March 24th, 2012

Dr. John Dixon
Department of Psychology
Lancaster University, Fylde College
Lancaster LA1 4YF
United Kingdom

Subject: Resubmission of a manuscript for publication

Dear Dr. Dixon,

We were very pleased with the response to our manuscript "*Group narratives in the Context of Dramatic Social Change in South Africa*" (BJSP1610). We are very grateful for your comments and those of the reviewers. Please note that, following your suggestion, we have modified our title that now reads as: "*Social change in South Africa: An historical approach to relative deprivation*".

To address all the comments, we have reproduced them in bold in the section named "Authors responses". A detailed description of the revisions made follows each comment. We hope that these revisions will meet your demands. We appreciate the comments and the reviews and believe our revisions greatly improve the overall quality of our paper.

Please contact us if you have any questions or comments.

Sincerely,

Roxane de la Sablonnière, Ph.D.
Associate Professor
Département de psychologie
Université de Montréal
C.P. 6128, succursale Centre-Ville
Montréal, Québec
Canada, H3C 3J7
http://mapageweb.umontreal.ca/delasabr/

AUTHORS RESPONSES: BJSP1610
Editor's comments

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 . . .

First, it is true that our title may have led to confusion, as "narratives" are often associated with personal life or group stories (e.g., McAdams). In order to address your comment, we did the following three changes in our manuscript. First, we have changed our title that now reads as: "Social change in South Africa: An historical approach to relative deprivation".

Second, in the methodology section, we have added some more specification in terms of how and why the specific periods were selected. Specifically, we now write: "In the context of the present study, temporal relative deprivation was assessed at four main historical periods: 1) the time of apartheid (1948-1991), 2) the immediate post-apartheid period (1991-2005), 3) the present time (2006), and 4) 5-years in the future. South African scientists and experts in research on South Africa, both Africans and Whites, identified these as the four most critical historical periods. These selected periods are also supported by research conducted in South Africa by Finchilescu and colleagues (Finchilescu & Dawes, 1998, 1999; see also Finchilescu & Tredoux, 2010 for a detailed description of changing South Africa)." (See pages 14-15).

Finally, in the discussion, we added a "Future directions" sub-section to address your comment about the different historical periods that we have selected for the present study (see page 27). In that section, please note that we also took the opportunity to add on the work you and the reviewers have suggested (i.e., Finchilescu & Dawes, 1999; du Preez, Bhana, Broekman, Louw, & Nel, 1981; Adwan & Bar-On, 2004; Bar-On & Kassem, 2004; we also judge important to add the work of Danziger, 1963). Specifically, we now discuss about the "essay methodology" (Danziger, 1963; Finchilescu & Dawes, 1999; Preez, Bhana, Broekman, Louw, & Nel, 1981) as well as about concrete means to resolve conflicts between group that have different narratives (Adwan & Bar-On, 2004; Bar-On & Kassem, 2004). We thank you and the evaluators for these suggestions and think that including the work of these colleagues increase the value of the present 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 . . .

As the editor underlines, we believe that xenophobic attitudes is one of the most important social problem in South Africa. In recent years, there have been many xenophobic attacks against immigrants from neighboring countries, which partly explains our interest in attitude towards African immigrants. However, because our main focus is on black and white South Africans' historical perspective, we agree with the editor that we should focus on interracial attitudes between whites and blacks instead. Given that interracial attitudes remain the important intergroup issue in South Africa, we believe that this issue needs to be covered in our manuscript. We hope that our changes will greatly improve the interracial attitudes theme in our manuscript. Specifically, we made two important changes:

Firstly, we conducted new analyses with a measure of interracial attitudes, as you suggested. Results revealed that, as expected, *Africans* who perceived the "decreasing group-based deprivation" trajectory had more negative interracial attitudes than those who followed the "stable" group trajectory or the "high-stable" group trajectory. For *Whites*, results follow the same pattern: *White South Africans* who perceived the "increasing group-based deprivation" trajectory had more negative interracial attitudes than those who follow the "stable" group trajectory. These results replicated what we previously found with our extensive measure of attitudes towards African immigrants. However, in order to focus on the Black and White context, we included only the measure of interracial attitudes in our manuscript on pages 16-17. Because we believe

that including interracial attitudes is an important component of our paper, we also added a note on page 30 that we obtain the same results with a more comprehensive measure of intergroup attitudes (i.e., the one we originally had in the paper).

