Dear Dr. Jennifer Gibbs, Co-Editor of Communication Research

We are grateful for the opportunity to revise and resubmit the manuscript ID CR-17-387 entitled as “*The Dynamics of Message Selection in Online Political Discussion Forums: Self-Segregation or Diverse Exposure?*” to the Communication Research. We would like to thank the editor and the two anonymous reviewers for the helpful suggestions and comments for improving the previous version of the manuscript. We appreciate the opportunity to get insightful and valuable feedback from attentive and knowledgeable reviewers, and glad that both of the reviewers see a considerable merit in our manuscript for advancing the literature on this topic.

Below we address, point-by-point, the concerns expressed by the reviewers. We have done our best to respond to the questions in great detail in this document to help alleviate any concerns regarding theoretical assumptions, analytical approaches, model specifications, and further considerations that arise from the results. We have, in many places, added additional information or clarification in the manuscript as well, but not in nearly the detail in that we offer it here for the reviewers (currently the manuscript is 41 pages long as a result of this revision). We are more than happy to offer this additional information as an online appendix (that we believe will be available at the journal website) or instead make it available from authors should one request such information, depending on the editor’s preference.

In closing, we hope that through this round of revisions we have made a compelling case for the current approach. We believe that the manuscript has greatly benefited from the revisions.

We look forward to your reactions.

Sincerely,

The Authors.

**Comments to the Author**

POINT #1 First, it would help to know more about the sample and the recruitment process. The manuscript states (p. 13) that respondents were recruited from a nationally-representative panel. I’d like to know how they were recruited and what incentives were offered. Did they receive additional incentives for using the forums or only for responding to the surveys? These details might help readers understand how generalizable the results are.

AUTHOR RESPONSE:

As requested, we incorporated more detailed information regarding the sample and recruitment process, as appear in the revised manuscript (pp. 13). In brief, we’ve relied on a research firm’s opt-in panelists to recruit the participants of the current study, where the firm maintains the national opt-in panel with access to one million identity-verified individuals that closely matches gender and age distributions of the entire Korean population. Among firm’s opt-in panelists, a total of 400 participants were randomly selected and invited to use the discussion forum for upcoming election (pp. 13 of the revised manuscript). Footnote 2 in the bottom of page 14 gives more details regarding the representativeness of the study participants from this opt-in panel (based on all 312 eligible samples in our main analysis), and we also note that a monetary incentive has been provided for the eligible participants *after* the completion of the study (as a matter of fact, those who provided all three panel surveys and stay remain active in the online forum were received compensation). There are now ample evidences that such opt-in panels (or crowdworkers in general receiving monetary incentives in exchange for their participation to social science research, such as Amazon Mturk) are somewhat different from general populations, and also they tend to be more engaged and interested in answering surveys (e.g., Levay et al., 2016; Huff & Tingly, 2015). Yet to date, to the best of our knowledge, we do not aware any existing research suggesting that such opt-in panels are systematically different from general populations in terms of their message selection and browsing behaviors (e.g., whether they favor counter-attitudinal or pro-attitudinal messages over others). Yet we still acknowledge that, using an opt-in, not probability-based, panel for recruitment would limit the generalizability of our results and would likely introduce implicit or unknown bias associated with participants’ motivations for “opting-in” to the panel (Hillygus, Jackson, & Young, 2014), where we explicitly state such caveats in footnote 2 of the revised manuscript.

POINT #2  Additional descriptive statistics about the forum users would also help. The manuscript (p. 13) notes that the surveys had 341 respondents and these respondents were encouraged to use the discussion forum. It also notes that “participants on average posted 24.78 messages and read 547.31 postings made by others.” (I’d suggest rounding to the nearest whole number here since the decimals are not substantively interesting) This information is useful, but readers might also like to know how many of the respondents posted messages to the forum, how many respondents read messages, and how often they used the forum. This information will help readers evaluate whether the results are driven by only a few outliers who are exceptionally interested in the election.

