open any (extra) shelter locations.

$$PVV_{it} = \alpha_i + \gamma_t + \delta * Treatment_i * Time_t + X_{it}\Gamma + u_{it} \quad (1)$$

PVV_{it} stands for the PVV vote share in municipality i at time t. α_i captures time-invariant differences across municipalities. γ_t captures municipality-invariant differences across time. $Treatment_i$ takes on the value 0 if the municipality is in the control group and the

[10] https://www.coa.nl/nl/opvanglocaties/typen-locaties
[11] https://www.coa.nl/nl/actueel/veelgestelde-vragen/opvangcapaciteit-2018

value 1 if the municipality is in the treatment group. $Time_t$ takes on the value 0 when pre-treatment and the value 1 when post-treatment. $X_{it}\Gamma$ is a vector of control variables. u_{it} is the error term. Standard errors are clustered at the municipality level.

Control variables should be factors other than the treatment itself that may explain the PVV vote share. If one of these factors has evolved differently in the treated municipalities than in the untreated municipalities, this may be the cause of any divergence in the average PVV vote share, instead of the treatment. Therefore, these factors must be controlled for in the estimations. In the literature, six commonly cited reasons why people vote for right-wing populist parties are Islamization, xenophobia, free movement of labour in the EU, trade globalization, economic downturn and crime/insecurity (Rydgren, 2008; Guiso et al., 2017). For Islamization/xenophobia, I use the population share of Moroccan[12] immigrants and the population share of non-Western immigrants as proxies. For free movement of labour, I use the population share of EU immigrants and the population share of Polish[13] immigrants. For trade globalization, I use local exposure to manufacturing imports from China (see appendix 6.5 for details on this measure). For economic downturn, I use the unemployment rate. For crime/insecurity, I use the number of registered crimes per inhabitant.

3.3 Subsamples

I estimate equation (1) again using several subsamples, in order to find out whether the results are robust to the type of municipalities included in the sample.

As mentioned in the related literature chapter, research with Danish and Italian data found that estimated effects are very different in the most urbanized areas as compared to the country as a whole, and therefore I create one subsample consisting of the largest 50 municipalities in terms of inhabitants (as of 2017).

Also, it could be argued that the impact of increased micro-level exposure to refugees may be different in a sample consisting of municipalities that are characterised by economic downturn, because concerns about the labour market effects of immigration are likely to be

[12] Moroccans form one of the major ethnic minorities in the Netherlands and the PVV has a strong dislike against them, see e.g. https://www.youtube.com/watch?v=DIOttM7_rNQ. That is why I choose to highlight this specific group of immigrants.

[13] The thing right-wing populist parties dislike most about free movement of labour is the inflow of low-skilled labourers who compete with native working classes on the labour market, see e.g. https://www.express.co.uk/comment/expresscomment/681776/nigel-farage-eu-referendum-brexit-vote-leave-independence-ukip. Especially Poland is a huge supplier of low-skilled labour to the Dutch market, see e.g. https://www.volkskrant.nl/binnenland/er-zijn-nu-meer-polen-dan-belgen-in-nederland~a4318971/, and therefore I choose to highlight this specific group of immigrants.

particularly predominant in these areas. Therefore I also create one subsample consisting of the 47[14] municipalities with the highest unemployment rate (as of 2017).

I create one subsample consisting of the 50 municipalities with the lowest population share of non-Western immigrants (as of 2012[15]), because if the estimated causal effect can indeed be seen as the impact of changing levels of xenophobia, as speculated upon earlier, the effect may be very different in areas where people are not used to ethnic diversity at all.

Finally, I create four subsamples based on geography. One consisting of Randstad municipalities (all municipalities in the provinces of Zuid-Holland, Noord-Holland, Flevoland and Utrecht), one of northern municipalities (all municipalities in the provinces of Groningen, Drenthe and Friesland), one of eastern municipalities (all municipalities in the provinces of Gelderland and Overijssel) and one of southern municipalities (all municipalities in the provinces of Noord-Brabant, Zeeland and Limburg).

3.4 Assumptions

My difference-in-difference strategy rests upon three assumptions. If they are satisfied, δ can be interpreted as the unbiased causal estimate of opening a (extra) shelter location in between 2012 and 2017.

First, there is the common trend assumption, which states that for a difference-in-difference approach to yield an unbiased effect, initial differences between treatment and control group should have stayed constant had there been no treatment. That means, both groups should be on the same 'trend' with regard to support for right-wing populism. Whether this assumption holds is likely to depend on the allocation procedure of shelter locations. The allocation need not be random; a difference-in-difference model allows group level omitted variables to be captured by group level fixed effects. However, the allocation does have to be exogenous to the municipalities. Otherwise, municipalities that are on a different 'trend' than other municipalities might have selected into the treatment. So was it exogenous?

In 2015, COA, the organisation that is responsible for refugee shelters, asked all Dutch municipalities to investigate whether a shelter location, either regular or temporary, was

[14] Those ranked from 48 to 55 had the same unemployment rate in 2016 so I have to choose a different cut-off point here.

[15] I use 2017 for unemployment: if one suspects labour market concerns to play a role in the electoral outcome of exposure to refugees, the unemployment rate at the post-treatment elections are obviously more relevant than the rate at the pre-treatment regressions. However, for non-Western immigrants, the 2012 value seems more relevant as this is about whether people were already used to ethnic diversity *before* the treatment started.

feasible on their territory[16]. The municipalities then searched for suitable buildings, in cooperation with COA[17]. City councils had the final say in whether or not to offer one of those locations to COA. When the council approved the plans, COA was enabled to house asylum seekers in the building. Not all municipalities opened a (extra) shelter location. For example because no suitable locations were found[18], because the city council didn't vote in favour[19], or because COA in the end did not make use of the offered buildings[20].

