Reviewer #1: Title: Exposure to Asylum Seekers and Changing Support for the Radical Right  
  
This paper tests how the sudden inflow of asylum seekers to a region changes voters’ support for the radical right in the region. Unlike many other studies on the support for the radical right, this paper utilizes an individual-level panel dataset on the regional level, employs a natural experiment method, and distinguishes between different types of refugee centers. Though authors’ finding that the inflow of asylum seekers strengthens the support for the radical right is not surprising, their methodological approaches contribute to the literature. Though I support the publication of this manuscript, there are some questions that should be answered beforehand.

1. The structure of the hypotheses is odd. Hypothesis 1 describes the positive correlation between refugee inflow and support for the PVV. Then, the two hypotheses (2a and 2b) suggest two different causal mechanisms between the two: one for the positive correlation (threat) and the other for the negative correlation (contact). I do not deny that the inflow of asylum seekers, or immigrants in general, can have dual effects (both positive and negative) through threat and contact mechanisms either on the support for the radical right or on public opinion on immigrants. Nonetheless, I think authors need to re-frame their hypotheses so that they incorporate all of these possible mechanisms and patterns.

*Following the suggestion of R#1 we reframed our hypotheses. We hope to have made clear that the threat mechanism is likely to mediate the expected positive relation and that the contact mechanism is likely to supress the expected positive relation. Thus we expect a positive relation between refugee inflow and support for the PVV (1a) and this is in part explained by the threat mechanism (1b). Recognizing that the inflow of refugees can have dual effects we expect that the contact mehanisms supresses the positive relation (2).*

2. Related to the first comment, authors find and conclude that their results support H1, but not H2a and H2b. That is, though they find a positive correlation between refugee inflow and PVV support, the causal mechanisms they hypothesized are not supported by the results. Then, the question is: WHY does the refugee inflow strengthen support for the radical right? Authors need to discuss this.

The reviewer is right, in so far that the threat (and contact) mechanism does not explain regional variation in changing support in PVV support. The question is however not exactly “WHY does the refugee inflow strengthen support for the radical right?” but more precisely “Why at the local level refugee inflow strengthen support for the radical right?”. With respect to the first question we give in our revised manuscript more attention to national-level trends. With respect to the second question we already briefly discussed these in our original manuscript in the discussion but in the revised manuscript we expanded the original discussion and already mention in the theory section possible alternative causal mechanism for the link between refugee inflow in the locality and increased support for the PVV.

3. Though authors distinguish economic threat and cultural threat in their theory section, their threat variable doesn’t. I understand that authors were not able to change the survey question, but they need to provide more discussion on the question wording itself. When the question was delivered to respondents, did it imply economic threat, cultural threat, or both of them to the people?

We discussed our threat measure in more detail in the method section. We interpet our neighbourhood threat measure as a catch all term for possible economic, cultural or safety threat. We like to stress that our threat measures explicitly refers to the local level.

4. As authors acknowledge, one critical weakness of their data is a plausible self-selection bias because respondents basically voluntarily sign up for the survey. One question related to this is: is there possibility that PVV supporters, after the sudden inflow of asylum seekers, are more motivated to accept the invitation to the survey in the 2nd wave because of, for example, their anger from the inflow? If this is true, then the self-selection bias problem occurred and it could make their results biased toward their findings. So, is there any way to make sure that PVV supporters and non-supporters had the same propensity to sign up for the survey, both in the 1st and the 2nd wave?

We discuss checks on in our opinion most important possible self-selection issues more elaborately in the revised manuscript. We demonstrate that:

1. Voting behaviour in wave 1 does not predict participation in wave 2
2. Living in local neighbourhood in wave 1 which will experience influx of refugees, does not participation in wave 2.

5. Authors describe three different types of refugee centers on page 11, but I don’t believe that they explained what a temporary ASC is. (Does the “crisis ASCs” on line 320 actually mean temporary ASCs?)

We explained the three different types of refugee centers in paragraph ‘Central Agency for the Receoption of Asylum Seekers’. Page 9, line 257 and further. We did mean ‘temporary ASC’.

Regular reception centres (‘reguliere opvang’): long-term (minimum lease contract 2 years). Most already existed before the 2015 refugee crisis.

Temporary centres (‘noodopvang’): mid-term (buildings are generally leased for a period of six to twelve months).

Crisis centris (‘crisisnoodopvang’): short-term (no minimum lease period). The same refugees are housed in crisis centers for a maximum of 72 hours.

Reviewer #2: This is an interesting paper on how the sudden inflow of refugees influenced vote intentions in the Netherlands. The strength of the paper is the possibly exogenous exposure to refugees due to the rapid inflow, combined with individual level panel data. The authors find that exposure to refugees increased the vote intention for PVV, the anti-immigration party in the study.  
  
My main issue with the paper is how they analyze the data. The current analysis makes me not convinced that the authors estimate the effects of exposure.  
  