AUTHOR RESPONSE:

As requested by the reviewer, such descriptive statistics are now incorporated in the method section. Specifically, over the period of data collection (i.e., during 27 days), participants on average posted approximately 25 messages and read 93 unique postings made by others, resulting an average of 486 reading instances per individual (please note that the reading count is slightly reduced due to the fact that previous descriptive statistic was based on a total of 341 respondents who remained in the online panel, whereas new descriptive is based on only 312 eligible respondents after filtering out 29 respondents, as described in page 14 of the revised manuscript). Data logs indicate that participants in general were active on the discussion forum. On average, there were 221.81 unique users per each day at the forum (either as a poster or a reader) during the study period (*SD* = 30.99). Data also indicates that participants were especially active for reading. All of the 312 participants read others’ messages at least once, while only few participants did not post at all (*N* = 5, 1.6%) during the study period. Indeed, participants typically read at least 5 or more posts per day (median reading counts divided by total no. days = 5.01) and posted at least 1 message or more every two days (median of posting count divided by total no. of days = .51). Overall, although the distributions of raw reading and posting frequencies are somewhat skewed to the right, since we dichotomized the message selection instances within each dyad based on the overall dyadic mean, the worry that our results were particularly driven by only few exceptional participants who are highly interested in election is, we believe, less warranted, not least most participants were engaged to a varying degree in online discussion. These updated descriptive statistics now appear in page 14 to 15 of the revised manuscript.

POINT #3 I’d also like to know more about the online forum itself. What does the user interface look like? What information do people have at their disposal when they are choosing which messages to read? Which real-world discussion forums does this one most closely resemble? This information will help readers place the results in the appropriate context and evaluate whether any idiosyncratic features of this forum might be driving the conclusions. For instance, does the interface provide any cues that might encourage understanding motivations or discourage consistency motivations?

AUTHOR RESPONSE:

As requested, we now provide more detailed information regarding the interface of the online discussion forum itself in footnote 1 in page 13. In specific, the structure and user interface of the discussion site were adopted from the typical format of most online forums (such as in Reddit). On the forum site, participants were not only allowed to initiate their own posts but also to read and respond to others’ original posts and the subsequent comments. Once logged in, participants were exposed to the main page of the forum where the list of post titles (along with user ID, a timestamp, and the number of views for the post) made by either themselves or other participants were presented, with the latest ones at the top; the title of the post was also accompanied by comment counts (displayed at the right end side of the title), following the common format of many Internet forums. Actual comments, if any, were posted under an initial post in the order they were posted, forming a thread of discussion and therefore were not readily visible when participants logged in. As a consequence, only following information is available for participants to choose which messages to read: the list of thread titles, comment counts, user IDs and timestamp of respective posts, and the number of views for the post. A separate minor section on the main page carried study-related information (e.g., reminder about panel surveys, announcements to encourage forum participation, etc.). No other information was provided that might influence participants’ behaviors.

POINT #4 Perhaps my most important suggestion is to provide more attention to the bivariate relationship between each predictor and message selection. Even if partisan preferences are not significant predictors after conditioning on the other terms, it would be interesting to know the unconditional relationship. Understanding the selection process is important, but so is the end result. From a practical standpoint, people may end up in echo chambers even when they didn’t seek them out. And an insignificant TERGM coefficient provides little insight into this question because it reflects the conditional relationship after controlling for many things that themselves might be consequences of partisan views.

AUTHOR RESPONSE:

As corrected noted by the reviewer, it is indeed possible that the effect of overt partisan preferences (i.e., *same candidate preference* or *policy preference similarity*) can be nonsignificant but individuals may end up being exposed to politically like-minded viewpoints -- even they do not intentionally and explicitly seek out such partisan preferences. Yet this requires several assumptions and corresponding evidences, which we think is not the case based on evidence we outline below.

