

## **Reply to decision letter Round 2 : RR #559**

Dear Recommender,

We would like to thank you and the reviewers for taking the time to reread our manuscript and for your constructive feedback.

Below, we have copied your comments and those of the two reviewers and explained the changes that we have made to our manuscript. Please note that the recommender's and reviewers' comments are in normal script, while our answers are underneath in bold.

We hope to submit a stronger Stage 1 manuscript. We are ready to answer any other questions or comments if necessary.

All the best,

Yara, Constantina & Béatrice

**A track-changes comparison of the previous submission and the revised submission displayed side by side can be found on: <https://draftable.com/compare/ZCLNxkuYcvlx>**

**A track-changes manuscript is provided: <https://osf.io/r3tcy>**

## **Response to Recommender: Anna Elisabeth Fürtjes**

Thank you for your revised submission to PCI RR. The two expert reviewers kindly agreed to re-evaluate the submission. Their comments are positive but still require clarifications as can be seen below.

The power analyses as revised, however, are not satisfactory. For example, you have indicated the same power for the two main effects in your ANOVA as for the interaction effect (first three rows in the design table). You will need to specify a roughly smallest effect of interest for each test in the table (note that main effects and interaction effects will have different expected effect sizes), which will also require arguing why this smallest effect is relevant for that particular test. Simply picking the interaction effect found in a previous study is inadequate for two reasons: (1) it is only relevant to the interaction effect (not the main effects), and (2) it is not the smallest effect you wish to make sure you don't miss. One way of getting a roughly smallest effect is to put an 80% confidence interval on the effect from the relevant past data you have available, and choose the bottom limit to calculate power. This heuristic and the logic behind it is described in the paper I referenced in my first decision letter (<https://doi.org/10.1525/collabra.28202>).

**RE/ Thank you for this clarification. We have tried to improve our power analyses in the present version of the manuscript. First, we reduced the design table on page 5 to a smaller set of effects. We only kept the effects that are central to our research questions. Second, we defined the smallest effect size of interest (SESOI) for the analysis that tests our main hypothesis, that is the interaction effect between self-affirmation and secularism representation (last row). We considered  $f = .10$ , as the SESOI for this analysis (reasons explained below). Then we conducted an a-priori power analysis to estimate the required sample size for a mixed ANOVA with 6 groups,  $\alpha = .05$ , and power = .95. The required sample to detect an effect size of  $f = .10$  was  $N = 504$ . We decided to expand our sample to  $N = 600$  to minimize the risk of overestimating the true population effect size and to account for potential exclusions (Perugini et al., 2018). Afterward, we specified a SESOI for each test in the table and explained why each one is relevant for the corresponding test. Finally, the sample of 600 participants was used to compute the power to detect each SESOI in the table.**

We consider the smallest effect size of interest for the critical interaction in our study to be  $f = .10$  for the following reasons. Our main hypothesis pertains to the interaction effect between self-affirmation and secularism representation on prejudice towards Muslims. In a previous pilot study, we found an interaction effect size of  $f = .10$ . However, the study design was different, and, as emphasized by Albert and Lakens (2018), relying on one pilot study to set the SESOI is not effective. Indeed, no other studies to our knowledge have tested this effect as our study is an original one, and not a replication. Therefore, we did not compute an 80% confidence interval from this past data. Given the absence of empirical data concerning our interaction effect, we were unable to identify clear criteria to set our SESOI. We thus decided to set this effect according to Cohen's criteria, corresponding to a small effect size of  $f = .10$ .

## References

Albers, C., & Lakens, D. (2018). When power analyses based on pilot data are biased: Inaccurate effect size estimators and follow-up bias. *Journal of experimental social psychology, 74*, 187-195.

Perugini, M., Gallucci, M., & Costantini, G. (2018). A Practical Primer To Power Analysis for Simple Experimental Designs. *International Review of Social Psychology, 31*(1). <https://doi.org/10.5334/irsp.181>

## Response to Reviewer #1 : Sauro Civitillo

I thank the Authors for addressing the issues that I raised during the first round of review. Overall, I believe the revised version has significantly improved from a theoretical standpoint, offering a stronger rationale for including ideological malleability.

In the revised version, I also note that the Authors have paid stronger attention to several methodological aspects (e.g., power analysis, manipulation checks, and assessment of different variables, including their DV). Thus, I am satisfied with the revision and wish all the best to the Authors for the upcoming data collection phase.

