

Ideological biases in research evaluations? The case of research on majority–minority relations

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Abstract

Social science researchers tend to express left-liberal political attitudes. The ideological skew might influence research evaluations, but empirical evidence is limited. We conducted a survey experiment where Norwegian researchers evaluated fictitious research on majority–minority relations. Within this field, social contact and conflict theories emphasize different aspects of majority–minority relations, where the former has a left-liberal leaning in its assumptions and implications. We randomized the conclusion of the research they evaluated so that the research supported one of the two perspectives. Although the research designs are the same, those receiving the social contact conclusion evaluate the quality and relevance of the design more favorably. We do not find similar differences in evaluations of a study on a nonpoliticized topic.

INTRODUCTION

Researchers in the social sciences tend to have liberal and leftwing political attitudes and preferences (Duarte et al., 2015; van de Werfhorst, 2020). In Norway, a recent survey (Fladmoe, 2021) finds that 82% of social scientists support one of the left parties, compared to 44% of the electorate. Moreover, 17% of social scientists disagree that immigration policy should be liberalized,

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compared to 47% of the total population. These associations are consistent with the finding that views on immigration are closely correlated with leftwing voting in the general population (Vernby & Finseraas, 2010).

In this note, we ask whether evaluations of a research project on majority–minority relations reflect the left-liberal perspective that dominates in the field. The query departs from the proposition that a high degree of political homogeneity in a research field might impede the proper functioning of critical peer review or “organized skepticism,” meant to secure scientific objectivity norms such as disinterestedness and accuracy (Merton, 1973 [1942]). The principle of scientific independence from sociopolitical or personal attributes of the researcher has a strong position across the sciences. Hence, ideological bias, if and when it occurs in the social sciences, is linked to unconscious psychological mechanisms (i.e., motivated reasoning, confirmation bias, and groupthink) rather than to intentional violations of scientific principles (e.g., Duarte et al., 2015).

Along these lines, Honeycutt and Jussim (2020) put forward a set of ways that ideological bias can be important (see also Duarte et al., 2015; Jussim et al., 2016). Bias might influence what questions are considered important and how research on sensitive topics is evaluated. Ideological homogeneity might result in unjustified confidence that some issues are settled in favor of the implicit ideological beliefs of the field so that further research on opposing theories is viewed as less important and relevant. If conducted, such research might be met with more critical scrutiny, perhaps published in less prestigious journals, and, for sensitive issues, be viewed as morally problematic. However, basic scientific norms imply that the impact of sociopolitical or personal attributes of the researcher should be minimized through rigorous research procedures and methodology. Due to this norm, ideological bias, if it exists, might be unconscious rather than openly expressed.

Despite much interest in the question, systematic evidence of the effects of ideological bias in academia is scarce. Previous experimental research (Abramowitz et al., 1975; Ceci et al., 1985; MacCoun, 1998) is typically conducted on a small sample of social psychologists, the realism of the experiments varies a lot, and most studies are quite old. There is a lack of studies on how political bias plays out across social sciences more broadly, and we are unaware of any studies conducted in Scandinavian countries. Our study brings new empirical data to a much-debated question.

We conducted a preregistered¹ survey experiment where Norwegian social scientists were asked to evaluate the quality and relevance of a research design on majority–minority relations. The described study is an experimental study on the effects of social contact between refugees and natives. We randomized the conclusion of the study so that half of the sample presented the conclusion that majority members developed “more liberal” attitudes to immigration after social contact with refugees, while the other half presented the conclusion that

majority members developed “less liberal” attitudes. Since the research design is the same in both groups, differences in evaluations imply that they use the conclusion, which is irrelevant information but with ideological implications, to form their research design evaluations. If so, the leftwing sympathies of most researchers make us expect that the “more liberal” conclusion will tap into their beliefs and be evaluated more favorably.

Our results reveal bias in research project evaluations. The “less liberal” study design receives more critical evaluation on both its quality and relevance. We do not have direct evidence that the mechanism is ideology, but we find no significant effect on evaluations of a second study on a less politicized topic, which in our view strengthens the interpretation that the findings are related to ideological bias.

