**PEER REVIEW HISTORY**

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**ARTICLE DETAILS**

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| **TITLE (PROVISIONAL)** | Lessons learnt from the implementation of new models of care delivery through alliance governance in the Southern health region of New Zealand: A qualitative study |
| **AUTHORS** | Gurung, Gagan; Jaye, Chrystal; Gauld, Robin; Stokes, Tim |

**VERSION 1 – REVIEW**

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| **REVIEWER** | Tenbensel, Tim<br>University of Auckland, Health Systems |
| **REVIEW RETURNED** | 12-Jul-2022 |

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| **GENERAL COMMENTS** | Thanks for the opportunity to review this manuscript. I found it very easy to read and navigate my way around. The argument is very clear. This will be a valuable addition to the literature on health service collaboration and implementation of integrated care policies.<br><br>The article in my view only requires a few minor tweaks, which are mainly about clarifying some aspects of the analysis.<br><br>1) In the description of the eleven participants, it would be useful to know how many participants were from the DHB and how many from the PHO. It appears as if most of the implementers (the six participants who were not part of the ALT) were all Southern DHB employees (that is my inference which could be wrong). Were South Link PHO managers/clinicians involved in implementation and were they approached for interview? If not, this is a potential limitation, but also an important piece of data.<br>2) The authors have used CIFR as a framework for analysis and this is well-justified. I was a little confused in a couple of areas, mainly regarding the link between Box 4 and the text. I see that Box 4 contains all the CIFR constructs, and that the items that are bolded are those that were reflected in the data. The layout of this box could be clearer, with separations between the five key components of CIFR – it is quite hard to read.<br>3) It would be useful if the authors could clarify how they decided whether there was enough data for specific constructs within the CIFR. Were those that were not included not referred to at all? Perhaps some information in an appendix could clarify the process of analysis here.<br>4) What is the correct name of the first CIFR domain? In Box 4 it is identified as ‘intervention characteristics’ whereas in the text it appears to be referred to as ‘implementation characteristics’.<br>5) The definition of complexity on p11 (and in CIFR) is potentially confusing. This definition is quite different to other uses of the word ‘complexity’ in health services literature, particularly the large body of work that draws on complexity theory. Damschroder’s CIFR definition of complexity seems to more about difficulty of |

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| | implementation. Complexity could be one source of implementation difficulty but it seems rather strange to conflate the two. There are other sources of implementation difficulty that may have little to do with complexity as it is more commonly understood (eg power of key stakeholders). If the authors are not using a 'complexity theory' definition of complexity (in which complex is different to complicated) then it might be worth making this clear.<br>6) The structure of the PCCS seems to create a few interesting issues for applying CIFR, particularly about what is being implemented and what is the object of analysis. Clearly there were a number of individual sub-elements that were implemented to varying degrees. But when the authors are referring to implementation of PCCS, they are taking a more holistic view of the whole programme. Given that some components of PCCS were more highly specified than others, while others were more 'initiatives under construction' – using an implementation frame is potentially confusing – or at least there are some important nuances that have implications for the application of CIFR. Related to this point, the authors take a rather linear approach to the relationship between different elements of implementation at the top of p18 when these elements may not be easily separable in practice, or unfold in a sequential process. A linear approach might be more applicable to, say, the HCH element of PCCS, but less applicable to the whole of the PCCS.<br>7) The discussion brings in a comparison with existing literature. But the authors appear to stop short of answering a 'so what' question. Do we learn anything from this case that hasn't already been learnt before? What is it about this particular piece of research that is different, distinctive or noteworthy? Might some elements of CIFR be more important than others for this type of initiative?<br>8) On p21 under the section 'Reflecting and Evaluating' the authors report that participants wanted a more 'quality improvement approach', but in the final paragraph of this section, participants also wanted a 'performance measurement framework'. Are these findings consistent with each other? Perhaps there are important differences between what different participants wanted. Many who argue for quality improvement do so because they believe that performance management is inappropriate. This reflects a potential limitation of the way in which the data is analysed and conclusions are drawn because the authors appear to be reporting common themes – whereas it might be that different types of participants saw different (and perhaps opposite) enablers and barriers.<br>9) The authors make a point about the 'disconnect between research evidence and its implementation' on p24. I don't quite understand the point though. Are the authors saying that the Southern Alliance didn't do what Canterbury did? If so, what is it, specifically, that they didn't do that was important. I would caution against thinking of the Canterbury Clinical Network as an 'intervention' that any other health system can take off the shelf and 'implement' simply because researchers have said that Canterbury 'worked'. Isn't local context significant? I think this point could be reworked and made clearer.<br>10) Some minor points<br>P5) – the point about Alliancing being used in UK and Australia – is that in the health sector, or more generally?<br>P13) 'The silo mentality did not provide opportunities'. Would it be more elegant to say 'the silo mentality was not conducive to….'<br>P16) First paragraph quote- should 'poll' be 'pool'?<br>P18) Participants would have appreciated a detailed implementation plan. Whose job would/should it have been to provide this?<br>P19) community consultation (line 35) – who (which manifestations |