**RE/ We thank you very much for your helpful feedback.**

## **Response to Reviewer #2 : Pete Harris**

I read the revised draft with great interest. The authors have clearly been very responsive to our initial reviews. As a result, most of my points are really comments rather than proposals for changes.

### 1) The interaction hypothesis.

I am still not as clear as I would like to be about why self-affirmation should increase prejudice when the individual affirms using a “new” representation of secularism, but I appreciate that the authors do attempt to explain this more clearly in this version. The explanation, as I understand it, is that “new” secularism enhances social hierarchy and, by making this representation salient during the value affirmation phase, the result is increased prejudice: “if the ideology’s representation enhances social hierarchy, self-affirmation can increase prejudice” (p. 14). I am afraid I do not really follow this reasoning, but I realise this is not really my zone of expertise.

**RE/ We understand that the reasoning behind this hypothesis is still not clear enough. To clarify why we expect self-affirmation on the new representation of secularism to increase prejudice, we added the following on page 15 : “Since new secularism places limits on religious practices and the display of religious affiliation, affirming this value could seem conflicting with expressing tolerant attitudes towards people that are concerned, religious groups. Being reminded of this value should lead to the perception of people that display their religious affiliation as incompatible with French values, thus perceiving these individuals as a threat to French society, and expressing prejudice against them, especially when they are members of a minority religious group. Muslims in particular can be targeted under this norm, as wearing visible religious symbols (e.g., headscarf) is common.”**

### 2) The theoretical implications of the related-value condition.

The authors develop the study out of conflicting findings around the impact of self-affirmation on prejudice. However, one effect of the related-value condition that results from their theorising about potential moderators is to blur the distinction between self-affirmation processes per se (where the content of the value is irrelevant to the effects)

and value-induction processes (where the content of the value is fundamental to the effects). This distinction isn't really addressed in the current draft.

Interestingly, the inclusion of an additional self-affirmation manipulation, assessing an unrelated value (humour) in the revised design may help with the interpretation of this issue, so I turn to that next.

**RE/ Thank you for this comment. We tried to address the distinction between the two self-affirmation processes on page 18 : “We hypothesized that participants who self-affirm on a threat-unrelated value will express lower prejudice compared to control, regardless of their secularism representation, as self-affirmation will buffer the psychological threat causing the expression of prejudice. On the other hand, we expected different results when self-affirming on secularism, as the affirmed threat-related value will moderate the buffering effect of self-affirmation. In this case, the content of the affirmed value is fundamental to the effect (unlike the affirmation of a threat-unrelated value, where the content of the value is irrelevant), and can increase the perceived threat when the value is intolerant or decrease it when it is tolerant, consequently increasing or decreasing prejudice.”**

3) The additional experimental and modified control conditions.

I appreciate the authors being so responsive to my previous comment about affirming on secularism that they have added a manipulation in which the self-affirmation is unrelated to the threat. I am not sure that I would have gone that far, but seeing as they have – and that they therefore deem it worthwhile in terms of the extra recruitment this entails – it is worth examining some of the implications of this.

First, in order to guarantee that the chosen value is unrelated to the threat, the manipulation constrains the participant's choice of value. This, of course, means that the value being affirmed (humour) may not be sufficiently important to the individual to offset the psychological threat. This is common to both self-affirmation conditions.

Second, having an unrelated value does potentially shed some light on the issue I raise above (point 2). If “pure” self-affirmation processes are involved, then the effects should be the same irrespective of the value. If the sort of additional cueing effects postulated by the

researchers in proposing secularism as the moderator are involved, then the effects should differ between the two conditions. I assume this is what underlies the prediction that the effects of humour on prejudice will be weaker than the effects of secularism (p. 17, p. 30) – but the reasoning underlying that prediction is currently not spelt out.

The researchers have also amended the control condition, so that it now involves a value task and is closer to a control task typically used in self-affirmation research. I share their hunch that physical endurance (the control value) will be low in importance for most respondents. The new manipulation check will reveal whether this is the case. Indeed, much now depends on this manipulation check, so I turn to that next.