THE SURVEY EXPERIMENT

Design

The respondents were asked to evaluate two fictitious research studies. In one of the two studies, the topic was research on majority-minority relations. A key distinction in the research literature is between social contact theory, which argues that prejudice is driven by fear and misconceptions that can be reduced through social contact, and group threat theory, which argues that social contact can make threats and concerns over immigration more salient. These mechanisms might spill over to views on immigration policy. The respondents have presented the following text:

A group of researchers want to study what shapes anti-immigration attitudes among native-born Norwegians. Drawing on contact and group threat theory, they are interested in whether personal contact between Norwegians and newly arrived refugees will decrease skepticism towards ethnic minorities and make attitudes to immigration more liberal (e.g. because misperceptions about unknown cultures are corrected), or have the reversed effect, leading to increased negativity towards minorities and less liberal attitudes (e.g. because perceptions about the costs of integration change). The research team conducts a randomized controlled trial to study this question. They recruit 160 native-born Norwegians that volunteer to participate in the study. 80 of the participants are randomly chosen to participate in three dialogue meetings with newly arrived refugees. The researchers collect background information about the participants before and after the dialogue meetings. Supporting *group threat theory/contact theory*, the researchers find that, after the dialogue meetings, the 80 participants report on average *less/more liberal attitudes* to immigration.

We randomized the conclusion of the study, as indicated by the italicized text.² Our aim was to describe a sound and realistic research design on a topic with empirical disagreement, see, for example, Paluck et al.'s (2019) review of the empirical support for contact theory, but at the same time not to make the described research design so strong that variation in evaluations would be limited because there is no room for individual judgements. For instance, one might reasonably object to the fairly small sample size of the described study. Others might object to its recruitment of participants. However, these weaknesses are the same for both groups. Finally, for both described studies, objections might be raised against the operationalization of the theoretical perspectives. Some might argue that the contact hypothesis, in Allport's (1954) original formulation, requires more intense and durable social contact to operate and that the design is better described as a test of intergroup interaction (MacInnis & Page-Gould, 2015). Others might argue that conflict theory should be tested by manipulating sociotropic threats rather than individual-level interactions, see, for example, Hainmueller and Hopkins' (2014) review of the literature on immigration attitudes. Since reasonable objections can be raised against both perspectives, our study is not by design biased in one direction.

In the second study respondents evaluated, they were asked to evaluate a qualitative research design on the use of robots in elderly patient care. Again the respondents were given a text describing the research design of a fictitious study of how the use of robots influenced the well-being of patients (see Supporting Information: Appendix for full description). The second study they evaluated is an integrative part of our design that serves several purposes; the main one is to hide the true objective of our study. Moreover, by describing a qualitative design, we avoid that the study appears to be targeted at quantitative researchers. We randomized the conclusion of the robot study to either an increase or a decrease in the well-being of patients so that we can (i) use the responses to the second study to examine if we find effects in the same direction for a less politicized topic, and (ii) to rule out a general bias against negative research results. We randomized the order of the two studies to avoid order effects or selective attrition.

Respondents were told that the purpose of the study is to understand how researchers evaluate research projects. We gave no information that could reveal the true purpose of the study, but we did not use deception since the respondents were told that the described studies are fictitious.³ To avoid priming effects and due to concerns about respondent anonymity, we did not ask questions about political attitudes or other sensitive topics. Since we do a targeted study, anonymity concerns arise if one combines the information from the background questions we ask. The Norwegian Centre for Research Data, our ethics board, therefore advised us not to ask sensitive questions. Since we employ a between-subjects design, the evaluations of the research designs do not reveal ideological bias at the individual level.

Hypothesis

We expect that the group that receives the “less liberal attitudes” conclusion will rate the quality and the importance of the study on majority-minority relations lower than the group that receives the “more liberal attitudes” conclusion. The leftwing sympathies among most researchers might make them susceptible to believe that anti-immigration attitudes are irrational fears that can be addressed by appropriate interventions to increase social contact between majority and minority members (see Stenner & Haidt, 2018, for a related discussion). This narrative might tap into deep-seated beliefs or values among social scientists with a predominant left-liberal outlook (Horowitz et al., 2018). Moreover, Honeycutt and Jussim (2020) and Jussim et al. (2016) identify several related psychological mechanisms that make it possible that this bias will result in a more critical evaluation of the “less liberal” treatment, such as motivated reasoning, myside bias, and confirmation bias. These motivations imply that evaluations will be shaped by predispositions. In addition, ideological homogeneity in the field might shape evaluations also among researchers without strong predispositions, as it might affect what research is praised, seminar discussions, and feedback on own research.⁴ We do not expect to find a similar difference in the evaluations of the robot study, as this topic is not politicized.