This procedure obviously means that municipalities had a say in whether they would end up in the treatment group or in the control group. It therefore seems likely that municipalities with a more left-wing climate were more likely to open a (extra) shelter location than municipalities with a more right-wing climate, either because of the political preference of city councils, or because of resistance from the local population[21]. Data suggest this may indeed be the case: as compared to the control group, in the treatment group, the average municipality-level PVV vote share in 2006, 2010 and 2012 was 0.5, 1.5 and 0.9 percentage points lower respectively (see table 1 at the end of this chapter). This gives reason to doubt whether the common trend assumption holds. The support for right-wing populism might, in the absence of the treatment (the refugee crisis), have evolved differently in a left-wing climate than in a right-wing climate in between 2012 and 2017. We are talking a five year period. Many other developments rather than just the refugee crisis may have influenced the support for right-wing populism during these years, and possibly in a different way in a right-wing climate than in a left-wing climate. A mentioned earlier, I add a rich set of time-varying control variables to the estimation equation. However, there may still remain unobserved time-varying differences between the treatment group and the control group.

In order to gain more insight into whether or not the common trend assumption holds, I look at the trend of the variable interest in additional periods before the treatment took place. The PVV also participated in the 2006 and 2010 general elections. If the average PVV vote share in the treatment municipalities followed a different trend between 2006 and 2012 than the average PVV vote share in the control municipalities, it is unlikely that in the absence of

[16] See e.g. https://www.coa.nl/nl/actueel/nieuws/oproep-bestuursvoorzitter-bakker-om-nieuwe-locaties

[17] See e.g. https://www.coa.nl/sites/www.coa.nl/files/paginas/media/bestanden/een_asielzoekerscentrum_in_de_gemeente_facsheet_van_idee_naar_azc_0.pdf

[18] See e.g. https://renkum.nieuws.nl/nieuws/2098/mogelijk-kleinschalige-asielopvang-in-renkum/

[19] See e.g. https://nos.nl/artikel/2111331-heftig-protest-tegen-opvang-asielzoekers-heeft-dat-effect.html

[20] See e.g. https://www.limburger.nl/cnt/dmf20170904_00045943/limburgse-gemeenten-eisen-geld-asielopvang-terug

[21] Whether or not to open a shelter location was hotly debated in many municipalities and in some cases the issue even lead to riots on the streets, see e.g. https://nos.nl/artikel/2110770-celstraffen-voor-azc-rellen-geldermalsen.html

the refugee crisis the average vote shares would have perfectly co-moved in between 2012 and 2017.

To test whether the trends co-moved in between 2006 and 2012, I perform placebo tests (controls included) on all 8 samples, both for the 2006-2010[22] jump and for the 2010-2012 jump. The causal estimate of being a treatment group municipality should be statistically insignificant in all 16 cases, as the treatment only took place after 2012. If some estimates *are* statistically significant, this gives a strong indication that the common trend assumption does not hold, as unobserved differences between the two groups seem to play a role in the trend of the PVV vote share.

As I will show in the next chapter, some of the placebo tests yield statistically significant values. In order to deal with this problem, I construct an adjusted control group, only consisting of those municipalities that wanted to be in the treatment group, but ended up in the control group for a reason outside their control. The unexpected drop in the inflow of refugees in 2016 caused many approved plans for shelter locations to be cancelled by COA, because the other shelters were sufficient to cope with the decreased inflow of refugees[23]. When using only the municipalities for which this has been the case as a control group, the variation in increased micro-level exposure to refugees is arguably determined much more exogenously than when using all non-treated municipalities as a control group, and therefore, the common trend assumption should be much more likely to hold.

A total of 48 municipalities committed to opening a (extra) shelter location, but in the end did not because the inflow of refugees in 2016 appeared to be much lower than foreseen[24]. The data suggest that in the new sample, the allocation of shelter locations indeed depended much less on the political climate (right-wing or left-wing) in the municipalities: contrary to the previous sample, now, at the 2006, 2010 and 2012 elections, the average municipality-level PVV vote share was extremely similar in the treatment municipalities and the control municipalities (see table 2 at the end of this chapter).

I estimate equation (1) again and also this time I consider robustness to the type of municipality included, by using the four geographic subsamples and the three demographic

[22] Unfortunately, no municipality-level registered crime data of periods prior to 2010 are publicly available, so in the 2006-2010 placebo tests, only six out of the seven controls are used.
[23] See e.g. https://www.limburger.nl/cnt/dmf20170904_00045943/limburgse-gemeenten-eisen-geld-asielopvang-terug
[24] Source: see appendix 6.2.

subsamples (even though I will now include just 30[25] municipalities instead of 50 in the demographic ones, as the sample has become much smaller). Again, I perform placebo tests on all eight samples in order to test whether the common trend assumption holds.

The second assumption of my difference-in-difference model is that there were no anticipatory effects at the 2012 elections. That is, it mustn't be the case that the treatment has influenced people's voting behaviour in 2012. This assumption would be violated if in 2012, people in municipalities that would later end up in the treatment group foresaw the opening of a (extra) shelter location near their homes and increased their support for the PVV straight away, because of their fear of having a (extra) shelter location. Then, the estimated impact of having opened a (extra) shelter location would be underestimated.

I do not believe this is really a threat to my identification strategy. First of all, the refugee crisis had not started yet in 2012. Refugee immigration was not a major political issue at the 2012 Dutch general elections[26]. Moreover, even if people expected a large inflow of refugees, this would only lead to a violation of the anticipation assumption if they also knew in which municipalities this inflow would lead to the opening of a (extra) shelter location. There is little reason to believe this is the case, as most plans for new shelter locations weren't made before COA's call in 2015. One could argue that people might have suspected whether or not their municipality council would open a (extra) shelter location in the event of a refugee crisis, but altogether, this seems a far-fetched theory. What's more, there were municipality elections in 2014, so councils were different in 2015 than they were in 2012.

The third assumption is that migration patterns between municipalities in between the 2012 and 2017 elections are uncorrelated to voting behaviour. The biggest concern in this respect is that PVV voters may have moved away from municipalities where a (extra) shelter location was opened. In that case, any rising local support for right-wing populism due to the opening of a (extra) shelter location may be disguised in the estimation results.

In order to gain some insight into whether or not the migration assumption holds, I look at migration patterns between municipalities. More specifically, for each of the 380 municipalities in my database I collect the share of people that moved to another municipality in each year from 2011 to 2016. If the average among the treatment group municipalities suddenly rises in 2015 or 2016 as compared to the average among the control group

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[25] And in the case of the 'many inhabitants' subsample, not 30 but 35, because there are just two non-treated municipalities among the top 30 municipalities in this sample. This could have led to issues with correct statistical inference.

