Decision on Manuscript ID CJB-20-0399 - Criminal Justice and Behavior

As you will see below, Reviewer 1 was quite enthusiastic toward your paper and provided some technical recommendations for improving your manuscript. Reviewer's 2 and 3 were much more critical noting the need for a general reframing of the paper (Reviewer 2), absence of important methodological information (both reviewers), concerns with your sample size (Reviewer 3), and concerns and recommendations regarding your data analytic plan (both reviewers). Unfortunately, after reading the manuscript and all three reviews, I agree with many of the concerns raised by the reviewers plus I noted a few of my own.

First, your review of the literature is incomplete. For example, you wrote on p. 3 that group therapy has proven beneficial citing one study when a more substantive citation would be a meta-analysis conducted by colleagues and myself. Additionally, in this sentence you indicated that discourse on group process with criminal justice populations is non-existent when again, colleagues and myself have published on this specific issue. The aforementioned group therapy meta-analysis conducted by my colleagues and me also provided support for open ended groups over closed groups. I do not raise this criticism due to my work not being cited; rather, as a note that your literature review is not complete.

Additional concerns include that decision to use therapists to collect evaluation (assessment) data with no method of accounting for response style (e.g., socially desirable responding), and the absence of the study acceptance rate (it is not clear who conducted the consent process and if it were correctional staff it could be participants believed they could not decline participation and thus responded throughout treatment and the assessment process accordingly). I also note that the recidivism time period is exceedingly short by correctional standards limiting the utility of this data. I recognize this latter point is largely out of your control as recidivism follow-up data is unwieldly and nearly impossible to gather appropriately without substantive grant funding (or other financial support), but as provided, the recidivism data could easily be over-interpreted by naïve audiences when in fact there simply was not enough time (e.g., 6 months) to obtain true recidivism rates. Other concerns are noted in the reviews.  Given the number of manuscripts that we receive and our very high publication standards, I regret that I am unable to accept your manuscript for publication.

Reviewer: 1

Well done to the authors of this article – you have clearly worked very hard to undertake this very interesting and much needed piece of research. It is evident that you have worked methodically for a long time to collect the data. The methods have also been well-considered. The article is very nicely written and have some comments and suggestions below, but these are minor in nature.
Abstract
I think ‘custodial sentence’ sounds better than ‘jail sentence’.
The first half of the Abstract needs to be clearer. When you say you examined the impact of group therapy what specifically do you mean? The impact in terms of desistance? Improvement on psychometric scores?
You say ‘A qualitative study’ – be clear that you undertook this as part of the study and that you are referring to your own research.
What are ‘social contacts’? Be specific.
Although you say that 90% of participants had not returned to crime, you need to acknowledge that this refers to they hadn’t been arrested, charged or convicted. We don’t know for certain i.e. it could be that they had re-offended but haven’t been caught.
Given the title of the paper, would it not be better to begin the Abstract with a sentence about open group cohesiveness?
Background
Page 2. You say that group therapy is a proven intervention – would it be more appropriate to suggest proven for some adult offenders? Or even better, give a more specific statistic?
Page 3. What is meant by ‘the essentials of group therapy’? I think you need to make it abundantly clear that other studies have focused on the nature of the group e.g. dynamics among group members/cohesiveness etc.
You’ve defined and explained group cohesion really well.
Page 4. Rather than ‘reducing the chances of recidivism’, how about ‘reduction in recidivism’?
Page 5. What is meant by ‘diverse content domains’? Can you say a little more about why a new group member represents both a regressive and progressive step?
Page 5. Achieving rather than achievement.
Page 6. Can you define ‘narcissistic deprivation’?
Methods
Page 7 – define what ‘and more’ actually is.
Page 10 – what is meant by ‘date of registry examination’?
Page 13 – you say that 90 per cent hadn’t returned to crime but we don’t know this for sure? Sure, it’s a good indicator but it needs to be acknowledged that 90 per cent had not been arrested, charged or reconvicted of a crime providing a good indicator of desistance.
Discussion
Page 14 – what are fraudulent sex offenders?
Page 16, second line ‘family and family relationships’.
The article is very nicely concluded with some interesting pointers for future research and policy implications.

Reviewer 2:

I approached reviewing this manuscript with a great deal of positivity given that the title suggested a valuable description of a complex therapeutic process with an outcome that is of enormous interest. Unfortunately, I didn't feel that it lived up to the title and below I will try to outline why that was the case. The paper is problematic in terms of readability, lack of clarity on research question and hypothesis, and methodology, especially the statistical plan. Writing style/Structure

 1. On a conceptual level, the writing is disjointed and hard to follow. I will provide an example. Page 8, the section on facilitators, “…study was conducted with 70 probation service facilitators (probation officers), counselors, and management to identify their expected results from open group therapy for adult offenders.” I had to pause here and think about the breakdown of these disciplines within each group. The subsequent sentence, provides the breakdown, however, it starts with unnecessary and confusing wording, “The researchers organized and conducted nine focus groups of probation officers from different regions across the country…” Authors could have simply combined these two sentences into one without all the distractions. After going through the trouble of making sense of the numbers, I had to ponder again how facilitators’ background could have affected outcome. This was my experience throughout reading of the manuscript.