Outcomes

The study has two main outcomes. The first is the evaluation of research *quality*. Our measure of research quality is an additive index of three questions on the quality, novelty, and ethics of the project. The second is an evaluation of research *importance*, which is an additive index of three questions on the societal relevance, policy relevance, and the importance of communicating the results to the public.⁵ All evaluations are made on a scale from 0 (low score) to 10 (high score) and the indices are scaled from 0 (low) to 10 (high). Cronbach's α is 0.72 for *quality* and 0.89 for *importance*. The exact question wordings are reported in the Supporting Information: Appendix.

Sample

The population consists of research faculty in Economics, Gender Studies, Geography, Political Science, Psychology, and Sociology at the universities and largest research institutes in Norway. Data collection was conducted online by Kantar with invitations per e-mail. We collected in total 3669 email addresses and received 371 valid responses (10% response rate). One hundred and ninety respondents were assigned the more liberal condition, while 181 respondents were assigned the less liberal condition. The response rate is lower than

anticipated and implies that the study has lower statistical power than we assumed. Descriptive statistics are reported in the Supporting Information: Appendix.⁶ In our sample, 31% of professors and 60% of PhD students are female. These shares are close to those in the population of social scientists (31% female professors in 2016 and 57% females among those who received a PhD in 2019). The same is true for the share of noncitizens. However, we do not claim that our sample is representative of the population of Norwegian social scientists.

EMPIRICAL RESULT

The majority–minority study received on average positive evaluations on both dimensions, with a mean score of 5.5 on quality and 6.6 on importance of the research (Supporting Information: Table A2). While favorably reviewed, evaluations are not too strong or too weak to prevent individual judgements from mattering. The robot study received an equal quality evaluation (5.5), but a slightly more favorable importance evaluation (6.8). Supporting Information: Table A3 indicates that randomization achieved balanced groups.⁷

We run OLS regressions to examine treatment effects. *Less liberal* is equal to 1 if the respondent received the less liberal, that is, the social conflict, conclusion in the majority–minority study. The main results are presented in Table 1, panel A and Figure 1. For both outcomes, we find a significant, negative treatment effect: Those who receive the less liberal/social conflict conclusion evaluate the research design as of lesser quality and to be less important, compared to those

TABLE 1 Treatment estimates.

	Quality index	Importance index
Panel (A) Main outcomes		
Less liberal	−0.432 (0.186) [0.021]	−0.476 (0.226) [0.035]
Constant	5.697 (0.130)	6.822 (0.157)
<i>N</i>	371	368
Panel (B) Outcomes in robot study		
Less liberal	0.145 (0.174) [0.407]	0.198 (0.217) [0.362]
Constant	5.458 (0.122)	6.710 (0.151)
<i>N</i>	371	365

Notes: High scores on the Quality index variables mean a positive evaluation of the research design, while high scores on the Importance index variables mean a positive evaluation of the importance of the research project. Less liberal is an indicator variable for random assignment to the less liberal conclusion of the study on majority–minority relations. Standard errors are given within parenthesis and *p* values within brackets.