| | of community) were participants referring to as not having been consulted? |
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| **REVIEWER** | Eastwood, John<br>The University of Sydney |
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| **REVIEW RETURNED** | 07-Sep-2022 |

| **GENERAL COMMENTS** | The method section lacks information on methodology. The section should include information on the researcher's theoretical or paradigm position. For example, are the researchers approaching this study using logical empiricist, critical theory, interpretivist, pragmatic or critical realist ontology and epistemology? It is not sufficient to state that you undertook rapid thematic analysis using CFIR. That statement describes the method used but not the methodology. Having added a Methodology paragraph, the Method section should be expanded to include standard content in qualitative studies, including sampling method, reflexivity and evaluative rigour. The analysis section should include more information on the coding approach. Was coding open or closed? What mode of analytical reasoning was used? Was the reasoning only inductive or did to use reproductive, retrodictive, abductive and deductive modes of analysis? The mode of analysis will relate to the methodology and philosophical approach taken.<br><br>From the COREQ checklist, there does not appear to have been any checking back with the participants. This will impact the evaluative rigour. This should be commented on in the limitations section.<br><br>Results<br>It is observed that the Health Care Home initiative was seen as being successful. In the paper, it should be noted that this "new" initiative had new funding and did not require the "actors" to modify or change their resource allocation to achieve the implementation goals.<br><br>You note in the paper the shortcomings of the "rapid approach" and observe that a realist-informed approach might have enabled you to identify the underlying mechanisms that contributed to success or non-success in achieving the desired outcomes. There were at least four "mechanisms" identified in the analysis including: "trust", "willingness to share power", "communication" and "leadership". They may have been others. You have identified these without formally using a realist retroductive or abductive analysis. You might consider reflecting on these possible underlying mechanisms in your discussion. In the New Zealand context, you might reflect on the apparent sector emphasis on "management" and "managerialism" and the possible weakness, as evident from your results, of sector "leadership".<br><br>You might consider emphasizing the importance of leadership in the Discussion, Conclusion and Abstract.<br><br>I believe that there is a need in New Zealand during the current reform process to highlight the importance of leadership.<br><br>Minor<br>Page 17, line 10 should poll be "pool" |
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**VERSION 1 – AUTHOR RESPONSE**

**No.**
**Reviewer's comments**
**Authors' Response**

***Reviewer 1:***

Thanks for the opportunity to review this manuscript. I found it very easy to read and navigate my way around. The argument is very clear. This will be a valuable addition to the literature on health service collaboration and implementation of integrated care policies.

The article in my view only requires a few minor tweaks, which are mainly about clarifying some aspects of the analysis.

Thank you.
1.
In the description of the eleven participants, it would be useful to know how many participants were from the DHB and how many from the PHO. It appears as if most of the implementers (the six participants who were not part of the ALT) were all Southern DHB employees (that is my inference which could be wrong).

Were South Link PHO managers/clinicians involved in implementation and were they approached for interview? If not, this is a potential limitation, but also an important piece of data.

