BJSP1610
"Group narratives in the Context of Dramatic Social Change in South Africa"
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Dear Professor de la Sablonnière

Thank you for submitting your paper on 'Group narratives in the context of
dramatic social change' to the BJSP. I apologize for the lengthy delay in
processing your paper - longer than we aim for -- but I'm glad to say that
we have two completed referees' reports (a third reviewer defaulted). I am
ready to proceed to a decision. In brief, I think the paper has a lot of
promise, but also requires quite a bit of revision. I therefore want to
offer you a chance to re-submit an amended version for further
consideration. As always, however, I must remind you that publication is by
no means guaranteed. I will, however, try to expedite any future
decision-making process in light of your long wait on this one.

The reviews below offer a detailed assessment of your paper, and I hope that
you find them helpful in developing your work. I want to add some brief
comments of my own. First of all, let me say what I liked about your
general argument: (1) the paper is generally well-written and coherent; (2)
the focus on how members of ethnic groups evaluate their socioeconomic
status/Relative Deprivation over time and under conditions of historical
change is important; (3) the choice of the South African context is fitting
(the fall of apartheid was a global as well as a local event of enormous
social psychological significance); (4) your group did very well to generate
and analyse data from such a large, nationally representative sample; and
(5) the focus on both within and between group variations in historical
understandings of Relative Deprivation and on what might explain such
variations is relatively novel (though you have broached these issues in
some of your other work and I
initially had some concerns about overlap).

Despite these strengths, I have reservations about several aspects of your
paper, some of which overlap with the comments of Reviewer 1. I list these
in no particular order.

1. I found the title to be somewhat misleading. In this study, you don't
really study directly the historical narratives of group members per se.
The word 'narrative' is actually a bit of a misnomer here. What you

actually explore are black and white South Africans' quantitative ratings of ingroup satisfaction across four, fairly crudely defined, historical periods, with the reassurance that this periodization is consistent with that of other South African 'experts'. Actually, I found it to be pretty arbitrary and, having lived previously in South Africa for about 15 years myself, I can think of more fine-grained ways in which to differentiate the country's history, e.g. the entire era of colonialism and the segregationist period prior to apartheid are disregarded, as are subgroup specific historical moments (e.g. the great trek). I think that you need to concede the limits of this way of carving up history. To my mind, it is probably not that much of an advance on the 'then versus now' methodology that you criticize in your paper . . .

2. The theme of intergroup attitudes is poorly handled throughout the paper, a point picked up by Reviewer 1. The theoretical rationale for studying such attitudes is under-developed in the introduction. Your explanation for your emerging pattern of findings in the discussion section is likewise brief and rather speculative (as I understand it, higher identification predicts a generalised tendency to make intergroup comparisons and thus a generalised negativity towards a rather wide range of outgroups, several of which do not feature much in the everyday thinking of ordinary South Africans, in my experience). Compounding this problem, your measures of intergroup attitudes sit awkwardly with the rest of your survey measures in a number of ways. First, these measures are numerous relative to the one or two item measures used to operationalize your more central constructs and this discrepancy is not explained. Also, you ultimately collapse attitudes towards a whole series of very different 'outgroups' into cumulative scores in a way that I don't find psychologically plausible (even if you can find statistical justification). Finally, although I concede that xenophobic attitudes are a social problem in South Africa, it strikes me that you should have focused instead on interracial attitudes between whites and blacks in the country. Research on these attitudes would, to my mind, have fitted far more directly with your overall argument and would have been more directly related (in a conceptual sense) to your various core constructs (e.g. identity, Relative Deprivation etc.). In its current form, then, I just don't think the bits of your paper on intergroup attitudes work very well. My own suggestion would be to remove them entirely - painful, I accept, but it will also help you to address my point 5 below concerning the paper's length. You may, of course, wish to try to convince me otherwise . . .

3. In general, I share Reviewer 1's concerns about the validity of your survey measures. They mostly comprise single item scales. Some researchers have defended this approach, but it still makes me (and Reviewer 1) a bit

uneasy. I also had some problems with how you labelled these measures. Notably, your measure of group self-esteem is categorised as a measure of 'well-being' - however, in many studies, ingroup pride is used as a straight proxy of group identity (including many studies in the SIT tradition). Certainly, collapsing pride in one's group with measures of personal 'well-being' such as 'hope for the future' under the general label 'well-being' seemed like a bit of a stretch (to me). Do they all tap this overarching construct? Also, I did not quite understand why the measure of group self-esteem focused on national level categorization rather than the sub-group (racial identification as 'white' or 'black'). Finally, I was not convinced that a single item measure of socioeconomic 'satisfaction' was able to capture fully, the is/ought discrepancy at the heart of the concept of Relative Deprivation.

In short, I think that you need to concede that some of your measures may be limited -- quite possibly because they were included as one small part of a much wider survey? -- and also say a little more about the concepts of group well-being and Relative Deprivation (that addresses my concerns above).

On the plus side, I would accept also that the potential problems with the measurement validity of your study are off-set by the virtues of acquiring a large, representative sample and data that seem to have been very rigorously gathered. I kept that in mind when evaluating your project as a whole, also recognising that getting items placed on large surveys of this kind often comes at the cost of having fewer items than may be ideal.

4. The study used the organising categories 'white' and 'black'. I accept that this kind of simplification is necessary in presenting the complex results of this kind of analysis in a single paper. Even so, I think that you should acknowledge more fully that it is a simplification. For example, I strongly suspect that Afrikaans and English-Speaking whites in South Africa may have a quite different take on historical relative deprivation etc. Africans living in rural and urban areas likewise will have a different take.