The first issue regards exogeneity. The authors have a clear ambition to estimate causal effects, but is not sufficiently clear on what variation in exposure to refugees that is exogenous. Clearly, between-unit variation in exposure is not exogenous, but part of the variation between the waves might be exogenous. To me, the most promising source of exogenous variation stems from the crisis ASC, and the paper and the analysis should be centered on that source of variation.

*We have the ambition to estimate a model by which it is most likely that estimates refer to causal mechanisms. We stress in our manuscript that thanks to our data and modelling strategy it is more likely that our estimates refer to causal mechanism than related research in which no individual-panel data is used and in which selection effects are more likely. We do not claim to have estimated causal mechanism and we refrain from causal language in our manuscript. We use sentences like “An influx in asylum seekers is related to a change in support of the PVV.” to describe our results.*

*We fully agree with the R#2 that the within-unit variation over time in exposure to asylum seekers is most interesting. This is exactly why we estimate FE models and a hybrid model. The estimates of interest refer to these within-unit variations over time. We argue that it is (very) likely that the within-unit variation over time in exposure to asylum seekers is exogenous because: (1) respondents did not have time to move between the survey waves; (2) asylum seekers could not decide where they were housed; (3) the dutch government and COA did not have time to select neighbourhoods for new temporary centers nor have time to select crisis centers.*

*We fully admit that even if we assume that there was no active selection by respondents, asylum seekers or institutions of the neighbourhoods in which asylum seekers could be housed, this not fully guarantees that all variation is changing exposure the asylum seekers is exogenous. But because we use individual-level panel data we already control for time-stable unobserved heterogeneity. This only leaves us with possible unobserved time-varying heterogeneity in exposure. We demonstrate that participation in wave 2 is not determined by (a) voting preferences in wave 1 and (b) whether or not the neighbourhoods responents live in will experience a sudden increase in exposure of asylum seekers . This makes one source of unobserved time-varying heterogeneity (different time trends in voting intentions between respondents) less likely but not impossible.*

*Finally, we agree with reviewer#2 that whether or not a change in exposure to asylum seekers may be assumed to be exogenous may depend on the type of ASC. I however am unsure for which type it is most likely to be exogenous. My guess would be this will refer to changes in asylum seekers housed in the regular ASC which did not yet reached full capacity before the 2015 refugee crisis. R#2 would guess exogenous changes will be most likely associated with crisis centres, probably because these locations were already marked out by local governments to house citizens in times of incidents or disasters (see page 10). However, especially for crisis centers local governments were responsible for asylum seekers (for regular and temporary ASC it is COA who is responsible) which may have caused some influence on whether or not asylum seekers were housed (see page 10).*

*We discuss the issue of the extent to which variation in exposure to refugees can be assumed to be exogenous in several places in the manuscript. We also come back to this issue in the discussion. Moreover, although we do not claim to estimate causal effects, we hope that R#2 agrees with us that our estimates are more likely to represent true causal mechanisms as compared to related previous research.*

This leads me to the second issue which regards the analysis. I think the authors should estimate a standard differences-in-differences model using the crisis ASC as the treatment indicator (equal to one if a crisis ASC was set up in the neighbourhood between wave 1 and 2) and the wave as the post-treatment indicator. The DD estimate from this analysis might be given a causal interpretation.

Reviewer 2 recommends us to "estimate a standard differences-in-differences model using the crisis ASC as the treatment indicator (equal to one if a crisis ASC was set up in the neighbourhood between wave 1 and 2) and the wave as the post-treatment indicator".

The traditional DiD model for individual-level panel data with additional time-constant covariates *ci* is:

*Yit= β1Timet + β2Treati + δ(Timet⋅Treati) + ci + ϵit*  (1)

with *δ* being the DiD estimator and Treati the dichotomous treatment variable.

Formula (1) is equivalent to:

*ΔYi=β1+δ Treati + ϵi* (2)

Since our outcome is a binary variable there is no standard DiD model. We therefore estimated:

*logit(Pr(ΔYi =1|Treati))= β1 + δ Treati ,* (3)

with *ΔYi =1* if the dependent outcome was 1 post-treatment (i.e. wave 2) and 0 if the dependent variable was 1 pre-treatment (wave 1). Our *Treati* variable is the change in exposure the asylum seekers. Formula (3) is the fixed effects logistic regression model for two waves (or, more precisely, the first difference model which for two waves which is equivalent to the more general fixed effects model).

Formula (2) demonstrates that the DiD model is a type of fixed effects model because the time constant covariates drop out of the model. With our FE-model, we are thus able to control for time-stable unobserved heterogeneity. This, together with the fact to within unit-variation in our treatment can be assumed to be to a large extent random (which we show in our manuscript), we can make strong (but not definite!) claims on causality.

Naturally, we are aware that because we have a binary outcome and we use a nonlinear link function we violate the common trend assumption necessary to interpret delta as the DiD estimator. Moreover, we like to point out that our original ‘treatment’ variable *Treati* is not a dichotomous variable, and this also makes why we cannot interpret our effect as the traditional DiD estimator. But we do not claim to estimate a DiD estimator.