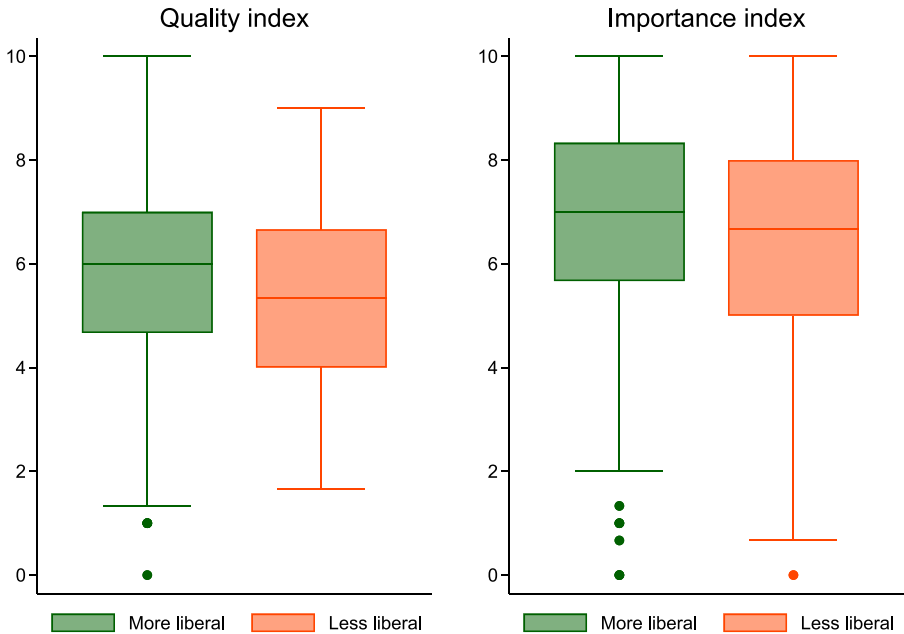


FIGURE 1 Box and whisker graphs. The figure shows box and whisker graphs for the two groups. The boxes are defined by the upper and lower quartiles, while the lines inside the boxes are the median values. The lines (whiskers) are defined by the spread of the data, while the dots represent unusual values. A high score on the *Quality index* means a positive evaluation of the research design, while a high score on the *Importance index* means a positive evaluation of the importance of the research project. [Color figure can be viewed at wileyonlinelibrary.com]

who evaluate an identical research design, but with support for social contact theory.⁸ The effect sizes are 24% ($0.43/1.80 = 0.24$) and 22% ($0.48/2.17 = 0.22$) of the outcome standard deviations. Very few respondents pick the extreme ends of the scale, so treatment does not cause extreme polarization. Supporting Information: Table A7 shows that the treatment effects are particularly large for evaluations of the ethics of the study and the importance of communicating the results, while the treatment effect estimate is close to zero for the novelty question.

Next, we use the evaluations of the nonpoliticized, qualitative, robotization study to help interpret the results. First, we use the evaluations of the second study as alternative outcomes, that is, we run the same regressions but replace the evaluations of the minority–majority study with the evaluations of the robot study. Large effects in these regressions suggest that those who received the “less liberal” treatment in the minority–majority study differs from the “more liberal” group on important characteristics that are not captured by the

background characteristics. Reassuringly, panel B in Table 1 presents treatment effects that are insignificant and much smaller in absolute size.

Second, one concern is that we pick up a classic framing effect (Tversky & Kahneman, 1981) by the negative wording (“less liberal attitudes”) of the conflicting treatment, that is, that research with negative frames is in general evaluated less positively. To study this issue, we examine the effects of receiving the conclusion that the use of robots decreased rather than increased the well-being of patients. Thus, we evaluate the effect of the conclusion they received in the robot study on the evaluations of the robot study. In Supporting Information: Tables A6 and A8, we find much smaller and insignificant treatment effects, which suggest that negative results bias is not very important.

Third, some might argue that a positive effect of a new policy intervention is more important to communicate to the public because it can be scaled up and then improve human well-being (Kasy, 2021), while the policy implication of the negative effect is less obvious. Such rational, nonideological, reasoning cannot explain different evaluations of research quality, but can explain why we find that the less liberal conclusion is considered less important to communicate. If such reasoning in favor of positive effects of scaleable interventions is widespread, we should expect a similar effect of the negative treatment (decrease well-being) in the robot study on the communication question.¹⁰ Supporting Information: Table A8 shows that this is not the case, which weakens this interpretation of the less liberal effect.

When outlining our hypothesis, we argued that a more critical evaluation of the social conflict theory could be due to ideological bias at the level of individuals or social science disciplines. Since we do not have information on respondents' ideology we cannot directly test these claims. Moreover, the sample size is small, which means that we lack the power to test treatment heterogeneity.¹¹ Nonetheless, we present two heterogeneity tests, neither of which were prespecified, that are motivated by findings in another survey of Norwegian social scientists (Fladmoe, 2021). Results from this survey (see Supporting Information: Table A9) show (i) that male social scientists have significantly less liberal immigration policy preferences than female social scientists, and (ii) that researchers with PhDs in Economics and Political Science have less liberal immigration policy preferences than researchers with PhDs from other social science disciplines. Based on these findings one might expect smaller treatment effects for men and economists/political scientists if ideology plays a role. Using interaction terms to test these expectations, we indeed find that the estimated treatment effects are smaller for male respondents and respondents with PhDs in Economics or Political Science (Supporting Information: Table A10). However, none of the interaction terms is statistically significant, thus we cannot draw strong conclusions from these results.