[26] See e.g. https://m.sussex.ac.uk/webteam/gateway/file.php?name=epernnetherlands2012.pdf&site=266

municipalities, that would suggest that the migration assumption is violated. The data are shown in appendix 6.6 and provide no strong indication that this is the case.

A second way to gain insight into whether or not the migration assumption holds is to check whether the change from 2012 to 2017 in the demographic composition of the group of people living in treatment group municipalities has been similar to the change in the demographic composition of the group of people living in control municipalities. If for some characteristics, the evolution between 2012 and 2017 is very different between the two groups, this would give an indication that certain types of people moved from one group to the other. This would then give reason to doubt whether the migration assumption holds. The comparison is presented in appendix 6.6 and provides little evidence that this is the case.

3.5 Data

My database contains information on 380 out of the 388 municipalities that the Netherlands existed of on 15 March 2017, the day the 2017 general elections took place.

During the period 2006-2017 several dozens of municipalities merged. For example, during the 2006, 2010 and 2012 elections, the current municipality of Oldambt existed of three separate municipalities: Scheemda, Winschoten and Reiderland. In order to solve for these issues, I manually calculate (weighted average) what would have been the PVV vote share in these years (and what would have been the value of all other municipality characteristics that are used in my thesis) had these municipalities already merged at the time of earlier elections. A full overview of all mergers can be found in appendix 6.1.

Unfortunately, in three cases a municipality was cut into several parts, which were then allocated to different, existing municipalities. In between the elections of 2010 and 2012, the municipality of Boornsterhem was cut into four parts. The municipalities of Heerenveen, Leeuwarden, Sudwest Fryslan and De Fryske Marren each took one part. No voting shares for these four separate parts are available. Therefore I delete these four municipalities from my database. In between the elections of 2012 and 2017, the municipality of Maasdonk was cut into two parts. Den Bosch and Oss each took one part. No voting shares for these two separate parts are available. Therefore I drop these two municipalities. Also, the municipality of Meerlo-Wansum was cut into two parts. Venray and Horst aan de Maas each took one part. I delete these as well, so I end up with 380 observations: the 388 municipalities that the Netherlands exists of as of 2017, minus the eight municipalities for which accurate data are not available.

The PVV vote share is defined as the number of PVV votes as a fraction of the total number of votes in a municipality. Data for the 2006, 2010 and 2012 elections are collected from the Kiesraad database. Data for the 2017 election are collected from the Algemeen Dagblad website. All municipality mergers are traced from the yearly CBS (National Dutch Statistics Bureau) reports on regional demographics. All data on other municipality characteristics (inhabitants, migration patterns, demographics and all control variables) are collected from the CBS online database. URLs to all data sources can be found the appendix.

COA only provide an overview of 'regular' shelter locations on their website, including their opening and closure years. In order to trace the locations of 'temporary' shelters, and to trace the municipalities that would have opened a shelter location had there been no sudden drop in refugee inflow, I use numerous individual sources. More details on this can be found in appendix 6.2.

A summary of the data can be found in tables 1 and 2 below, where table 1 summarizes the full database and table 2 the database with the adjusted control group.

	Full sample	Many inhabi-tants sample	High unem-ployment rate sample	Few non-Western immi-grants sample	Nor-thern sample	Rand-stad sample	Sou-thern sample	Eastern sample
Municipalities	380	50	47	50	55	140	106	79
Of which treated	99	32	24	6	18	37	21	23
Average PVV vote share 2006 – treatment group	5.3	5.8	5.2	5.1	3.5	5.7	7.6	4.0
Average PVV vote share 2010 – treatment group	14.5	14.9	14.9	15.0	12.1	13.9	19.9	12.4
Average PVV vote share 2012 – treatment group	9.4	9.9	10.0	9.3	7.6	9.2	12.8	8.0
Average PVV vote share 2017 – treatment group	12.5	12.7	13.8	12.4	12.1	11.8	15.8	11.1
Average PVV vote share 2006 – treatment group	5.8	7.9	6.4	4.6	3.5	6.2	7.5	4.3
Average PVV vote share 2010 – treatment group	16.0	19.4	18.7	14.1	12.0	15.4	19.7	14.0
Average PVV vote share 2012 – control group	10.3	13.4	12.5	8.9	7.6	10.0	12.7	8.8
Average PVV vote share 2017 – control group	13.6	16.4	16.5	12.5	11.9	13.0	15.9	12.2

Table 1: Descriptive data. Full control group.

	Full sample	Many inhabi-tants sample	High unem-ployment rate sample	Few non-Western immi-grants sample	Nor-thern sample	Rand-stad sample	Sou-thern sample	Eastern sample
Municipalities	147	35	30	30	32	50	34	31
Of which treated	99	30	24	15	18	37	21	23
Average PVV vote share 2006 – treatment group	5.3	5.9	5.2	4.1	3.5	5.7	7.6	4.0
Average PVV vote share 2010 – treatment group	14.5	14.9	14.9	12.6	12.1	13.9	19.9	12.4
Average PVV vote share 2012 – treatment group	9.4	9.9	10.0	7.8	7.6	9.2	12.8	8.0
Average PVV vote share 2017 – treatment group	12.5	12.6	13.8	11.1	12.1	11.8	15.8	11.1
Average PVV vote share 2006 – treatment group	5.3	7.9	4.9	4.8	3.6	6.9	6.4	3.8
Average PVV vote share 2010 – treatment group	14.8	19.4	14.6	13.7	12.1	16.2	17.7	12.8
Average PVV vote share 2012 – control group	9.5	13.0	9.5	8.6	7.7	11.0	11.0	7.8
Average PVV vote share 2017 – control group	13.0	16.7	13.4	12.4	11.9	14.3	14.4	10.5

Table 2: Descriptive data. Adjusted control group.

4 Results

4.1 Full control group

Table 3 shows the estimation results for the full sample, both with and without control variables. The difference-in-difference estimator is represented by δ. In the model without controls, I find a value of -0.15. When controls are added, the estimate rises to -0.14. The estimate should be interpreted as saying that the predicted increase in the local PVV vote share from 2012 to 2017 decreases by 0.14 percentage points when the municipality in question opened a (extra) refugee shelter location in between the elections, taking into account the time-varying covariates included. However, for both models the estimate is not statistically significant at the 10% level. Hence, no indication is found that the effect of micro-level exposure to refugees on the support for right-wing populism is significantly different from zero.