Following the suggestion of R#2 we estimated formula (1) directly for a binary outcome variable as an additional robustness check. That is, we estimated a linear probability model (LPM), while controlling for heteroscedasticity in the error term. We did this once with our original ‘treatment’ variable and once applying the dichotomization as suggested by the reviewer. For the latter model, the estimate could now be interpret as a DiD estimator. The additional additional time-constant covariates *ci* included in our model are similar as in our original model. The estimates of interest are summarized below.

Continuous treatment variable dichotomized treatment variable

M1 M2

AZC (all)

AZC (regular)

AZC (temporary)

AZC (crisis)

This again leads me to the third issue which is the examination of “as-if-random” exposure to refugees. The balance analysis in the paper (Table A3) is not properly explained. The appropriate way to examine balance is to conduct an F-test of whether the exogenous covariates can jointly predict the treatment. For instance, if you use the setup of crisis ASC in the respondents as the treatment you need to show that treated neighborhoods are similar to comparison neighborhoods.

*R#2 we hope the reviewer understand that space is limited in the manuscript. We will provide a full replication package would this manuscript be accepted for publication. In our understanding of the literature there are quite a lot of different ways how to test for unbalance and how to take this into account. We followed the strategy that we simply accepted that our dataset is to some extent unbalanced on key variables (whether or not this is univariate of multivariate significant unbalance is somewhat irrelevant) and ‘corrected’ this unbalance. [even though we are able to control for these charactersitics. They are ci ]*

*https://web.mit.edu/~r/current/arch/i386\_linux26/lib/R/library/Matching/html/MatchBalance.html*

The fourth issue is how the authors threat the contact and threat variables. To me these variables should be analyzed as outcomes that are potentially affected by exposure, they should not be analyzed as covariates (see the literature on post-treatment bias). I understand that the authors considers them as mechanisms or mediators, but one needs separate exogenous variation to properly estimate the role of mediators. Or, if the authors are willing to make strong (and in my view implausible) assumptions, they conduct a Baron-Kenny-type of mediation analysis.

We are unsure whether we understand R#2 suggestion. We did analyze these outcomes which may be potentially affected by exposure. We demonstrate that changes in exposure are NOT related to more threat or contact. Hence they cannot explain the impact of exposure. For a (Baron-Kenny-type) of mediation the independent variable (change in exposure) should predict the mediator (change in threat and contact). We tried to make this more clear in the revised manuscript (see page).

We (think to) know what post-treatment bias: (unnecessary) controlling in the regression model for the consequence of the treatment. But in our case we estimate the model once without threat and contact and once with threat and contact. Moreover, threat and contact are not controls but indeed key mediators. Our second model (including contact and threat) show the ‘direct’ effect of the treatment once the threat and contact mechanism are taken into account.

https://stats.stackexchange.com/questions/163174/why-is-post-treatment-bias-a-bias-and-not-just-multicollinearity

Other issues. i) The attrition analysis mentioned on page 9 should be reported in the appendix.

Okay

ii) The underrepresentation of different groups (pg 9) should be presented in the appendix.

Okay

Also, are sample weights applied?

No, we see no reason given our design.

iii) The authors are probably interested in the experimental literature on contact theory, reviewed in Paluck et al. (2019, The contact hypothesis re-evaluated, Behavioural Public Policy)

thank you for the reference. Is included in the text.

iv) Have they considered non-linearity in the relationship between exposure and vote intention, perhaps exposure has larger effects in neighborhoods with low prior exposure (e.g. Hopkins 2010, cited in the paper)?

Please note our main model is already a non-linear model. Second for exposure resulting from temporary and crisis centres the prior expose is simply zero. Below we show the results for total exposure.

Reviewer #3: This is a very important study. The effect to contact with asylum seekers on support for radical right-wing populist parties is an important subject to study. This is not the first study to examine this, but it is a very rigorous study. In this field, in particular given the social relevance of this research (how do values like tolerance develop, what is the societal reaction to refugees) and academic relevance in the debate between contact and threat, I think that studies like these, which rigorously look at the effect of specific events are welcome.  
In particular I think this contributions stands out because of their advanced quasi-experimental design, which is apt to study the phenomenon, well-executed and convincing.

Thank you.   
  
I have only very minor concerns. The first of these is that there is a typo on p.13: it now reads concerning the main effect studied in the paper “(b=0.022, se=0.12; Model 1, Table 2)” but then the effect would not be significant. This has to be (b=0.022, se=0.012) in line with the Table 2.

We corrected the typo.

The second is that the paper mixes the terms refugees and asylum seekers while the first term refers to people who have an official status as refugee and these second term refers to people who want that status. That means that in COA centres there only are asylum seekers and no refugees (who get their own housing once their status has been given).

R#3 is absolutely right. We replaced all terms refugees with asylum seekers.

Note that regular centers may also house refugees because it proves difficult to provide them with housing once their status has been given.