

Subject: (International Journal of Social Psychology) A decision has been made on your submission
From: Revista de Psicología Social/International Journal of Social Psychology <em@editorialmanager.com>
Date: Tuesday, April 4, 2023 18:37 CEST
To: Julio Iturra <jiturra@bigsss.uni-bremen.de>
Reply-To: Revista de Psicología Social/International Journal of Social Psychology <rev.psicologiasocial@ugr.es>

CC: gwillis@ugr.es, t.kuppens@rug.nl

Apr 04, 2023

Ref.: Ms. No. RPPS-2023-0009
Subjective status, Social Class and meritocracy perception
International Journal of Social Psychology

Dear Mr Iturra,

Thank you for submitting your manuscript 'Subjective status, social class, and meritocracy perception' to the special issue of IJSP, edited by Matt Easterbrook, Lusine Grigoryan, and myself.

I was fortunate to receive two excellent reviews from experts in the area of socio-economic status and meritocracy. They both provided extensive and constructive comments and I would like to thank the reviewers for the time they dedicated to helping me in evaluating this manuscript. I also read the manuscript carefully myself, and so did my co-editors.

We all agree that the topic you are investigating is interesting. However, it is also clear that the manuscript has important weaknesses, that I elaborate on below. We have a very tight schedule for the special issue. Given the extent of the changes that would be required, I doubt that you would be able to implement them on time. I therefore regret to inform you that I have decided to reject your manuscript and not invite a revision.

My main concern is the lack of theory and reasoning for the hypotheses. This is clearly articulated by both reviewers and there is no need for me to repeat what they wrote. What I thought was crucial in this regard is the potentially interesting moderation by subjective status, but the lack of theoretical reasoning or interpretation of this effect. The moderation effect is what could have been the main contribution of the paper and it is certainly an intriguing result on a topic that remains underdeveloped ('What exactly is capture with a measure of subjective social status?'). So I would like to emphasise that empirically, there is potential in these data. However, without theoretical reasoning it is unclear what readers can learn from the manuscript. You do cite the Evans and Kelly (2017) paper in support of the moderation hypothesis, but that paper is about perceptions of inequality, not meritocracy. Because of this, and other issues with the introduction, you would need to fundamentally rewrite it and that would not be feasible within the timeline for the special issue.

Both reviewers also refer to a lack of care in the preparation of the manuscript and I agree with them. Table 2 is unreadable, the Introduction could be streamlined, and the Discussion lacks depth. I also suspect from reading Table 2 that you did not center your variables before estimating the moderation, which is standard practice (in social psychology and sociology). Again, a lot of work is needed to make this into a publishable manuscript.

The reviewers offer many other constructive comments that will be a great source for you as you develop this work further.

In addition to strengthening the theoretical rationale, I think you also need to look for additional data. Reviewer 2 mentions a potentially interesting dataset in this regard. Furthermore, the key variables you use are also available in datasets such as the ISSP. You might really be onto something here that has the potential for a strong contribution to the literature. Despite the present negative outcome, I wish you all the best with this line of research.

Yours sincerely,

Toon Kuppens
Guest editor

Reviewer #1: This study present various correlates of meritocracy perceptions, using nationally representative survey data from Chile. I hope that the suggestions below will aid the authors in further developing their work.

Before I provide my substantive feedback, I would like to emphasize that the manuscript would benefit from being more carefully prepared.

a) Without page numbers it is harder to give specific feedback.

b) Tables 1 and 2 are essential to the manuscript, but spread out over multiple pages and therefore almost impossible to read.

My most general and most important feedback is that the study would benefit from being more clear about its theoretical contribution. Various correlates of perception of meritocracy are presented. The key question is: why? Especially because these have already been described in previous studies (some of which are referenced).

In fact, in key parts of the manuscript, the main reason to expect a correlation seems to be that prior research also found this correlation. An example is: "Castillo et al. (2018) found that higher subjective status is related to higher perceived meritocracy. Based on this evidence, our main prediction is: 1: subjective social status is positively associated with perceived meritocracy". What do we learn from the present manuscript that is not already known? This should be thoroughly discussed throughout the manuscript (and it currently underdeveloped in both the introduction and the section that presents the hypotheses as well as the discussion and conclusion).

Closely related to the observation above: the rationale for some key hypotheses seems to be that other empirical studies found the same or similar empirical relationships. But what about the **theoretical mechanisms**? I feel the manuscript could be greatly improved by more systematically theorizing throughout the entire manuscript why one would expect the relationships that are central to the hypotheses.