2. The semantics of the paper was equally distractive. Frankly, I found phrases like “our country” strange. Was this meant to maintain confidentiality during the review process? If no, I would welcome knowing about the country and cross-cultural implications, if relevant. Static and dynamic risk factors. In the literature, static factors are referred to as historical, whereas dynamic factors are referred to as changeable. There is no need to use a different terminology like non-modifiable and modifiable. I doubt Gendreau et al. (1996) ever used “addiction to dangerous substances” as a dynamic factor, most likely it was defined as alcohol/drug problem. In other words, stick to the common language. Methodological Issues

 3. Sample size (Page 8). I reread this section multiple times and I still could not understand how the sample size was determined. I will walk you through the process from a reviewer’s perspective: “The adult offenders were divided into 94 therapy groups, with each group comprised of participants from one of four categories of criminal offense: violence in the family (N = 57), general assault (N = 49), female offenders (N = 21), and other crimes (N = 59).” I add these numbers and I come up with 186. I then glance over the sample size which is defined few sentences earlier that states, “The research population in this stage included all adult offenders (N = 419) served by our country's…”

4. Study follow-up. The authors state that the research was carried out “through a three year period.” Group participation occurred “during the period from December 2013 to December 2014...” In the abstract, the study follow-up period is defined as “Importantly, six months to one year after conclusion of therapy…” Where is the third year period?

5. Group Cohesiveness Scale (GCS). Given that this is the authors’ main proxy for group cohesiveness, not to mention one would think a predictor (at least according to the title), one would expect seeing the before and after values in a table, especially with all the variations in facilitators’ background and composition of group members.

 6. Statistical Plan. There are well known statistical tests for examining variables that affect recidivism rates. At the very least, if the above issues were addressed, a simple binary regression with GCS as predictor and recidivism as outcome would have set the precedence for the quantitative component of the paper.

Reviewer: 3

Thank you for the opportunity to review your manuscript on group cohesiveness of court ordered group therapy sessions. This is an interesting treatment model that, though it has flaws, seems to provide an avenue for more rehabilitative-focused criminal legal systems. Overall, this manuscript is well-written. I have outlined some questions/concerns that would strengthen the manuscript though.

• In the background section, Andrews & Bonta’s work on criminogenic risk factors is omitted. They have written extensively on this topic and their work should be included in this discussion.

• In the Methods section, I’d like to see more of a connection between how the qualitative focus groups led to the use of the GCS as the way to measure cohesion. How did you conclude to use this instrument, and was that decision tied to what you found from the focus groups? More information is needed on this tool.

• Regarding data collection, it’d be helpful to have some additional information about how/when data was collected. “Before joining the group” is vague and since “facilitators distributed the questionnaires” and “participants were new or had joined therapy group less than one month prior” it is unclear at what point, and how, participants completed the data collection. Did they complete the questionnaire in their first session? Or did they somehow complete it prior to coming to a session? More details in this area would be helpful.

• Along those lines as well, since group compositions changed, did you track this information? Also, did you only collect data from those who were initially part of the group from the beginning of the study? If not, how/when did you consent new members? And, were there any participants who did not complete the 2nd data collection point due to the study ending prior to them finishing the group session?

• On page 13, about lines 14-21 you discuss findings about getting along with others. This section is unclear as there is no mention of this scale/item and what it is measuring or how. Is there an anchor point for the response to this item? For example, does the item say “In the last month, please identify the extent to which you have gotten along with others.” Or was this only at follow-up and since they had joined the group? More information is needed on this item.

• I encourage you to consider renaming your “categories of criminal offenses” since one of these categories is women. It makes it seem as though being a woman is a type of criminal offense rather than the type of group.

 • I’m curious as to why group cohesion wasn’t compared for any other variables? Many other variables are likely associated with the sense of cohesion and analyzing other relationships would add to the literature on this topic. As is, all that can be observed from this study is that cohesion was relatively high at the end of the group. There is no comparison or understanding of this cross-sectional information. I would hesitate to draw the conclusion that the group facilitators or any other variable had anything to do with group cohesion since that information is not presented.

 • The conversation around recidivism is an interesting one. First, the manuscript doesn’t make it clear why group cohesion would be related to recidivism. The background section needs to include some rationale for why recidivism is being studied along with group cohesion. Next, I think that without any information as to the offence past of the participants in the group it’s a little hard to contextualize recidivism at such a low rate. Were the participants in the group first- or second-time offenders? There’s also no analysis presented on how rates differed by type of group, which may have important implications. I believe the study as presented doesn’t make the case to support the conclusion that this type of group is responsible for low recidivism rates.

• Did you consider having a focus group with individuals on probation to identify themes they thought were important to group cohesion? That’s an important distinction and voice to hear when making determinations about how cohesive a group was. Having their insight would help to identify those factors that they deem important which may or may not be what the facilitators thought.

• I believe a note should be added in the limitations that probation officers were the facilitators and the data collectors. First, the therapeutic atmosphere is different with a PPO than with a trained/licensed therapist. Second, the officer doing the data collection may unduly pressure the participants. It would be helpful to have information in the sample or group description about how many people participated in the groups compared to how many participated in the study.

• In the discussion section, the authors draw a connection between the changes in social connections and the perception/realization that these were not positive connections. Did you collect any data on this to verify this assertion?