

**Subject:** European Sociological Review - Decision on Manuscript ID ESR-2024-313  
**From:** European Sociological Review <onbehalf@manuscriptcentral.com>  
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23-Feb-2025

Dear Author,

Thank you very much for submitting your manuscript ESR-2024-313 entitled "Class-based network segregation, Economic Inequality, and Redistributive Preferences across societies" to the European Sociological Review.

The reviews of your manuscript are now complete and accompany this message. On the basis of the reviewers' comments and our own reading of the manuscript among the editors, we now invite you to revise and resubmit your manuscript.

As you will see, the reviews we received on this version of your manuscript are mixed. While the reviewers were generally positive about your work, one provided a more critical evaluation, and all raised concerns that require a major revision. In your revision, please pay particular attention to the following points:

- Reviewer 1: issue of (too) many country-level variables in the same model; discussion of alternative interpretation regarding the Gini coefficient effects (also in relation to welfare state size, see R2); issue of income as control variable and its potential posttreatment bias
- Reviewer 2: clearer and more coherent explanation of the theoretical mechanism for the homogeneity of social network for both upper and lower classes; provide an analysis which assesses the extent to which homogeneity level of network exerts an effect that is additional to the effects of own social class and of the social class of network ties (including both worker and service class ties): revise conclusion section given new findings; define social class
- Reviewer 3: a better explanation of the paper added value compared to the contribution of Lindh and Andersson's 2024 ESR; consider comment on isolated vs homogeneous network; provide additional analysis for non-collapsed EGP class categories

We therefore invite you to carefully make the required revisions and also to address all the more specific issues raised by the reviewers. Please explain the revisions and your answers to both editors' and reviewers' feedback in a separate revision memo. If you are able to meet the emerged concerns, the article has the potential to become an important contribution to the literature. If it is not sufficiently revised, of course, the paper would not be publishable in the European Sociological Review. Once we receive back the revised manuscript, it will be sent out for review again.

To revise your manuscript, log into <https://mc.manuscriptcentral.com/esr> and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision. DO NOT "submit a new manuscript."

When completing your revision, please keep in mind that articles should usually be around 8,000 words, including footnotes and references, but excluding tables and figures. For detailed instructions visit our link [https://academic.oup.com/esr/pages/General\\_Instructions](https://academic.oup.com/esr/pages/General_Instructions)

You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript using a word processing program and save it on your computer.

It is important that you explain on the "response to decision" page how you have dealt with the points raised by the reviewers. This SHOULD NOT include the authors' names. These will be read by the reviewers, and this journal uses a double-blind reviewing system. You can use this space to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s). Remember to include a separate title page with the authors' names, addresses and acknowledgements and ensure that your manuscript is clean (ie does not contain any highlighted changes).

Once the revised manuscript is prepared, you can upload it and submit it through your Author Centre.

IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.

The time limit for the resubmission of a revised manuscript at European Sociological Review is 6 months, although we suggest that authors resubmit as soon as possible. If it is not possible for you to submit your revision within 6 months, we may have to consider your paper as a new submission.

Once again, thank you for submitting your manuscript to the European Sociological Review and we look forward to receiving your revision.

Sincerely,  
Prof. Céline Teney  
Associate Editor  
Prof. Fabrizio Bernardi  
Editor in Chief, European Sociological Review

Reviewers' Comments to Author:

Reviewer: 1

Comments to the Author

Review "class-based network segregation, economic inequality, and redistributive preferences across societies"

This article tackles the interesting issue of how networks, inequality, and class work together to shape redistributive preferences. The article is well-written and easy to follow. The hypotheses follow from the literature review and arguments, and the empirical analyses are competently performed.

I do, however, have one (potentially fundamental) concern. The main novelty of the paper is to assess how inequality at the country-level shapes the interaction between class-based network segregation and social class. However, there are only 31 countries. The analyses estimate the independent "effects", at the macro-level, of economic inequality, economic development (GDP) and welfare state type simultaneously, while also estimating the interaction effect between economic inequality and the individual-level interaction between network segregation and class. This is demanding a whole lot from the data, and I'm not sure I would trust the estimates coming out of such a model (even estimating four coefficients with 31 units without an interaction term is probably too demanding). Particularly in light of recent findings about power issues when using country level data, see Doucette 2024. While Doucette's article is from a different field, I think it could be useful for the authors to engage with this fundamental criticism and do some power analyses to show us how many units their analysis would need to reliably discover interaction effects of different magnitudes.