The rationale for the inclusion of the moderation hypothesis mainly rests on observations from extant studies about economic **inequality** that is moderated by subjective status. Since the present study does not measure inequality, the argument could be more precise here.

In general, it seems vital to include more relevant theorizing. Especially because the present study criticizes prior work for a lack of "an extensive dialogue" between social psychology and sociology, it struck me that relevant theorizing at the intersection of these disciplines could be developed much more strongly. The work of Spruyt and Kuppens (e.g. Spruyt and Kuppens, 2015a; 2015b) might for instance be relevant here, as it engages with Bourdieusian theorizing that suggests that status differences are institutionalized, and therefore legitimized among both low and high status individuals, which would predict the absence of a relationship between objective status indicators and perceptions of meritocracy. In addition, given the importance of objective and subjective indicators, it would be worthwhile to engage with a recent paper by Mijs et al. (2022).

A more thorough engagement with the theoretical implications of the study's findings would also aid in improving the discussion and conclusion. These now mainly address with which prior research the present findings are / aren't in line. It would be helpful to reflect more (and more overall than focusing on individual correlates) on what we learn from the present study about the relationship between social position and meritocracy perceptions.

Some statements require more attention. "higher-income individuals perceive higher meritocracy because they have accumulated economic resources through effort and talent". It would be good to make explicit whether they **believe** to have accumulated their resources through effort and talent, or whether this is a fact. The text suggests the latter, sociological insights on structural inequalities suggest the former.

Empirically, it is important to provide a rationale for the inclusion of controls. For age and gender this might be quite straightforward, but it is not self-evident that political identification and perceived salary gap need to be included. I recommend: a) provide an explanation for the inclusion of each control variable (and the exclusion of others); b) reflect on the potential implications of including specific controls (like 'overcontrolling', which might e.g. be relevant in case of political identification, which may also be a result rather than a cause of meritocracy perceptions).

The authors argue that the effect of subjective social status is strong. I would like to see a more elaborate discussion of the substantive effect size. Is an effect of 0.05 really a strong effect, with meaningful implications?

Minor issues:

- It might be good to reflect on your description on meritocracy as an "ideal" while referring to the seminal book by Young, which describes a dystopian society.

- Some arguments are hard to follow. Some examples:

- o a statement about "social interactions" follows a sentence including rather static status characteristics.

- o The "However" in the following part is hard to understand: "the poor are less critical of dominant beliefs like meritocracy as a psychological mechanism for coping with deprivation (Solt et al., 2016). However, other research shows that low-income individuals in highly unequal contexts have firmer meritocracy beliefs, but the differences tend to fade as inequality declines"

- o "with higher-status individuals showing more variation in subjective status" is hard to follow

References

Mijs, J. J., Daenekindt, S., de Koster, W., & van der Waal, J. (2022). Belief in meritocracy reexamined:

Scrutinizing the role of subjective social mobility. *Social Psychology Quarterly*, 85(2), 131-141.

Spruyt, B., & Kuppens, T. (2015). Education-based thinking and acting? Towards an identity perspective for studying education differentials in public opinion and political participation. *European Journal of Cultural and Political Sociology*, 2(3-4), 291-312.

Spruyt, B., & Kuppens, T. (2015b). Warm, cold, competent or incompetent? An empirical assessment of public perceptions of the higher and less educated. *Current Sociology*, 63(7), 1058-1077.

Reviewer #2: The manuscript examines the relationship between several SES indicators (i.e., income, education, occupation, and social class) and perceived meritocracy in Chile. Using cross-sectional data from a longitudinal survey from Chile, the authors found that perceived meritocracy was positively associated with subjective socioeconomic status and negatively associated with education; while the relationship with income and occupation was not statistically significant. Furthermore, the authors found that subjective socioeconomic status moderated all the effects between objective SES indicators with perceived meritocracy.