First, if we only focus the bivariate relationship between message selection behavior and three preference homophily variable (i.e., *candidate preference*, *policy preference similarity*, and *ideological placement similarity*), we observe that those variables generally do not predict message selection behaviors well at bivariate-level analysis (for this test, we used QAP-based *netlogit* function in R *sna,* andsince this is essentially a dyadic-level analysis, we only focus on three dyadic-level predictors here). When we test whether our dependent network (message selection behaviors, 0 vs. 1) can be predicted by three political preference variables alone (one for each predictor oneat a time) without any covariates in the model, neither *same candidate preference* nor *ideological placement similarity* variables were significant in predicting message selection behaviors as reported in Table 2 of the revised manuscript. We do find some indication that *policy preference similarity* variable becomes significant at wave 2, yet the effect of the predictor was actually in the opposite direction (notice here the coefficient of this variable is negative, *b* = -2.0456), meaning that policy preference “similarity” between a given dyad may contributes to a “less” message selection instance for that dyad. In order to help readers to evaluate unconditional bivariate relationship, these results now appear at page 21 and in Table 2 of the revised manuscript.

Second, given the strong influence of other network-endogenous, structural predictors (e.g., *reciprocity* or *preferential attachments*), it is possible that such factors are already capturing some impact of partisan preference indirectly, therefore the residuals variance is not well explained by our partisan preference variables when such variables are simultaneously controlled for. This explanation is possible, yet essentially this requires that our network-endogenous predictor variables themselves are well explained by our partisan preference variables.

In order to investigate this possibility, we chose three network-endogenous predictor variables in our final model – *popularity spread* (i.e., gw-indegree: unstandardized *b* = -4.05), *activity spread* (i.e., gw-outdegree, *b* = -4.35), and *reciprocity* (*b* = .769, all reported in Table 3 of the main manuscript and Table S3 “Final model I” column in the supporting information) as exemplary cases. As can be seen in their coefficient values, these factors are the three most highly significant and strongest variables in our final results.

For this test, we constructed what is called a “change statistics” from these three variables – a “change statistic” is the matrix representation of “predictor values” internally used in ERGM package where each row of this matrix is associated with a particular ordered pair (for directed network) of nodes, and the row equals the *change* in the vector of network statistics (as defined in formula below) when that pair is toggled from a 0 (no edge) to a 1 (edge), holding all the rest of the network fixed (please see Krivitsky, 2012, for a detailed discussion of this concept within the context of ERGM and its role in estimations), such that:

Dependent network at time t ~ *popularity spread at time t*

Dependent network at time t ~ *activity spread* *at time t*

Dependent network at time t ~ *reciprocity* *at time t*

This yields a total of 9 continuous “change statistic” matrices for each dyad (*ij*) in network time *t* for a given variable (three time point x three variables per each time point) that ERGM internally utilize to fit the MPLE estimates in main estimation phase. We set these 9 “change statistic” matrices as a dependent variable one at a time, and then predicted them as a function of our partisan preference homophily variables (i.e., *same candidate* preference, policy *preference* *similarity*, or *ideological self-placement similarity* variable), again one at at time, using R sna *netlm* function (which is based on QAP procedures). If our partisan preference homophily variables are a significant predictor of those change statistics matrices, it would mean that such a variable is already capturing – or indirectly conveying – some impact of partisan preferences (since such endogenous predictors themselves are significantly correlated with partisan preferences). Yet as reported in Table S4 in online supporting information (please see page 11 of the online appendix), none of the QAP regression results are significant when we use *same candidate preference* nor *ideological self-placement similarity* variable as a predictor of a change statistics for network endogenous variables (upper and bottom columns, respectively).

When we instead use *policy preference similarity* in network regression models (middle rows), we do see preference homophily significantly predict three network endogenous predictors in our final model, yet only in wave 2 (results are nonsignificant at all other waves). One may therefore (erroneously) conclude that, based on results for wave 2 in particular, similar policy preference indirectly increase the message selection behavior in a way that individuals end up being exposed to similar viewpoints even though they do not consciously seek out such perspectives. However, the actual direction of the coefficient appears to be in opposite of what would have been expected under such scenario.