Another but related issue is that of how to interpret the estimates. Income inequality/the gini coefficient is correlated with so many other things at the country-level that it is hard if not impossible to know whether it's indeed the income inequality that is doing the work here, or rather any other variable correlated with income inequality. This is not necessarily a reason not to publish the paper, if this is the best we can do, but I do think it deserves more attention than it is currently receiving. The author acknowledges this problem very briefly towards the end, but I think a more comprehensive discussion would be worthwhile: What other relevant confounders may exist at the country-level? What alternative study designs may cast more light on this phenomenon in future studies (beyond merely saying "longitudinal studies")? What can we as researchers do to gain more insight into the plausibility of the author's main claim?

A couple of other, less fundamental issues:

1) The analyses include controls for socioeconomic characteristics such as income, education, and labor market status (see p. 8). Especially income seems to me to be a source of post-treatment bias, as income follows from class position and in turn affects redistributive preferences. Income is one of the mechanisms through which class may affect such preferences, and so controlling for it may introduce bias in the class coefficient (these kinds of issues have been discussed in several contributions on the link between class and attitudes or party choice). I think it may be worthwhile for the author to justify the inclusion of the controls better in terms of a causal model. Which variables are confounders and which ones are mediators?

2) I would have liked to see more on magnitude and real-world significance of the interaction effect. This is depicted very nicely in Figure 4, but a bit more on the "so-what question" would be good. The findings shown in this figure are the core findings of the whole article, so it would deserve more discussion in the text. What does it tell us about sociological and political phenomena? What's the real-world significance of the differences shown in the different panels?

In summary, an interesting paper, but I'm somewhat unsure how far the evidence can take us with the amount of data available at the country-level. For the paper to be published I think it would need to engage with this issue very explicitly (e.g. by following suggestions in this review or in other ways).

Reference mentioned:

Doucette, Jonathan (2024). What Can We Learn about the Effects of Democracy Using Cross-National Data? American Political Science Review. <https://www.cambridge.org/core/journals/american-political-science-review/article/what-can-we-learn-about-the-effects-of-democracy-using-crossnational-data/>

Reviewer: 2

Comments to the Author

Review of: "Class-based network segregation, Economic Inequality, and Redistributive Preferences across societies".

Summary: The paper argues that homogenous class-based networks foster segregated life-worlds, leading to a larger divide between social classes and diminishing awareness of others' living conditions. Empirically, the paper shows that working-class homogeneity is

associated with higher redistributive preferences, while upper class homogeneity is associated with lower redistributive preferences. Overall assessment: This paper presents an interesting and generally well-crafted analysis of class homogeneity in networks and political attitudes. The authors exhibit strong research skills, and the paper includes a thorough review of existing literature, along with some well-designed figures. However, the empirical contribution is somewhat limited, and the micro-level theoretical framework could be improved.

My main comment developed below is about motivating what I believe to be the problems with the micro-level theoretical model.

Theory and introduction:

Previous research argues that the social class of network members matters for an individual's attitudes for various reasons (e.g., Paskov and Weisstanner 2022). In relation to this literature, the current paper adds a new micro-level theoretical framework focusing on the homogeneity of networks rather than the content or characteristics of network members. Although this is theoretically appealing, I think this theory is underdeveloped and has problems in making coherent predictions.

At first glance, the model focusing on network homogeneity, appears to be parsimonious and symmetric across classes, but it quickly becomes clear that this is not the case. I will first cite from the abstract.

"Homogeneous class-based networks foster segregated lifeworlds, deepening the divide between social classes and diminishing awareness of others' living conditions and perspectives. Specifically, homogeneous upper-class networks may reduce empathy and solidarity toward those in need, lowering support for redistribution. In contrast, homogeneous working-class networks amplify marginalization and strengthen the demand for redistribution."

Two of the mentioned mechanisms, empathy and solidarity, appear to be specific to the upper-class, while the function networks for empathy and solidarity of the working class is less clear. In contrast, the awareness mechanism seems to apply to all social classes. However, it is not evident why the working class's awareness of the living conditions of the upper class (which arises from a less homogeneous network) should lead to reduce the support for redistribution. To me, it seems like awareness of the living conditions of the upper classes would likely increase the demand for redistribution.

In the main paper, the arguments provided in the abstract are not developed further. Instead, we are given the following theoretical motivation for the micro-level hypothesis:

"I hypothesize that the association between network homogeneity and redistributive preferences is conditional on social class because homogeneous social networks should reinforce attitude similarity"

My interpretation of this statement is that it is the similarity (homogeneity) as such that brings the effects, rather than the characteristics of ties or the content of the relation. Here, the argument seems to be that in-group ties reinforce opinions of typical class positions. This particular view is however not fully tested in the paper.