	Full sample	Full sample
δ	-0.15 (0.35)	-0.14 (0.43)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.85	0.85
Observations	760	760

Table 3: The difference-in-difference estimator of the treatment effect on the local PVV vote share - full sample. P-value in parentheses.

Table 4 shows the estimation results for the subsamples based on demographic characteristics. In all models, the estimate is negative, which again suggests that micro-level exposure to refugees decreases the support for right-wing populism. Among the controlled models, the highest estimate is found in the 'many inhabitants sample', which suggests that in the most urbanised areas, people had a more dismissive response to the opening of a (extra) shelter location as compared to people in other areas. However, in all models, the causal estimates are statistically insignificant at the 10% level, so again no evidence is found of any impact of the opening of a (extra) shelter location.

	Many inhabi-tants sample	Many inhabi-tants sample	High unem-ployment rate sample	High unem-ployment rate sample	Few non-Western immi-grants sample	Few non-Western immi-grants sample
δ	-0.25 (0.48)	-0.06 (0.87)	-0.25 (0.68)	-0.24 (0.66)	-0.50 (0.12)	-0.44 (0.32)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.83	0.85	0.79	0.85	0.90	0.91
Observations	100	100	94	94	100	100

Table 4: The difference-in-difference estimator of the treatment effect on the local PVV vote share - demographic subsamples. P-value in parentheses.

In table 5 the results are shown for the subsamples based on geography. The eastern model with controls yields an estimate that is significant at the 5% level. The estimate of -0.44 suggests that the opening of a (extra) shelter location caused a decrease in the support for right-wing population in the eastern part of the Netherlands. All other controlled models yield estimates that are statistically insignificant at the 10% level.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
Δ	-0.39 (0.03)	-0.10 (0.53)	0.17 (0.75)	-0.14 (0.78)	-0.24 (0.28)	-0.44 (0.03)	-0.20 (0.54)	-0.07 (0.83)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.91	0.93	0.85	0.88	0.92	0.93	0.82	0.86
Observations	280	280	110	110	158	158	212	212

Table 5: The difference-in-difference estimator of the treatment effect on the local PVV vote share - geographic subsamples. P-value in parentheses.

Polls show that the popularity of the PVV rose enormously during the refugee crisis, which makes it plausible that there is a positive impact of refugee immigration on the support for right-wing populism in the Netherlands. However, the results in table 3, 4 and 5 suggest that there is little evidence of an *additional* effect of *micro-level* exposure to refugees. When covariates are controlled for, only in one of the eight samples the average municipality-level PVV vote share diverged in municipalities that opened a (extra) refugee shelter as compared to municipalities that did not.

However, whether the estimates are unbiased depends on the common trend assumption. As argued in the previous chapter, this assumption may not hold, for example because municipalities with a left-wing climate were more likely to open a (extra) shelter location as compared to municipalities with a right-wing climate. Placebo tests confirm my concern. The p-values of the 16 main tests are shown in table 6 (full results can be found in appendix 6.3). Eight of them are significant at the 5% level, including the two tests related to the eastern sample. This makes it highly unlikely that the statistically significant effect in the eastern model in table 5 is unbiased. Remember that all estimates should be statistically insignificant, as the treatment only took place after 2012.

	Full sample	Many inhabitants sample	High unemployment rate sample	Few non-Western immigrants sample	Northern sample	Randstad sample	Southern sample	Eastern sample
2006-2010 with controls	0.04	0.01	0.01	0.61	0.66	0.00	0.58	0.01
2010-2012 with controls	0.11	0.02	0.06	0.61	0.91	0.00	0.50	0.02

Table 6: P-values associated with all placebo tests.

Only in the few non-Western immigrants, northern and southern samples, no evidence is found that the common trend assumption is violated. None of the six placebo tests associated with these three samples yields a statistically significant value. What's more, all p-values are at least 0.50, which provides a strong indication that in these three samples, the control group and the treatment group were on the same trend between 2006 and 2012. This implies that the corresponding estimated causal effects in table 4 and 5 are not necessarily biased, despite the theoretical concerns. Recall that both for the controlled and for the uncontrolled models of each of these three samples, the causal estimate δ was not statistically significant. Hence, this suggests that the opening of a (extra) shelter location did not affect voting behaviour at the 2017 elections in these three subsamples. That is the only conclusion that can be drawn so far, since in all other samples, the treatment group and the control group were clearly on a different trend in between 2006 and 2012 (taking into account the time-varying covariates included). Therefore, in these samples, it is unlikely that in the absence of the treatment, the average local PVV vote share would have evolved similarly in both groups (taking into account the time-varying covariates included). Consequently, the corresponding estimates in table 3, 4 and 5 cannot be said to be unbiased, and the initial conclusions drawn from these estimates do not stand. In fact, no conclusion can be drawn at all with regard to these samples.

4.2 Adjusted control group

As argued in the previous chapter, the violation of the common trend assumption may be overcome by using an adjusted control group consisting of those municipalities that committed to opening a (extra) shelter location, but in the end did not because of the sudden drop in the inflow of refugees in 2016. Tables 7, 8 and 9 show the estimation results using this adjusted sample.

	Full sample	Full sample
Δ	-0.34 (0.14)	-0.27 (0.28)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.86	0.87
Observations	294	294

Table 7: The difference-in-difference estimator of the effect on the local PVV vote share - full sample. P-value in parentheses. Adjusted control group.

	Many inhabitants sample	Many inhabitants sample	High unemployment rate sample	High unemployment rate sample	Few non-Western immigrants sample	Few non-Western immigrants sample
Δ	-0.98 (0.04)	-0.69 (0.26)	-0.16 (0.86)	0.11 (0.91)	-0.45 (0.26)	-0.38 (0.50)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.81	0.83	0.78	0.86	0.92	0.93
Observations	70	70	60	60	60	60

Table 8: The difference-in-difference estimator of the effect on the local PVV vote share - demographic subsamples. P-value in parentheses. Adjusted control group.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
Δ	-0.67 (0.04)	-0.44 (0.15)	0.29 (0.64)	-0.02 (0.98)	0.43 (0.27)	0.05 (0.88)	-0.43 (0.27)	0.27 (0.51)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.90	0.94	0.87	0.93	0.93	0.96	0.88	0.92
Observations	100	100	64	64	62	62	68	68

Table 9: The difference-in-difference estimator of the effect on the local PVV vote share - geographic subsamples. P-value in parentheses. Adjusted control group.