In general terms, the authors provide an interesting view of meritocracy perceptions, which is not commonly addressed in the literature. In addition to the difference between perceptions and preferences, the authors try to disentangle how indicators of SES can influence people's perceptions of the meritocracy that exists in their reality. The nature of the data is also relevant, since it brings information from Chile, a paradigmatic case of extreme economic inequality from the Global South, with high levels of social unrest and political mobilization. For the sake of improvement, however, I'd suggest taking into account some of the following points before publishing this research:

- The theoretical foundation of this research could be developed further. In the current state, the introduction presents some definitions and empirical findings, but I couldn't find an integrative theoretical framework that back up the authors' hypotheses. Perhaps I missed something, but I'd suggest being careful about combining results and theorizations from inequality perceptions, redistributive preferences, and meritocracy beliefs when it comes to explaining the relationship between SES and meritocracy perceptions. Although they might be somehow correlated, in my view, they are still very different in terms of nature, antecedents, and implications. Therefore, I'd suggest the authors try to differentiate those concepts very clearly in the introduction, or just stick to the information that concerns their core ideas (i.e., SES and inequality perceptions).
- In line with the previous comment, how do the authors understand the nature of meritocracy perceptions? are these mere descriptions of reality, or are they ideologically motivated? The authors differentiate between descriptive and normative meritocracy, but they use a framework from Janmaat that systematizes inequality perceptions, which is different from meritocracy ideas. The paper from Castillo et al (2018) seems very relevant here since they show differences between what they call perceptions (descriptive) and preferences (prescriptive) meritocracy. However, they do not provide a clear theoretical framework for understanding the nature and antecedents of meritocracy perceptions. Thus, I just want to draw attention to social psychological literature that addresses this theorization, such as belief in a just world, system justification, social identity theory, etc. (e.g., <https://doi.org/10.1037/a0024618>, <https://doi.org/10.1111/j.0963-7214.2005.00377.x>, https://link.springer.com/chapter/10.1007/978-1-4899-0448-5_2, <https://doi.org/10.1002/ejsp.2925>, <https://doi.org/10.1016/j.jesp.2017.11.001>, <https://doi.org/10.1177/0001699317748340>). Of course, these references are given just in case the authors find them useful to strengthen their theoretical claims.
- What do the authors understand by "subjective meritocracy"? Given the argument of the authors about the mixture of terms when it comes to meritocracy, perhaps they might want to be more specific about the use of this term.
- Regarding subjective socioeconomic status, perhaps the authors find it useful to double-check the research from Brown-Iannuzzi (e.g., <https://doi.org/10.1016/j.copsyc.2017.06.018>)
- Could you please expand a little bit more about the "group and reality blend hypothesis"? This seems an interesting hypothesis, but it wasn't clear to me what is the theoretical foundation of these claims.
- The results are consistent but should be discussed further. I found really interesting the consistent findings shown by the authors. That is, the greater the SES, the more perceived meritocracy for the people that place at the top of the social ladder. However, the effect sizes seem to be very small, which should be discussed in the manuscript. Indeed, the correlations between SES and perceived meritocracy are pretty small, and the interactions seem to be even smaller.
- The interaction plots are very illustrative for delivering the message of the manuscript, but I'm wondering if the chosen levels of the moderator (i.e., 0 and 11 in the Subjective socioeconomic status) are fair to depict such interactions. I mean, very few people placed at the bottom, and even fewer placed themselves at the top of the ladder. People usually show simple slopes analysis (i.e., 1 or 2 SD above and below the mean), or just plot the conditional effect over different levels of the moderator. This comment is made just for aiding to acknowledge some of the nuances of the research that were not discussed in the manuscript. Perhaps the differences at other levels of the moderator (e.g., 2-3 vs. 7-8) might not be able

to appreciate.

- I'm curious why the authors did not take the opportunity of using the longitudinal data from COES to test their hypotheses. I'm not asking to do that, but it seems to me like a gold opportunity to provide more robust findings. Perhaps the effects are found cross-sectional but not longitudinal, which can be an interesting finding in itself due to the stability of the meritocracy perceptions over time.

Minor (but also important issues):

- The tables are hard to read. I value the intention of rendering reproducible documents (as this might be the case), but the tables and figures are out of margins and it is not easy to follow the logic of variables, coefficients, etc.

- Why did the authors include the perceived salary gap and political identification as covariates? Is there any reason for thinking that those variables are related to meritocracy perceptions? A brief mention of this rationale might be of help to the reader.

- Why did the authors use the two indicators of perceived meritocracy separately in the correlation matrix when they assume they form a single construct? Perhaps they can simplify this result by using the composed measure of perceived meritocracy.

- In addition to the plots depicting the interaction, perhaps it would be useful to report the regression coefficients (either in a table or in the text) at different levels of the moderator.

All in all, I think the research is very interesting and makes a good contribution to the literature. I'd say that strengthening the theoretical discussion and taking care of some details in the results and method section will improve the paper substantially.

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