Secondly, in the introduction, we have completely changed the section associated with the third goal---the one about the outcomes. Specifically, we clarified that our objectives with that third goal are twofold. First, we aim at replicating in another social context the impact of the trajectories of relative deprivation on both collective and personal outcomes. Second, that we aim at extending to other consequences of relative deprivation. Specifically, relative deprivation has a tradition of being associated with intergroup relations. However, never has that association been tested using an historical perspective. Moreover, at a theoretical level testing interracial attitudes is important because of the tradition in relative deprivation research of associating high levels of relative deprivation to higher negative intergroup attitudes. What we argue here is that this traditional approach might not hold when an historical approach is considered. Instead, we hypothesized that it is change in the trajectories, i.e., assumed (versus stability) that will predict negative interracial attitudes. Please see our arguments on page 11-12.

With these changes, we hope that we have convinced you of the importance of keeping intergroup attitudes in our manuscript. If we have not, please let us know and we will remove it from the manuscript.

3a. 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.

This issue was addressed in the discussion in the "Limitations" sub-section. As you suggested, we have underscored the fact that our survey was representative, but also at the same time said the price to pay, which is to have less items. However, as we have elaborated in the discussion on page 26-27, we have added references in support of single item measures. Indeed, some researchers argue that single items are in some contexts more valid as compared with multiple items. For instance, because multiple items are adding synonyms questions, they often decrease the content validity of the measure (e.g., Rossiter, 2002).

3b. 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?

In this comment, the editor questions the relevance of "collapsing pride in one's group with measures of personal 'well-being' such as 'hope for the future' under the general label

'well-being'". To address this limitation we decided to conduct a separate ANOVA for interracial attitudes and group self-esteem. Because we had two measures of personal well-being, a MANOVA was then conducted with our measures of personal well-being. This way, the distinction between the personal and the collective are made clearer. Moreover, throughout the manuscript we have changed the label "collective well-being" for "group self-esteem". (See pages 22-23).

3c. 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').

First, on page 16, we have specified that our measure of group self-esteem was focused on the national level because our measure of relative deprivation also focus on the national level.

Second, in our survey, we also had a measure of group self-esteem that focused on the racial group (i.e., collective esteem) as you are describing. We conducted the analysis with that measure and we found the exact same results as our original measure of group self-esteem. Because another manuscript based on the same data set made use of that measure we thought it inappropriate to repeat it here. Although the goals and scale items are totally different from the present study, we chose here to keep the group self-esteem measure to ensure no overlap of items. However, because the results are the same using both measures, we feel confident about our results. We have added a note on page 30 that specifies this.

3d. 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.

We agree that it is presumptuous to use one item to capture a construct as complex as SES satisfaction. As we noted in our answer to 3a, there is some precedent for doing so and in the present study we were greatly constrained by time and literacy. Also, and fortunately, Smith et al. (in press) have conducted a meta-analysis on relative deprivation which included several studies that used the single items used in the present study.

3e. 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.

Here, you noted the limited nature of our measures. Indeed, because our research was part of a larger survey on xenophobia in South Africa (by SAMP), we had to limit our measures. That also explains, why we had, for example, more items on intergroup attitudes as compared with the well-being measures. In order to address this issue, we added a paragraph in the new sub section "Limitations" on the cost of having a representative sample and thank you for your understanding concerning that limitation, which is of course impossible to fix. (See pages 26-27)

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.

In order to address this comment, we ran a new series of trajectory analyses. We conducted the analyses separately for Afrikaans (N=261) and the English-Speaking whites (N=180). Results are similar to the one reported. For the Afrikaans, two trajectories emerged: the "increasing group-based deprivation trajectory" and the "stable" trajectory. For the English-Speaking whites, the same two trajectories also emerged. We thus decided to report the analysis combining both Afrikaans and English-Speaking whites. On page 19 of our manuscript we have added this specification.

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.

In order to address this comment, we significantly shortened the introduction (more than 930 words), the methodology section (about 1,100 words) as well as the results section (about 370 words). You will see that our new introduction is less repetitive and more concise. This exercise was helpful and necessary, especially that we chose to keep intergroup attitudes. We also try to clarify the method of analysis, but kept the necessary details to understand the trajectory analysis.

As requested by the editor, and in accordance with what Ms. Emma Stephens asked in her email of March 16, 2012, our manuscript is now 1500 words shorter than the former version (word count: 7814).

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.

We now discuss the alternative explanation on page 26-27 in the sub-section "Limitations". Here is what we wrote: "The present study suffers from two limitations that should be taken into consideration in future studies. First, our research indicates that social change affects the way group members interpret their collective history through relative deprivation. That is, the extent to which people perceive their group as an important part of their identity influences how they perceive social change; by extension, people interpret through this lens the history that defines their group's identity. Nevertheless, since our data are correlational, we are careful to avoid implying causation between these variables. For example, the possibility that it is the perception of past events that influences people's current level of social identification cannot be ruled out (see Liu & Hilton, 2005). In order to fully test the causal link between social identity and group trajectory in the context of social change, future studies might include a distinct manipulation or a prime."