In our main results (as reported in Table 3 of the main manuscript), we see *popularity spread* and *activity spread* are highly negative (since they represent “evenness” of the in- and outdegree distributions), while *reciprocity* is a positive predictor of message selection in networks. Now, since *policy preference similarity* variable positively predicts *popularity spread* and *activity spread* variables (as in Table S4), its *net indirect effect* through *popularity spread* or *activity spread* variables become *negative*; it means that the higher the policy preference similarity of a given dyad, they are actually “less likely” to select each other’s message through *popularity spread* or *activity spread* mechanism. Likewise, given *policy preference similarity* variable negatively predict reciprocity and reciprocity in turn positively predict message selection, an *indirect* effect of *policy preference similarity* through reciprocity becomes negative, meaning that higher policy preference similarity contributes “lesser” selection instances*.* If it had been the opposite, it would have indicated that partisan preference homophily would make individuals to be exposed to like-minded viewpoints indirectly through network endogenous factors (i.e., positive indirect effect through network-endogenous factors) even though they are not purposefully seeking out like-minded viewpoints; However, the empirical evidence directly contradicts with this possibility and points to the opposite explanation. In brief, we think there is a convincing evidence that (a) at bivariate-level partisan homophily is not a significant and positive predictor of message selection instances, and that (b) there is no evidence that other explanatory factors in our final model can be regarded as a consequences of partisan homophily.

POINT #5 The network structure is two-mode (a user is connected to a message if they access the message), but the models instead focus on a one-mode projection (user A is connected to user B if A accessed an above-average number of messages written by B). I was initially concerned with this modeling decision because one-mode projections ignore useful information about the decision process and can exaggerate the importance of network features such as clustering. One way to evaluate the extent of this problem is with the goodness of fit statistics, which are provided in Figure S1 (supporting information p. 6). To my eye, these fit statistics look good, so I am not overly concerned about this decision to focus on the one-mode projection. Further, the conclusion addresses this concern directly by emphasizing the need for future research examining the role of message characteristics. Readers may wonder, however, how sensitive the results are to the choice of threshold for dichotomizing the network. Do the key results change under other reasonable thresholds (e.g., anything greater than zero)?

AUTHOR RESPONSE:

We particularly appreciate this suggestion to clarify the implications of our analytical approach. We ground our choice of modeling message selection behaviors as a one-mode network based on several reasons, although we surely acknowledge (as correctly pointed out by both of the reviewers) that one-mode projections may ignore some nontrivial information about the decision process and can exaggerate the importance of certain network features.

First, due to our analytical and theoretical focus on individual-level behaviors (i.e., whether “individuals” intentionally or not seek like-minded viewpoints within online settings), our dependent variable is correspondingly defined at the individual-level message selection behaviors. Second, for that matter, a two-mode network application (where written messages are a secondary type of nodes and individuals are the primary type of nodes) creates a particular challenge in properly modeling interactions among individuals when our analytical focus is in the interactions among individuals. Since we cannot model ties between individuals directly in bipartite networks, many of our theoretically relevant variables (such as “candidate preference homophily” or “similar evaluation criteria”) becomes obsolete in that cases, let alone that this additionally creates some ambiguities in assessing the impact of individual-level predictor variables. Although there surely exists a method to address these issues in the two-mode network application of ERGMs, yet such methods often become very complex to properly interpret. Therefore we opted to rely on one-mode network ERGM over bipartite versions. And as correctly noted by the reviewer, our goodness of fit statistics indicate that our model does a fairly good job reproducing the properties of an observed network.