Like previously, most of the causal estimates are insignificant. Only the Randstad and the many inhabitants samples yield a value that is significant at the 10% level, but both turn insignificant once controls are added. The results in tables 7, 8 and 9 suggest that micro-level exposure to refugees does not affect the support for right-wing populism. Again, whether the estimates are unbiased depends on the common trend assumption. Does it hold now? This time, placebo tests provide little evidence it does not. P-values of the 16 main tests are shown in table 10. Full output can be found in appendix 6.4. Only one of the tests yields a value that is statistically significant at the 10% level. Together with the theoretical arguments in the previous chapter, this provides a strong indication that the common trend assumption is not violated in the adjusted sample. Hence, apart from those associated with the Randstad sample (for which one placebo test yields a statistically significant value), there is little evidence that the estimates in tables 7, 8 and 9 cannot be interpreted as unbiased causal effects. The data point to the final conclusion that the opening of a (extra) shelter location in a municipality had no effect on the local support for right-wing populism. This conclusion is highly robust to the type of municipalities included in the sample.

	Full sample	Many inhabitants sample	High unemployment rate sample	Few non-Western immigrants sample	Northern sample	Randstad sample	Southern sample	Eastern sample
2006-2010 with controls	0.64	0.12	0.69	0.69	0.89	0.00	0.17	0.14
2010-2012 with controls	0.58	0.24	0.18	0.17	0.58	0.22	0.45	0.13

Table 10: P-values associated with all placebo tests - adjusted control group.

5 Conclusion

In my thesis I attempt to estimate the causal impact of micro-level exposure to refugee immigrants on the support for right-wing populism in the Netherlands. In order to circumvent the selection problem caused by the non-random dispersion of refugee immigrants, I exploit the recent refugee crisis (2013-2016) in a difference-in-difference design. During the crisis, 99 out of the 380 Dutch municipalities in my database opened a (extra) refugee shelter location. I use them as a treatment group, hence using the opening of a (extra) shelter location in a municipality as a proxy for an increase in micro-level exposure to refugees. The remaining municipalities did not open any (extra) shelter locations. I use them as a control group. I use the municipality-level PVV vote share at the 2012 general elections as a measure of pre-treatment support for right-wing populism, and the municipality-level PVV vote share at the 2017 general elections as a measure of post-treatment support for right-wing populism.

As the allocation process of (extra) shelter locations was not exogenous to municipalities, the common trend assumption upon which my difference-in-difference design rests is likely to be violated. Municipalities with a left-wing climate may have been more likely to open (extra) shelter locations than municipalities with a right-wing climate, and in the absence of (extra) shelter locations, in between 2012 and 2017 the support for right-wing populism may have evolved differently in a left-wing climate than in a right-wing climate. Placebo tests confirm that the common trend assumption is violated. In order to deal with this problem, I construct an adjusted control group consisting of municipalities that committed to opening a (extra) shelter location, but in the end did not because of a sudden drop in the inflow of asylum seekers in 2016. This control group arguably forms a better counterfactual than the initial control group, that also includes municipalities that deliberately avoided opening a (extra) shelter location. Placebo tests show that when the adjusted control group is used, there is indeed little evidence that the common trend assumption is still violated.

Estimation results using the adjusted control group indicate that opening a (extra) shelter location in a municipality had no impact on the local PVV vote share. When controls are included, the causal estimate is not statistically significant from zero. This result is highly robust to the type of municipalities included in the sample. My result stands in sharp contrast with earlier literature that looks at electoral outcomes of exposure to refugee immigrants in other European countries. All main studies find either a statistically significant positive or a statistically significant negative impact of exposure to refugee immigration on right-wing populist parties' vote shares.

A limitation of my work is that the opening of a (extra) shelter location may not be a very accurate proxy for increased micro-level exposure to refugees. First of all, it does not consider the actual *number* of refugees present in each municipality in each year. This implies that within the treatment group, the increase in micro-level exposure to refugees (defined as the number of refugees as a share of the municipality population size) may vary. What's more, in some of the control group municipalities, the number of refugees housed has risen somewhat due to a rising occupancy rate in pre-existing shelter locations. Lastly, it would be more accurate to use zip code-level data, as in some cases, areas may have been allocated to the treatment group while in fact, they have hardly been affected by a shelter location that was opened e.g. at the other end of their municipality. In other cases, areas might have been allocated to the control group while in fact they *have* been affected by a shelter location that was opened very close to their homes, just across the municipality border. Unfortunately, neither the number of refugees present in each shelter location in each year nor zip code-level election results were publicly available at the time of writing this thesis.

I convincingly show that the estimated effect of opening a (extra) shelter location is unbiased. However, if one wants to interpret my results as telling the effect of increased micro-level exposure to refugees, one should keep in mind that the above-described measurement error in increased micro-level to refugees may have led to a bias towards zero in the estimations. It is my hope that future research with more detailed data will be able to overcome this problem.