CONCLUSION

Our survey experiment shows that in our sample of Norwegian social scientists, researchers evaluate identical research designs on majority–minority relations differently depending on the stated conclusion of the studies. Our interpretation is that researchers use information that is irrelevant to evaluate the quality and importance of a study's research design. We believe this partly reflects ideological bias: The two different conclusions presented are likely to tap into deep-seated beliefs that dominate among social scientists. This triggers critical scrutiny when results go against those beliefs and ease critique when results align with beliefs. Following Honeycutt and Jussim's (2020) model of political bias, this could have consequences for what research questions are asked and considered important to fund and publish. Like the general public, researchers are susceptible to bias in their evaluations and reflect on them.

We stress that our results and interpretation should be treated with caution. First, the response rate is low and the sample might be biased in ways we are unable to account for. We do not claim that we have a representative sample of Norwegian social scientists, which means that external validity might be low. Second, since we refer to distinct research traditions (conflict and contact theory), some might evaluate the less liberal study design poorly because they implicitly associate the contact theory tradition with superior research designs and not because of ideological reasoning.¹² Third, while we use evaluations of the robot study to argue against alternative interpretations of the communication result, we cannot rule out that for this particular outcome, respondents use the study conclusion in some form of Bayesian learning process without having a political motivation (Kahan, 2015). Fourth, we do not study behavior and cannot know whether the bias we find is sufficient to influence behavior, particularly not in a real-world setting.

Future research should replicate and extend our work in several directions. The estimated effect size we find is much larger than we assumed in our power analysis (-0.25), which explains why we get a significant effect even with a smaller than anticipated sample size. Replications would therefore be particularly useful, but extra work has to be done to recruit more participants, to ensure that the study has sufficient statistical power, and treatments should be pretested to strengthen the basis for interpreting results as representing ideological bias. Comparisons across countries, disciplines and individuals' value orientations can yield further insights into the role of political conformism. Furthermore, there is a need to develop research designs that allow for a more careful assessment of the potential mechanisms at work; for instance, how ideology might play out in different ways: Is it the case that research results that go against dominating ideological views in the field are the targets of inaccurate, unscientific critique? or Is it the case that results that align with ideological views are viewed as of good quality and high importance,

despite weaknesses in research designs? Finally, future research should examine whether ideological bias exists in peer review, project evaluations, and hiring decisions, situations where researchers have more information to base their decisions on.

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ENDNOTES

- ¹ The pre-analysis plan was submitted to the EGAP registry and can be accessed here: <https://osf.io/t29d4>. The approval from our ethics board does not allow us to share data. We encourage researchers to contact us if they want additional information about the design or the data; for instance, to replicate or conduct meta-analyses.
- ² Technically the randomization was done by Kantar Norway, which administered the data collection. Each respondent had a 50% chance of being randomized to either of the two groups.
- ³ The project was approved by the Norwegian Centre for Research Data (case number 617267).
- ⁴ We did not pretest whether the two treatment conditions are indeed perceived as less or more liberal by researchers.
- ⁵ We use the approach of Kling et al. (2007) to handle missing responses to components of the index. Treatment status is not significantly correlated with missing answers on the outcome variables (see Supporting Information: Table A5).
- ⁶ The age question was erroneously left out of the questionnaire sent to respondents. Forty percent answered that they are full professors (or equivalent), 29% that they are Associate Professors (or equivalent), 20% are PhD students, 6% are postdocs, while 5% have answered “Other”.
- ⁷ We conduct the balance test since errors in randomization might happen. While imbalance can occur also if randomization is implemented correctly, a massive imbalance would be a reason for further investigation. We find a significant treatment-control imbalance for the missing indicator for the year of PhD. Supporting Information: Table A4 shows that treatment effect estimates are very similar to the main results when we control for this covariate.
- ⁸ The critical values if we correct for testing multiple outcomes (Benjamini & Hochberg, 1995) are 0.025 (quality index) and 0.05 (importance index). Both p values are below these values.
- ⁹ This will not necessarily be the case if respondents consider the minority-majority study as of high quality (strong signal) and the robotization study as poor (weak signal), but this is not the case here, since the two studies receive equal ratings on average (Supporting Information: Table A2).
- ¹⁰ The preplan specified to use the machine learning approach of Chernozhukov et al. (2019) to search for treatment heterogeneity. Using this approach, we find no significant treatment heterogeneity across the background characteristics we examine.

¹¹ In light of Paluck et al. (2019), such associations might, however, be influenced by ideological homogeneity within the discipline.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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