As to the sensitivity issue regarding the specific threshold of dichotomizing ties, we would like to stress that such results are already reported in the online supporting information, Table S3 (page 7 of the online appendix), and also noted in the page 15 of the revised version of the manuscript. To briefly reiterate, we observed largely the same results with minor discrepancies in estimated coefficients and significance level when we rely on “anything greater than zero” as the threshold values. As can be seen in Table S3, when we use such a threshold (see “No Threshold” column), the results are largely consistent with the main results reported in Table 3 of the main manuscript, except the fact that same candidate preference variable becomes positive and statistically significant (b = .072, 95% CIs = [.059, .094]) whereas similar evaluative criteria becomes less powerful in predicting message selection dynamics (b = .053, compared to main results where b = .407). With respect to network-endogenous variables, no-threshold model (using “anything greater than zero”) still report substantial influence of such self-organizing dynamics in message selection behaviors. Given a small magnitude of the effect of same candidate preference variable in this model, we think that our overall conclusion – that overt partisan consideration plays rather a limited role – still holds when we use “anything greater than zero” as the threshold values.

In order to better highlight this aspect, we direct readers’ attention to online supporting information in the discussion section where we discuss the implications of findings concerning dyadic-level predictor variables. This now appear in page 27 of the revised manuscript.

*Reviewer: 2  
Comments to the Author*  
  
POINT #6 First, as noted in the limitation section, this study did not consider the message characteristics and took a naïve assumption (all messages are equal, in terms of message quality indices, such as attractiveness, persuasiveness, and others) when conducting Temporal ERGM. I also agree with the authors’ opinion that analytic approach to network of message features has not been mature. However, is it not possible to examine the textual similarity between messages (i.e., the shared number of words between two messages generated from the ith and jth actors in the network)? While I am not a super-expert in the network analyses, recent application of latent topic models (and even the basic descriptive statistics of text data) might be used to extract message features in the authors’ work. Anyway I would not suggest that the authors re-analyze the temporal ERGM. However, the absence of textual analyses in message selection seems to be an important caveat, at least for readers like me. Thus I would like to suggest that the authors relocate the potential limitations to the Methods section, rather than in the end of the manuscript.

AUTHOR RESPONSE:

Again, we appreciate this suggestion to clarify the implications of our analytical approach. For a detailed reason of why we opted to rely on one-mode network ERGM while excluding textual information available in the forum, please see our response to the reviewer 1 (point #5) above. Yet we fully agree with the reviewer that this is an important caveat that needs to be acknowledged more earlier in the manuscript. As suggested, we now address this issue in the method section (as appear in page 20 of the revised manuscript) while we also advice future research to take this issue more seriously in the discussion section.

POINT #7 Second, I have a slight doubt in the authors’ judgment over their conclusion. The authors concluded “social and utility considerations strongly override overt partisan considerations” and I fully agree with their conclusion. However, this finding is really new? I do not think so. As reported cumulatively in attitude research, general attitude measure, compared to specific attitude construct, has weaker predictive power. Given that the authors’ study is based on national presidential election, it is never surprising that candidate evaluation rather than overt partisanship, would serve stronger predictor of message selection. While the findings and the authors’ conclusion seem totally believable, I do not think they are really new and surprising. I think overt partisan considerations cannot be simply ignored due to their weak predictive power. The weak predictive power is originated from the nature of ‘general and abstract construct’ rather than their theoretical impotency. I would like to suggest that the authors slightly tone down the value of the findings. 

AUTHOR RESPONSE

As requested, we have now toned down our conclusion by stating that “our results overall suggest that overt partisan considerations played a limited role in message selection dynamics than often assumed in prior research (e.g., Bakshy et al., 2015; Himelboim et al., 2013)” as appear in page 28 of the revised manuscript.

Yet we would like to clarify few things regarding this comment of the reviewer. First, our measure of “candidate evaluation *criteria similarity*” (not “candidate evaluation” per se) is the *relative importance* of policy/candidate trait-centered (e.g., policy, competence, integrity) *versus* personal background-centered (e.g., party affiliation, political career, place of origin, etc.) dimensions in one’s candidate evaluations (as reported in page 17 of the manuscript). So, for a given dyad who score higher in this similarity dimension, it means that an ego and his or her alter have similar judgmental *criteria* regarding candidate evaluation but notice here its does not actually mean that they end up agreeing on their evaluations – it merely means that they employ the same “criteria” in making evaluations.

