Author's response to reviews

Title: Organizational interventions employing principles of complexity science have improved outcomes for patients with Type II diabetes

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Author's response to reviews:

Dear Dr. Mittman,

Thank you for your thoughtful comments regarding our manuscript, "Organizational interventions employing principles of complexity science have improved outcomes for patients with Type II diabetes." We have incorporated these comments into the manuscript in a way that we hope adequately addresses the issues you raise, and would like to explain how we have done so. Areas of the manuscript that have been edited in response to reviewer comments are highlighted in yellow throughout the text.

The first major point raised by you and the reviewers relates to the operationalization of CAS characteristics. There are several aspects to this, the largest of which seems to revolve around the idea of "sensitivity to initial conditions." We believe that sensitivity to each microsystem's unique set of local conditions is an important implication of two key CAS characteristics - self-organization and co-evolution. We believe that our focus on these characteristics incorporates this idea, and have explained this in greater detail in the text in the background, methods, and discussion sections, as well as in our responses to Dr. Aron's comments. We have addressed other specific reviewer concerns related to operationalization in our point by point responses below.

The second major point you raise is that of using the CAS lens for "planning and conducting" interventions, rather than for "simply observing and understanding." We agree that this is a critical distinction, one that emphasizes how our manuscript adds to the literature related to CAS by focusing on the former. We have made this distinction in our manuscript, and expanded both our background (p.6-7), discussion (p.12-13), and conclusion (p.16) sections to reflect this important extension of the use of CAS in clinical settings. We have referenced the article by Litaker et al. as part of this discussion (ref #20).

Specific comments from David Aron

1. Question for reviewer - "Did the authors look for the appropriate sort of papers?"
Reviewer comment: "The papers are limited to those in the English literature identified by standard bibliographic methods. It is not clear that all management literature would be identified by their approach."

We purposefully limited our search to the peer-reviewed medical literature, as we believed that this would provide the most simple and universally acceptable approach to identifying studies. We have added a sentence to our methods section to clarify that we did not seek additional sources, and expanded our discussion to comment specifically on this approach (bottom p.14-top p.15).

2. Question for reviewer - "Was methodological quality assessed and the trials weighted accordingly?"
Reviewer comment: "There is no weighting by number of participants or magnitude of effect."

We weighted studies both by number of participants and by duration of intervention. While we commented on this in the results section, we had neglected to state this specifically in the methods section, and have added this information (methods p.10, results paragraph 2, p.11).
3. Question for reviewer - "How sensitive are the results to the way the review has been done?"

Reviewer comment #1: "The authors chose only some of the characteristics of complex adaptive systems. Among those omitted was 'sensitivity to initial conditions.' While there may be some debate about the exact number of characteristics of complex adaptive systems, we believe that we captured the key concepts by looking for learning, interconnections between individuals, self-organization, and co-evolution. We have added additional references on page 6 (#18-19) to reflect this and added a point to our discussion (middle p.15). We would argue that sensitivity to local conditions is an implication of the characteristics of self-organization and co-evolution, and that our inclusion of these two characteristics incorporates the idea of the importance of this implication. However, we have commented on this in detail in the background (p.6), methods (p.9), and discussion(p.13) sections to clarify our appreciation of this point.

An additional aspect of this issue is that to our understanding, the concept of "sensitivity to initial conditions" is a construct with meaning specific to non-linear mechanics in chaos theory that does not necessarily translate directly into complexity science or complex adaptive systems. However, the more general notion of sensitivity to local conditions or current conditions is applicable to the study of complex adaptive systems as we have outlined above. For that reason, we avoid the exact terminology "sensitivity to initial conditions" in our manuscript.

Reviewer comment #2: "Second, the nature of the operational definitions and the typically low level of detail provided in the publication made it more likely that the presence of certain types of interventions would bias the results."

We believe that Dr. Aron is concerned that some types of interventions might be less likely to be described than others. This could potentially be true, but we believe that most authors would describe all interventions or components of interventions in at least some detail, and that no interventions would be completely omitted from the methods sections of the studies. Therefore, we would have at least some description of all aspects of an intervention from which to make an assessment.

Reviewer comment #3: "I am not sure that the inter-related reliability would be as high...if the literature were evaluated by someone from a different 'school' of complexity."

We have added two sentences to the discussion (end of first full paragraph on p.15) to reflect this potential limitation. Because we chose what we felt were the most "key" CAS characteristics whose definitions are relatively well-established, we think that the chance of this is likely to be small.

4. Question for reviewer: "Have the numerical results been interpreted with common sense..."

Reviewer comment: "I am not sure that I could reliably distinguish mixed from positive."

In table 3, we make these distinctions, and our explanation on p.10 provides additional detail.

5. Question for reviewer: "Are the discussion and conclusions well balanced...?"

Reviewer comment: "The limitations of the analysis are not sufficiently outlined."

We have expanded our discussion to include a more comprehensive assessment of the limitations of this study, particularly those related to the methodology used. This expansion can be found on page 15.

Specific comments from Dr. Rhydderch

We appreciate Dr. Rhydderch's suggestions related to potential references for the introduction. We did not include the Griffin reference, as it primarily focused on differences in care provided by primary care versus subspecialist physicians (though we agree there was an organizational component to some of their findings), but we agree regarding the relevance of the Weingarten reference and have added it to our introduction (#12).

We appreciate Dr. Rhydderch's thoughtful comments on the nature of CAS characteristics and our ability to see and categorize them in traditional research studies. We have incorporated her comments into our discussion, and our responses to her specific comments are as follows:

1. In her first comment, Dr. Rhydderch pointed out that it may be difficult to categorize an intervention as having CAS characteristics with accuracy after the event. We agree with the concern regarding this possibility. While we believe that the high level of inter-rater reliability helps to address this concern, we have added this to our discussion as a potential limitation on page 15.

Dr. Rhydderch also points out the overlap between CAS characteristics and other theories of organizational
change, and wonders if this could be considered grounds to change our scoring scheme. We agree that there are elements from these different theories in CAS theory, which might be part of why some of these approaches may be effective in producing organizational change. However, CAS theory provides a cohesive backdrop to unify these elements. We have added a sentence to address this in the discussion (p.13). Because we think that not only the presence of characteristics but the way in which they interrelate is important, we believe that giving each characteristic a separate rating that is used to create a total score provides a more rich assessment of the characteristics.

2. Dr. Rhydderch also comments that “it seems a bit confusing to look for evidence of complexity theory in studies where organizational interventions are introduced in a planned way.” We agree that there may be an inherent tension in looking for CAS characteristics in traditional research studies, and have added a comment related to this in our discussion (middle p.15). However, we would also comment that it is still possible to incorporate the constructs of self-organization and co-evolution into a planned study - for example, though having participants influence the study design as an explicit part of the intervention - and our ratings reflect this possibility. We hope that our edits and responses to review comments satisfactorily address reviewer concerns, and would be happy to expand in greater detail on any of these points.

Best regards,
Luci Leykum