Methodologically, the regression models do not convince me that there is such a specific effect of homogeneity that is different from – or on top of – the coefficients of the class composition of the network. Although models include a control variable measuring network size, there is no control for the class position of ties. This means that the specific effect of a homogenous network is different in terms of class composition for the working class and the service class. A homogenous network for workers means that contacts are workers while a homogenous network for the service class implies contacts with CEOs and lawyers. To show a significant interaction between own class and homogeneity is hence not enough since it just reflects that the homogeneity variable means different things deepening on ego's class position (given the construction of the homogeneity variable). To convince the reader that the interaction effect indeed is about homogeneity rather than class composition of the network, one way could be to run a full interaction model including own social class and the social class of network ties (including both worker and service class ties). A substantially different effect of homogenous ties i.e., a more important effect of worker ties for workers and a more important effect of service class ties for those in service class occupations would strengthen the case that homogeneity of ties is what matters. If effects appear to be largely similar, then it is questionable whether it is the homogeneity/heterogeneity as such that matters.

Finally, I do not think that the following conclusion is supported by the current analysis: "These results support the idea that low cross-class embeddedness can reduce collective solidarity as they limit awareness about the living conditions of other classes". Results however suggest that a low cross-class embeddedness increases "solidarity" for the working class. One interpretation is that authors mean that cross-class embeddedness increases solidarity only for the higher classes, but then it should either be specified that this theoretical model only applies to the service class, or discuss why cross-class interaction reduces solidarity (?) for the working class, which appears to be the result of the models.

In summary, the micro-level theory focusing on homogeneous networks is not consistently presented throughout the paper, leading the author to draw ambiguous conclusions.

I have also some minor comments:

- Social class is not defined in the paper which makes it a bit unclear if social relations are a part of class or not.
- The term upper class is used synonymously with service class. This is a bit problematic since the upper class is often used for a smaller group whereas the service class refers to (in this case) 42% of the population (see Table A5).
- As noted on page 12, Lindh & Andersson use market inequality while the current paper uses GINI post-taxes and transfers. These two are quite different variables and it is unsurprising that they show different results.
- Please clarify why GINI post-taxes and transfers are included in the same model as the welfare state index. How should we interpret the effect of GINI conditioned on the welfare state – as market inequality?
- Cross-class embeddedness is not exactly what the dependent variable estimates, rather, it measures homogeneity and hence whether ties are in your own class or not. This might be a subtle but not unimportant difference.
- The motivation for the descriptive part is (p9-10) largely missing. Please describe the methods and purpose of this analysis. In particular, I do not fully understand the role of Figure 4.
- The heading of Table 2 does not declare the dependent variable.
- I would prefer if model 2 of table 1 should include the respondent's social class.
- If the authors want to convince the readers of the importance of inequality rather than welfare state size, not only their main effect but also their interactions could be entered in the same model.

Reviewer: 3

Comments to the Author

The current study uses 2017 ISSP data from 31 countries to examine the link between network class segregation, country level inequality, and support for redistribution. The most intriguing findings, in my opinion, are the class and inequality contrasts: lower class respondents with more homogenous networks have more support for redistribution, whereas higher class respondents with more homogenous networks have lower support for redistribution. These class contrasts tend to be a bit more pronounced among countries with lower inequality.

Overall, I find this article to be important, sophisticated, and interesting. The results are produced well. The research question is creative and important.

A few questions and comments below:

- This article is very similar to Lindh and Andersson's 2024 ESR, if I'm not mistaken. The difference seems to be a minor variation on measuring class network composition and selecting a few different attitudinal questions. Now, this similarity is not an insurmountable problem, as both Lindh and Andersson 2024 and this paper provide interesting and important findings. But I think that more care needs to be done to explicitly situate what specific value is added by this paper above Lindh and Andersson. Just to reiterate, I think there is value added, but the authors need to make a bit more of a clearer and stronger case.
- Know / don't know. If I'm not mistaken, how do we differentiate the homogenous from the isolate? If I'm not mistaken, a hermit and a highly social but highly cocooned upper class person will both score as highly homogenous networks, since both will not know a police officer, for example. How can we differentiate these two contributors to the homogenous, or at least be confident that the hermit is not driving main results?
- Relationships - If I'm not mistaken, all forms of knowing in any capacity are coded as known. But are there reasons to wonder whether the kin-based and the much looser, simply know in some capacity, categories might indicate something that the article is not interested in addressing? Perhaps I am extremely homogenous in my middle-class network formation, but my two siblings rose to the ranks of lawyer and ceo, as a silly example. Do I have a heterogenous network in the way that would alter attitudes? Perhaps, but perhaps a bit more justification of collapsing these types of knowing is needed.
- I don't totally understand why the EGP categories are collapsed. EGP class categories don't seem terribly overwhelming. Wouldn't it at least make sense to verify that the patterns of association are similar within the collapsed classes?

Overall, this is an important and clever paper. I look forward to hearing the authors' thinking in response to the above comments.