In contrast, when we refer to “overt partisanship,” we mean traditional “political ideology” similarity (in terms of respondents’ self-placement on traditional 7-point scale of ideology), “candidate evaluation” similarity (in terms of whether they are actually supporting the same candidate or not), or “policy preference” similarity (in terms of their preferences towards specific policy issues).

While we reasonable expect that partisanship homophily (as measured candidate / policy preference homophily or ideological similarity in our analysis) would play a major role in how people acquire political information from their peers as exemplified in previous research such as in Bakshy et al. (2015) or in Himelboim et al. (2013), surprisingly we find no evidence in our analysis that candidate / policy preferences nor ideological similarity play a major role in one’s message selections (as appear in Table 3 of the main manuscript). The reviewer has reasoned that general attitude measure, compared to specific attitude construct, has weaker predictive power, and wondered whether our political preference-based homophily measures (especially our candidate preference variable) has somewhat abstract and general than more specific measure of attitudes.

We respectively doubt the reviewer’s assessment that our candidate preference measure is “general and abstract construct” – quite contrary, we think candidate preference measure is more context-dependent and highly specific measure of attitudes (given the context of the current study, which is national presidential election), and our candidate preference is indeed defined as the preference between two major presidential candidates. In contrast, our another preference measure is defined using more abstract and general political ideology – as a higher-order abstract evaluative composition of one’s belief system – that determins individuals’ context-specific political behaviors such as candidate/policy preferences (e.g., Jacoby, 1991; Jost et al., 2009). Yet even with this measure, we do not see that political preference variable plays a substantial role in message selection behaviors.

Nevertheless, there is a possibility that our three political preference measures (i.e., *candidate preference*, *policy preference similarity*, and *ideological placement similarity*) and the nature of our dependent variable, message selection preference, are not well aligned with each other, therefore we are seeing a reduced impact of that variable. However, even if we assume so, we still find no evidence that a more domain-specific measure of one’s directional preferences that are directly aligned with message selection behaviors meaningfully predict message selection behaviors. That is, our measure of “consistency motivation” directly taps one’s preference for selecting massages based on their preexisting political attitudes – the measure asks whether they visit online discussion forums (including discussion forums other than in the current study) primarily (1) “to find rationales and reasons for supporting my opinions on the issue,” (2) “to justify my opinion of the issue,” (3) “to confirm that my opinion on the issue is correct,” (4) “to argue and assert my opinion on the issue,” (5) “to refute the opinions of others,” and (6) “to persuade others.” As we already stress in our previous version of the manuscript, if it had been the case that individuals are self-selecting others’ messages that are aligned with their pre-existing preferences, we should see this variable significantly influence message selection dynamics, especially for the out-going ties (i.e., individuals making their own selections). Yet this is not the case based on our analysis. In brief, the balance of evidence points to the conclusion that partisan preferences, regardless of whether it is more specific or general/abstract, do not contribute to self-segregated message exposure patterns.

Also, as per our response to reviewer 1’s comment #4 (please refer our response above), we also do not find significant unconditional relationships between partisan preferences and message seeking behaviors at bivariate-level analysis as well.

Overall, we do believe this finding should deserve more attention, especially given the fact that many of the prior research in this topic have relied on suboptimal measure of message seeking behaviors, and subsequently conclude that partisan preferences are among the strongest predictor of message seeking behaviors. Overall, we hope that our analysis and conclusion would sufficiently point this matter more clearly.

Third, one very minor point. H7: “The effect of preference homophily in message selection”: “in” seems to be replaced with “on”.

AUTHOR RESPONSE

As suggested, we have changed the wording to “on” instead of “in” for H7.

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