6 Appendix

6.1 Municipality mergers

In between the 2006 and 2010 elections, the following municipality mergers took place:

Former municipalities	New municipality
Ter Aar, Nieuwkoop, Liemeer	Nieuwkoop
Bloemendaal, Bennebroek	Bloemendaal
Roerdalen, Ambt Montfort	Roerdalen
Arcen en Velden, Venlo	Venlo
Binnenmaas, 's-Gravendeel	Binnenmaas
Rotterdam, Rozenburg	Rotterdam
Bergschenhoek, Bleiswijk, Berkel en Rodenrijs	Lansingerland
Heel, Maasbracht, Thorn	Maasgouw
Scheemda, Winschoten, Reiderland	Oldambt
Alphen aan den Rijn, Boskoop, Rijnwoude	Alphen aan den Rijn
Moordrecht, Nieuwerkerk aan den IJssel, Zevenhuizen-Moerkapelle	Zuidplas
Kessel, Helden, Meijel, Maasbree	Peel en Maas
Haelen, Heythuysen, Hunsel, Roggel en Neer	Leudal
Alkemade, Jacobswoude	Kaag en Braassem
Obdam, Wester-Koggenland	Koggenland
Noord-Koggenland, Wognum, Medemblik	Medemblik

In between the 2010 and 2012 elections, the following municipality mergers took place:

Former municipalities	New municipality
Eijsden, Margraten	Eijsden-Margraten
Abcoude, De Ronde Venen	De Ronde Venen
Wieringen, Wieringermeer, Anna Paulowna, Niedorp	Hollands Kroon
Millingen, Ubbergen, Groesbeek	Berg en Dal
Bodegraven, Reeuwijk	Bodegraven-Reeuwijk
Nederlek, Ouderkerk, Vlist, Bergambacht, Schoonhoven	Krimpenerwaard
Loenen, Maarssen, Breukelen	Stichtse Vecht
Medemblik, Andijk, Wervershoof	Medemblik

In between the 2012 and 2017 elections, the following municipality mergers took place:

Former municipalities	New municipality
Zijpe, Harenkarspel, Schagen	Schagen
Sint-Oedenrode, Veghel, Schijndel	Meierijstad
Goedereede, Dirksland, Middelharnis, Oostflakkee	Goeree-Overflakkee
Spijkenisse, Bernisse	Nissewaard
Bussum, Naarden, Muiden	Gooise Meren
Graafstroom, Liesveld, Nieuw-Lekkerland	Molenwaard
Zeevang, Edam-Volendam	Edam-Volendam
Schermer, Graft-De Rijp, Alkmaar	Alkmaar

6.2 Data sources

PVV poll results shown in figure 3 (chapter 2):

https://d1bjgq97if6urz.cloudfront.net/Public/Peilingwijzer/20170314/Peilingwijzer+2012-2017.html

Municipality-level PVV vote shares 2017:

https://www.ad.nl/politiek/bekijk-hier-de-uitslagen-landelijk-of-per-gemeente~aa11d305/

Municipality-level PVV vote shares 2006, 2010, 2012:

https://www.verkiezingsuitslagen.nl/

Municipality-level registered crimes:

http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=83648ned&D1=0&D2=0&D3=19-22,24-35,38-52,54-59,61-69,72-74,76-78,80-82,84-90,92-106,108-123,125-135,137,139-145,147-152,155-156,158-170,172-228,230-231,233-235,237-238,240-243,246-249,251-254,256-259,261-263,265-269,272,274-275,277-278,281-302,304-311,313-321,323-330,332-338,340-342,344-348,350-355,357-360,362-364,366-385,387-408,410-422,424-430,433-443,446-449,451-453,455-464&D4=0,2,l&HDR=T,G3&STB=G2,G1&VW=T

Municipality-level inhabitants and Polish, EU, Moroccan, non-Western immigrants 2006, 2010, 2012, 2016:

http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=37713&D1=0&D2=0&D3=4,10,36,40&D4=57-60,64,66-67,69-72,74,77,79,81-82,87-88,91-93,96-97,99-100,102-103,105-106,109,112,114-117,123-124,126-132,134,136,139-141,145,147-148,150-151,153,156,158,161-162,164-166,168-169,171-175,177,179-181,183-184,186,191,196-197,199-200,202-203,205-206,208,210,215-217,219-221,225-227,229-234,236,238,240,243-244,246,249,253-254,256,258-261,263,265,269,272,277-278,281-286,288,291-294,296,299,303-306,308,310,312-313,316-320,322-323,325-326,328-330,333-334,340-342,344-345,348-349,354,356-360,365-377,379,381-384,386-387,389-390,398-400,402-403,405,407-409,418-419,421-422,428-429,431,433-434,437-440,442-443,447,449,451,453-455,460,463,465,467,470,476-480,482-484,487-489,492-497,499-502,506,508-509,511-513,516-517,519,521-524,526,528,530,532-533,538-540,542-545,548,552-553,555-556,559-560,563,565-566,572,575-577,579,584,586-588,591,594-596,598,601-603,607,609-611,614-616,618-619,624-625,627-629,631-636,638,640-645,648,650-653,655-656,658-659,661,663-664,667,669,671,674-675,679-683,687,689,691-692,694,696,701-702,704-707,710-711,714,717-718,721-722,724-727,731-734,736-737,739,744-746,748,750-752,755-757&D5=10,14,16,20&HDR=T,G4&STB=G1,G3,G2&VW=T

Municipality-level unemployment rate 2006, 2010, 2012, 2016:

http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=83524ned&D1=12&D2=0&D3=57-446&D4=3,7,9,l&HDR=G3&STB=T,G1,G2&VW=T

Yearly asylum requests shown in figure 1 (chapter 2):

http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=80059ned&D1=1&D2=0-2,4,7-8,11,20,23,26,35-37,41,45,52-53,56-57,60,63,67,69-70,72,75&D3=a&HDR=T,G1&STB=G2&VW=T

Monthly asylum requests shown in figure 2 (chapter 2):

http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=83102ned&D1=1&D2=0&D3=0&D4=0,2,4-6,15,22,25,28,34,36,38,45,48&D5=0-2,4-6,8-10,12-14,17-19,21-23,25-27,29-31,34-36,38-40,42-44,46-48,51-53,55-57,59-61,63-65,68-70,72-74,76-78,80-81&HDR=T,G3&STB=G1,G2,G4&VW=T

Regular shelter locations:

https://www.coa.nl/nl/zoek-locatie

Municipality mergers:

https://www.cbs.nl/nl-nl/zoeken/?query=demografische%20kerncijfers%20per%20gemeente

Temporary shelter locations:

I googled the following three terms for each of the 380 municipalities in my sample: "[municipality] vluchtelingen", "[municipality] noodpvang asielzoekers", "[municipality] azc". When the first ten search results of each of these three terms did not give any indication of a temporary shelter location having been opened in the municipality, I concluded that no location was opened. If some search results did suggest the opening of a shelter location, I clicked on those results to find out more. When search results were about plans for a shelter location, I continued searching in order to find out whether the plans had been realized or not.

A very similar method was used to find the municipalities that should be included in the adjusted control group.