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.

Please see our response to comment 1 of the Editor. In addition to the authors you suggested, we have added the work of other South African psychologists on relative deprivation (e.g., Appelgryn & Bornman, 1999; Duckitt & Mphuthing, 1998, 2002). We also integrated recent published work by Cakal et al. (2011) on the association between identification and relative deprivation, on pages 8-9.

(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?

We did an additional proof-reading of the manuscript to remove typos.

(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.)?

Items were back translated. This information was added on page 14 of the manuscript.

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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.
1. 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.

This insightful comment was also raised by the editor (comment 6). We have reproduced our response here to avoid having to search for our discussion of this important issue.
We now discuss the alternative explanation on page 26-27 in the sub-section "Limitations". Here is what we wrote: "The present study suffers from two limitations that should be taken into consideration in future studies. First, our research indicates that social change affects the way group members interpret their collective history through relative deprivation. That is, the extent to which people perceive their group as an important part of their identity influences how they perceive social change; by extension, people interpret through this lens the history that defines their group's identity. Nevertheless, since our data are correlational, we are careful to avoid implying causation between these variables. For example, the possibility that it is the perception of past events that influences people's current level of social identification cannot be ruled out (see Liu & Hilton, 2005). In order to fully test the causal link between social identity and group trajectory in the context of social change, future studies might include a distinct manipulation or a prime."

2. 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.

See our response to the editor, comment 3, concerning the limits of our measures. In terms of the identification measure specifically, we acknowledge that this item is skewed,

although fortunately it does not compromise our statistical analysis. See on page 17.

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

A detailed description of our measure of socioeconomic status can now be found on page

4. 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.

See our response to the editor, comments 2 and 3e.

5. 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).

The reviewer underlines two important points in this comment. Firstly, it is suggested that our effect may be due to participants' socioeconomic status. We included both the socioeconomic status and the ingroup identification as a covariate in ANCOVAs. Although socio-economic status and ingroup identification predict some of our outcomes, group-based trajectory of relative deprivation remains a significant predictor of interracial attitudes, group-based esteem and personal well-being. On page 23, we present these results: "Additional analyses were conducted in order to control for the potential effect of socioeconomic status and ethnic group identification. Results remained unchanged, although some effect sizes in our study are small." Here again, for the sake of parsimony, we decided to report the ANOVA and not the ANCOVA with the socioeconomic status and ingroup identification as covariates.

In this comment, the reviewer also questions why intergroup attitudes were considered in the analysis with well-being. We decided to conduct separates ANOVAs for interracial attitudes, group self-esteem and personal well-being (MANOVA). For more details, see our response to the editor, comment 3b.

6. 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.

Thank you for this interesting comment. To address it, we made two important changes in our manuscript. First, we have changed the labels of our trajectory from "expected" to "assumed" and make it clear throughout the manuscript (see especially in the introduction on page 5) what we mean by that term. Second, we have not used the word "surprisingly" when we describe the "stable" trajectories. Indeed, as the reviewer noted, it is possible that even in the context of dramatic "assumed" change, for some people their realities in terms of their every day lives has not changed.

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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.
1. 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.

Thank you for these suggestions, which we believe increase the value of our paper. The studies of Yechiel Klar were included on page 4 and 15.

Also, please see on pages 27-29 in the new sub-section "Future studies". Specifically, we wrote: "Another research tool that could prove useful is essay writing, which was used extensively in South Africa as a means of finding different "future orientations" for South Africans of various ethnic origins (Danziger, 1963; du Preez, Bhana, Broekman, Louw, & Nel, 1981; Finchilescu & Dawes, 1999).

The present study can also form a basis for future research oriented towards decreasing interracial tensions between ethnic groups. Bar-On and colleagues (Adwan & Bar-On, 2004; Bar-On & Kassem, 2004) used an innovative historical approach with the aim of resolving intergroup conflict and engaging group trauma. For example, in one of their studies, they worked closely with Jewish and Palestinian students who were asked to listen to each other's narratives as a means of "work[ing] through intractable conflicts" (Bar-On & Kassem, 2004, p. 289). Because they were building on finding similarities

between the groups, their method had a positive impact by way of increasing empathy and openness among the participants. Having established that the way people perceive their group history has important consequences for intergroup relations, the technique used by Bar-On and colleagues could be helpful in diverse cultural contexts."