**RE/ Concerning the first point, we are aware that imposing a value in the self-affirmation task is risky in terms of the importance of this value for the participant. However, we measure the importance of the value proposed in each experimental condition, which will allow us to verify that the importance of the values used in the two self-affirmation conditions is on average higher than that of the control value.**

**Concerning the second point, we tried to present the reasoning more clearly in the present draft. We rephrased the hypotheses on page 31 : “We expected self-affirmation on humor (the threat-unrelated value) to decrease affective and behavioral prejudice against religious groups compared to control, regardless of the chosen secularism representation, since the process of self-affirmation should protect from any perceived threat leading to the expression of prejudice. On the other hand, we expected self-affirming on the threat-related value to be affected by the content of the value. In other terms, affirming one’s representation of secularism should moderate the protecting effect of the mere self-affirmation process.” (see pages 18 and 31 for changes)**

4) The new manipulation check for self-affirmation

I appreciate that the authors have introduced a manipulation check (p. 22) for self-affirmation at my suggestion, but I am concerned that the way they have chosen to do this will be too intrusive. I wonder if they might instead consider using a single item assessing value importance (as is used in many self-affirmation studies) and, if they really want to know about relative value importance to the individual respondent, measure the three items retrospectively? Let me explain.

First, to be clear, a value-importance measure as used in a standard self-affirmation study is essentially a fidelity check. That is, in experiments in which the control condition asks participants to choose their least important value it can be used to check that at least the value chosen by the experimental and control participants differs in importance. Whether that results in the former being self-affirmed and the latter not, of course, is another matter.

The analyses of the new manipulation checks (p. 25) test for relative value importance. It will certainly be useful to know whether physical endurance is less important than either secularism or humour, but I don't see why we should expect secularism to be more important than humour? Indeed, it may be a problem for interpretation if it is. (As an aside, it is interesting to see if being asked to self-affirm on a value boosts that value's importance, but that is a separate issue.)

Testing for value importance could, it seems to me, be achieved more simply by just adopting the more typical procedure of simply asking how important the value the participant was asked to consider (be it secularism, humour or physical endurance) is to the participant. (The researchers would, of course, need to bear in mind that this does not tell us whether someone is self-affirmed.)

If the researchers are really interested in knowing the relative within-person ratings of all three values, they could consider getting these ratings towards the end of the study, by asking them retrospectively

However, my main concern about doing it as currently described is that it may be too intrusive to ask participants to rate three values in between the manipulation and the dependent measures. All that is required, it seems to me, is to ask them how important to them is the value they used.

**RE/ Thank you for this comment. First, following your suggestion, we will keep one item assessing the affirmed value's importance in each condition (i.e. the importance of the humor value in the self-affirmation on humor condition; the importance of the secularism value in the self-affirmation on secularism condition; and the importance of the physical endurance value in the control condition). Second, we will keep in mind when interpreting the findings that this manipulation check does not inform us if the participant is self-affirmed or not. However, it will be informative concerning the importance accorded to each of the used values.**

To analyze this manipulation check, we propose to conduct an ANOVA 3 (value: secularism, humor, physical endurance) between subjects with the importance of the value as the dependent variable. We expect the importance of the values of secularism and humor to be higher than that of physical endurance. We modified the corresponding part in the results section as follows on page 26: “Secondly, to verify that the affirmed values were considered important to participants, we ran an ANOVA 3 (Value: secularism, humor, physical endurance) between-subjects with the importance of the value as the dependent variable. Results did/did not indicate a significant difference in value-importance between conditions,  $F(X, XXX) = X.XX, p = .XXX, \eta^2 = XXX$ . The secularism value ( $M = X.XX, SD = X.XX$ ) was/was not significantly perceived as more important than the physical endurance value ( $M = X.XX, SD = X.XX$ ),  $p = .XXX, d = .XX$ . The humor value ( $M = X.XX, SD = X.XX$ ) was/was not significantly more important to participants than the physical endurance value,  $p = .XXX, d = .XX$ . The difference between the importance of the secularism value and that of the humor value was/was not significant,  $p = .XXX, d = .XX$ .”

#### 5) Distributive matrices

I appreciate that the changes to the dependent measures have been introduced in response to feedback from both reviewers, but I have some concerns about the extent to which the behaviour matrices will be sensitive to the self-affirmation manipulation. These matrices seem potentially quite complex and involved cognitively and I am not sure the manipulation, which is relatively subtle and potentially time limited, will be sufficiently robust to have an impact on the individual's responses to it. I am not suggesting the researchers change this element of the design but they should bear this issue in mind when interpreting any null findings.

**RE/ Thank you for pointing this out. We realize that observing differences on the distributive matrices as an effect of the subtle self-affirmation manipulation will be relatively difficult, and we will bear this in mind when interpreting the findings. Nevertheless, we hope that the use of relative scores of prejudice (affective or behavioral) instead of direct scores, and the comparison between two religious groups, will make our measures more sensitive to subtle differences.**



6) Other issues:

P 25-26 Do the authors mean “effective” rather than efficient?

**RE/ Yes, this has been corrected now.**