6.3 Placebo tests

	Full sample	Full sample
δ	-0.96 (0.01)	-0.66 (0.04)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.91	0.93
Observations	760	760

Table A.1: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, full sample. P-value in parentheses.

	Full sample	Full sample
δ	0.60 (0.00)	0.30 (0.11)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.91	0.93
Observations	760	760

Table A.2: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, full sample. P-value in parentheses.

	Many inhabi-tants sample	Many inhabi-tants sample	High unem-ployment rate sample	High unem-ployment rate sample	Few non-Western immi-grants sample	Few non-Western immi-grants sample
δ	-2.42 (0.01)	-1.76 (0.01)	-2.63 (0.02)	-1.92 (0.01)	0.41 (0.75)	-0.52 (0.61)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.93	0.95	0.91	0.96	0.92	0.94
Observations	100	100	94	94	100	100

Table A.3: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, demographic subsamples. P-value in parentheses.

	Many inhabitants sample	Many inhabitants sample	High unemployment rate sample	High unemployment rate sample	Few non-Western immigrants sample	Few non-Western immigrants sample
δ	1.09 (0.01)	0.86 (0.02)	1.38 (0.01)	0.65 (0.06)	-0.45 (0.67)	-0.02 (0.98)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.93	0.95	0.92	0.96	0.90	0.93
Observations	100	100	94	94	100	100

Table A.4: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, demographic subsamples. P-value in parentheses.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
δ	-1.02 (0.01)	-1.23 (0.00)	0.06 (0.94)	-0.39 (0.66)	-1.29 (0.01)	-1.14 (0.01)	0.02 (0.98)	0.40 (0.58)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.95	0.96	0.90	0.91	0.94	0.96	0.92	0.95
Observations	280	280	110	110	158	158	212	212

Table A.5: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, geographic subsamples. P-value in parentheses.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
δ	0.69 (0.00)	0.58 (0.00)	-0.10 (0.78)	-0.05 (0.91)	0.75 (0.00)	0.60 (0.02)	-0.08 (0.86)	-0.31 (0.50)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.95	0.96	0.92	0.93	0.94	0.95	0.93	0.95
Observations	280	280	110	110	158	158	212	212

Table A.6: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, geographic subsamples. P-value in parentheses.

6.4 Placebo tests – adjusted control group

	Full sample	Full sample
δ	-0.37 (0.41)	-0.19 (0.64)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.92	0.94
Observations	294	294

Table A.7: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, full sample. P-value in parentheses.

	Full sample	Full sample
δ	0.22 (0.39)	0.14 (0.58)
Municipality fe	Yes	Yes
Year fe	Yes	Yes
Controls	No	Yes
R^2	0.92	0.94
Observations	294	294

Table A.8: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, full sample. P-value in parentheses.

	Many inhabitants sample	Many inhabitants sample	High unemployment rate sample	High unemployment rate sample	Few non-Western immigrants sample	Few non-Western immigrants sample
δ	-2.50 (0.02)	-1.70 (0.12)	-0.01 (0.99)	-0.35 (0.69)	-0.39 (0.62)	0.27 (0.69)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.93	0.94	0.92	0.96	0.95	0.98
Observations	70	70	60	60	60	60

Table A.9: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, demographic subsamples. P-value in parentheses.

	Many inhabi-tants sample	Many inhabi-tants sample	High unem-ployment rate sample	High unem-ployment rate sample	Few non-Western immi-grants sample	Few non-Western immi-grants sample
δ	1.33 (0.01)	0.63 (0.24)	0.23 (0.70)	0.56 (0.18)	0.31 (0.63)	0.90 (0.17)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R^2	0.92	0.95	0.93	0.97	0.90	0.94
Observations	70	70	60	60	60	60

Table A.10: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, demographic subsamples. P-value in parentheses.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
δ	-1.13 (0.06)	-1.64 (0.00)	0.16 (0.86)	0.13 (0.89)	-0.65 (0.56)	-1.55 (0.14)	1.04 (0.26)	1.22 (0.17)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.95	0.96	0.93	0.95	0.95	0.97	0.95	0.96
Observations	100	100	64	64	62	62	68	68

Table A.11: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2006-2010 placebo test, geographic subsamples. P-value in parentheses.

	Randstad sample	Randstad sample	Northern sample	Northern sample	Eastern sample	Eastern sample	Southern sample	Southern sample
δ	0.54 (0.05)	0.32 (0.22)	-0.19 (0.62)	-0.20 (0.58)	0.56 (0.38)	0.94 (0.13)	-0.40 (0.43)	-0.45 (0.45)
Municipality fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.97	0.98	0.95	0.97	0.95	0.97	0.95	0.96
Observations	100	100	64	64	62	62	68	68

Table A.12: The difference-in-difference estimator of the treatment effect on the local PVV vote share - 2010-2012 placebo test, geographic subsamples. P-value in parentheses.

6.5 Exposure to manufacturing imports from China

Standard economic theory predicts free trade with low wage countries to make low skilled workers in rich nations worse off (Samuelson, 1948). Together with the notion that right-wing populism opposes trade globalization in order to protect jobs of natives (Rydgren, 2008; Guiso et al., 2017) and the notion that Dutch manufacturing imports from China have grown spectacularly since the 1990s, this justifies the use of local import exposure to manufacturing imports from China as a control variable in my difference-in-difference model. To construct a proxy for this exposure in 2006, 2010, 2012 and 2017, I use equation (2), to the example of Autor et al. (2013).

$$\Delta IPW_{nit} = \sum_{(j)} L_{ijt} / L_{njt} * \Delta M_{ncjt} / L_{it} \qquad (2)$$

L_{ijt} is the number of hours worked in region i in industry j in 1994. L_{njt} is the number of hours worked in the Netherlands in industry j in 1994. ΔM_{ncjt} is the change in Dutch imports from China related to manufacturing industry j in between 1996 and 2016 [27] (alternatively 2006, 2010 or 2012). L_{it} is the total number of hours worked in all industries in region i in 1994.