5. The paper is too long. In my view, the same message could be presented in a version that is at least 1500 words shorter without much loss of core content. Removing the bits on intergroup attitudes may help this problem. However, I also think that the introduction could be trimmed. I also wish that we could shorten the results section. However, I appreciate that your (interesting) method of analysis will be unfamiliar to many social psychologists. As such, a somewhat longer explanation of its basic principles is probably warranted in this case, even if it is a bit tedious

to wade through at times.

6. Reviewer 1 points out some problems about your assumptions regarding the direction of causality in the interrelations between your key variables. You will not be able to resolve this issue based on your current research design; however, you should consider his or her (equally plausible?) account of such relations in your discussion.

7. A few minor issues:
(a) There is an interesting local tradition of work that has examined young South Africans' 'narratives of the future' - using an essay writing methodology -- that might be worth considering as a foil/complement to your results. I don't have the references to hand, but Gillian Finchilescu (WITS) and Andy Dawes (UCT) have both done work in this tradition and, interestingly, there is a historical dimension to this tradition of work. The same method (involving the production of essays about the future) has been conducted at several junctures in South African history (see, for example, the early work of Peter du Preez, which will be cited in the more recent papers). More generally, I would invite you to make greater reference to the work of South African social psychologists (e.g. on Relative Deprivation etc.). They will certainly be very interested in this paper and will want to read and cite it: you should take the opportunity to increase its appeal to this audience and thus

increase the likelihood that your paper will set the agenda for future research.

(b) The paper is generally well-crafted and free of the usual errors. Thank you. However, there are a few typos/issues of phrasing, e.g. p.23, para 2, line 'the optimal model for Africans should included?', and p.29, last line, 'effect sizes were quite smaller'. A further proof-reading may be in order?

(c) The translation of questionnaire items across South African languages is notoriously tricky. Can the authors provide any further evidence that their translations were valid (e.g. was 'back-translation used etc.)?

Overall, lest this editorial turn into a long list of problems and complaints, I want to reiterate that there were lots of things that both the reviewers and I liked about your paper. The general framework is original, of social psychological importance, and, in my view, fits with what used to be called the 'European tradition' of social psychology (and I mean that as a compliment). I do hope that you are able to undertake a revision, though I accept the recommendations above are fairly onerous. If you are not and

prefer to resubmit elsewhere, please let me wish you the best of luck in developing a rich and interesting line of research. I enjoyed reading your work. On a final note: I am sending this letter from a train and without the luxury of a hard copy to proof read, so please forgive any typos or spelling errors.

Regards

John Dixon
Editor

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Reviewer #1: This article identifies the trajectories of perceptions of the group economic status of African and white South Africans; and investigates the correlates of these trajectories. There certainly is much merit to studying the social psychological features of dramatic social change, and the methods and analysis are mostly rigorous. However, I have a number of concerns about the article.

The article theorizes that ingroup identification increases sensitivity to unfavourable comparisons, increasing threat, and thus relative deprivation. 1) This does not explain why ingroup identification should increase sensitivity to favourable comparisons (i.e., among the African sample). (2) To test this theory, threat would need to be measured and used as a mediator. (3) The opposite causal direction is equally plausible: group members who make unfavourable comparisons disidentify with the group and those that make favourable comparisons identify more strongly.

A number of the measures are single item indicators, so we have no way of judging their reliability. I was especially concerned about the single item group identification measure, which we are told was not badly skewed (despite having a mean of 8.4 on a 10 point scale, see also the group esteem mean), whereas previous research in SA and elsewhere indicate that these measures are highly skewed.

The measurement of socioeconomic status is not described in the article, and this can be quite contentious in South Africa.

In contrast to many of the other measures, intergroup attitudes were measured with many items. However, I was surprised to see such rigour here when the concept had not been discussed in the theoretical introduction - and so it was not clear why it was included or what hypotheses were being

tested.

I have a number of concerns with the final section of the analysis. (1) The effect sizes of the MANOVAs are very small (although the large sample ensures significance). Socioeconomic status needs to be included as a covariate in these analyses. It is highly likely that socioeconomic status predicts membership in relative deprivation trajectory group and most certainly is associated with xenophobia. So the small effect may be due to socioeconomic status (and possibly also ingroup identification)! The same argument applies to the analyses of wellbeing. More generally, (and related to my point about theory above) I did not see why intergroup attitudes were considered in the analysis with well being and not with the ingroup identification analysis. Ingroup identification and prejudice are strongly related constructs in social identity theory (which the author(s) use).

Finally, and most fundamentally, the article did not convince me that the empirical work actually studied a context of dramatic change. The title says 'social' change - and perhaps dramatic social and (certainly political) changes have happened in SA. But the empirical materials focus on economic change. One of the major concerns about transformation in South Africa is in fact the lack of change. The black majority remain miserably poor and whites continue to dominate the economic sphere (whites occupy 74% of management positions in the private sector). So I'm not sure why the author(s) argue that change in RD is expected. In my view, the more stable trajectories are much more expected. In fact this is what the results for the majority of both groups show.

The study has many strengths but the report needs revision before it would be acceptable for publication.

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Reviewer #2: Although clearly important, little systematic research attention is devoted to the role of narratives in divided societies undergoing social and political change. Thus - the important of this manuscript that presents a convincing, methodologically sound and well written research on this topic. The conceptualizations as well as the analyses are interesting and add importantly to the understanding of the role of narratives in divided societies going through dramatic change. My only comment is that the authors should situate their study more clearly in the context of existing studies on narratives and conflict. The studies of Yechiel Klar on narratives in the Israeli-Palestinian conflict are very relevant as well as the measures developed in these studies. Also the

studies of Dan Bar-On on narratives in the Holocaust and in the Israeli-Palestinian conflict should be referred to.

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