This measure combines initial (1994) differences in sector specializations between regions with differences in the growth of imports from China between manufacturing sectors, in order to measure the change between 1994 and 2016 (alternatively 2006, 2010 or 2012) in Chinese import exposure per labour unit in each Dutch region. The intuition behind this approach is that depending on ex-ante sector specializations, different Dutch regions are exposed more or less to manufacturing imports from China. The ΔIPW measure captures variation in import exposure arising from regional ex-ante differences in manufacturing versus non-manufacturing activities, and from regional ex-ante differences in sector specializations *within* manufacturing. For a more thorough discussion of the ΔIPW measure, I refer the reader to Autor et al.'s paper.

I distinguish between 40 different commuting zones, the finest level at which labour volumes are publicly available. Each municipality is allocated the import exposure level of the commuting zone to which it belongs. I distinguish between 11 industries. Labour units are

[27] 2017 data are not publicly available.

measured using the SBI index, while imports are measured using the SITC index. I match SITC categories to their corresponding SBI categories as shown below.

Labour units (SBI)	**Imports (SITC)**
Agriculture, forestry and fishing	0, 22, 29, 4
Mining and quarrying	27, 28, 32, 333, 34
Food, beverages and tobacco	1
Textiles	21, 26, 61, 65, 83, 84, 85
Paper and printing	25, 64, 892
Petroleum, chemicals, rubbers and plastics	23, 334, 5, 62
Metals and machinery	67, 68, 69, 71, 72, 73, 74
Electronics and transport equipment	75, 76, 77, 78, 79, 87, 88, 898
Construction	81
Remaining manufacturing industries	24, 63, 664, 665, 666, 82
Non-manufacturing	None

Data on labour units per sector and per region in 1994 are traced from the CBS website: https://opendata.cbs.nl/statline/#/CBS/nl/dataset/70090NED/table?ts=1525037102836

Just like the value of imports per industry in 1995, 2006, 2010, 2012 and 2016: https://opendata.cbs.nl/statline/#/CBS/nl/dataset/7137shih/table?ts=1525114895888

This last URL also contains the information that supports my claim that manufacturing imports from China have grown spectacularly since the 1990s.

6.6 Migration patterns 2011-2016 and change in demographic averages 2012-2017

Source:
http://statline.cbs.nl/Statweb/publication/?DM=SLNL&PA=70072ned&D1=1-2,21-27,33,35-36,81-84,88,102-106&D2=57-60,64,66-67,69-72,74,77,79,81-82,87-88,91-93,96-97,99-100,102-103,105-106,109,113-117,123,125-133,135,137-138,140-142,146,148-152,154,157,159,162-163,165-170,172,174-177,179,181-183,185-186,188,193,196,198-199,201-202,204-205,207-208,210,212,218-220,222-224,228-230,232-236,238,240,242,245,247-248,250,253,257-258,260-261,263-264,267,269-271,273,276,280-282,285-290,292,295-300,303,307-310,312,314,317-318,321-324,326,328-329,331-332,334-336,339-340,346-348,350-351,354-355,360,362-366,371-374,376-379,381-384,386,388-391,393-397,404,406-408,410-411,413,415-417,425-427,429-430,436-437,439,441,445-451,456-458,462-464,466,468-469,471-472,474,476,479-480,485-486,488-489,491-493,496-498,501-506,508-511,515-518,520-522,525-526,528-533,535,537,539,541-542,547-549,551-554,557-558,561-562,564-565,568-569,573,575-576,582,584-589,592,594,596-599,601-602,604-606,608,611-614,618,620-622,625-627,629-630,635-636,638-640,642-649,651-656,659,661-667,669-670,672,674-675,678,680-682,685-686,690,692-695,699,701,703-704,706,708,713-714,717-720,723-724,727,730-731,734-735,737-740,744-750,752,756-759,761,763-765,768-769&D3=17&HDR=T&STB=G1,G2&VW=T

	2012	**2017**
Inhabitants per municipality	82,316	84,610
Share of men	0,493	0,495
Share of women	0,507	0,505
Share of 0-5 years olds	0,057	0,053
Share of 5-10 years olds	0,056	0,054
Share of 10-15 years olds	0,057	0,055
Share of 15-20 years olds	0,058	0,059
Share of 20-25 years olds	0,071	0,071
Share of 25-45 years olds	0,283	0,270
Share of 45-65 years olds	0,266	0,269
Share of 65-80 years olds	0,112	0,128
Share of 80+ years olds	0,040	0,042
Share of unmarried people	0,503	0,518
Share of married people	0,371	0,354
Share of divorced people	0,077	0,081
Share of widowed people	0,049	0,046
Share of people with a Dutch background	0,723	0,702
Share of people with a western immigrant background	0,108	0,116
Share of people with a non-western immigrant background	0,169	0,182
Share of households consisting of one person	0,421	0,431
Share of households without children	0,266	0,261
Share of households with children	0,313	0,308
Average household size	2,110	2,057

Table A.13: Demographic composition of all people living in treatment group municipalities.

	2012	2017
Inhabitants per municipality	28,350	28,762
Share of men	0,497	0,497
Share of women	0,503	0,503
Share of 0-5 years olds	0,053	0,049
Share of 5-10 years olds	0,060	0,055
Share of 10-15 years olds	0,064	0,060
Share of 15-20 years olds	0,062	0,063
Share of 20-25 years olds	0,054	0,054
Share of 25-45 years olds	0,240	0,223
Share of 45-65 years olds	0,295	0,296
Share of 65-80 years olds	0,131	0,153
Share of 80+ years olds	0,042	0,048
Share of unmarried people	0,433	0,440
Share of married people	0,446	0,434
Share of divorced people	0,063	0,071
Share of widowed people	0,055	0,055
Share of people with a Dutch background	0,855	0,843
Share of people with a western immigrant background	0,079	0,083
Share of people with a non-western immigrant background	0,065	0,074
Share of households consisting of one person	0,307	0,322
Share of households without children	0,320	0,319
Share of households with children	0,373	0,360
Average household size	2,360	2,278

Table A.14: Demographic composition of all people living in control group municipalities.

	Treatment	Control
2011	0,0380	0,0342
2012	0,0383	0,0350
2013	0,0353	0,0363
2014	0,0407	0,0378
2015	0,0445	0,0398
2016	0,0411	0,0460

Table A.15: Share of people moving to another municipality - average of all treatment group municipalities and average of all control group municipalities.

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