We have clarified this point in the text. 1 of the 6 implementers were based in the DHB.

There is no SouthLink PHO in existence – Southlink Health failed to be awarded PHO status in 2015 (https://www.stuff.co.nz/southland-times/news/71092184/southern-clinical-network-applies-for-primary-health-organisation-status ).

2.
The authors have used CIFR as a framework for analysis and this is well-justified. I was a little confused in a couple of areas, mainly regarding the link between Box 4 and the text. I see that Box 4 contains all the CIFR constructs, and that the items that are bolded are those that were reflected in the data. The layout of this box could be clearer, with separations between the five key components of CIFR – it is quite hard to read.

Thank you. We have re-edited Box 4 to improve clarity.
3.
It would be useful if the authors could clarify how they decided whether there was enough data for specific constructs within the CIFR. Were those that were not included not referred to at all? Perhaps some information in an appendix could clarify the process of analysis here.

We only found the interviews coded into three of the five CFIR domains. We have added an additional supplementary file (file 3) to this effect.

4.
What is the correct name of the first CIFR domain? In Box 4 it is identified as 'intervention characteristics' whereas in the text it appears to be referred to as 'implementation characteristics'.

Our apologies. The original wording is incorrect (P. 10, lines 1-12). It should read: intervention characteristics, inner setting and implementation process. We have re-worded.

5.
The definition of complexity on p11 (and in CIFR) is potentially confusing. This definition is quite different to other uses of the word 'complexity' in health services literature, particularly the large body of work that draws on complexity theory. Damschroder's CIFR definition of complexity seems to more about difficulty of implementation. Complexity could be one source of implementation difficulty but it seems rather strange to conflate the two. There are other sources of implementation difficulty that may have little to do with complexity as it is more commonly understood (eg power of key stakeholders). If the authors are not using a 'complexity theory' definition of complexity (in which complex is different to complicated) then it might be worth making this clear.

We agree. To summarise, complexity is used in CFIR in terms of an implementation science definition – the problems of implementing a complex intervention. This is not the same as health system complexity, which draws on complexity theory.

We have addressed this in the limitations section of the discussion regarding CFIR.

6.
The structure of the PCCS seems to create a few interesting issues for applying CIFR, particularly about what is being implemented and what is the object of analysis. Clearly there were a number of individual sub-elements that were implemented to varying degrees. But when the authors are referring to implementation of PCCS, they are taking a more holistic view of the whole programme. Given that some components of PCCS were more highly specified than others, while others were more 'initiatives under construction' – using an implementation frame is potentially confusing – or at least there are some important nuances that have implications for the application of CIFR. Related to this point, the authors take a rather linear approach to the relationship between different elements of implementation at the top of p18 when these elements may not be easily separable in practice, or unfold in a sequential process. A linear approach might be more applicable to, say, the HCH element of PCCS, but less applicable to the whole of the PCCS.

We agree. In particular, the implementation process domain constructs are presented linearly, although the CFIR developers do note that the process may be linear or nonlinear (see reference: 19, Damschroder et al, 2009).

We have addressed this in the limitations section of the discussion regarding CFIR.
7.
The discussion brings in a comparison with existing literature. But the authors appear to stop short of answering a 'so what' question. Do we learn anything from this case that hasn't already been learnt before? What is it about this particular piece of research that is different, distinctive or noteworthy? Might some elements of CIFR be more important than others for this type of initiative?

We have reworded both the discussion's strengths and limitations section (that refers to the methods used) and the comparison with existing literature section so that we do answer the "so what" question when comparing against the existing literature. In short, what is different is the use of an implementation science framework – the findings are in line with those reported in the existing literature.
8.
On p21 under the section 'Reflecting and Evaluating' the authors report that participants wanted a more 'quality improvement approach', but in the final paragraph of this section, participants also wanted a 'performance measurement framework'. Are these findings consistent with each other? Perhaps there are important differences between what different participants wanted. Many who argue for quality improvement do so because they believe that performance management is inappropriate. This reflects a potential limitation of the way in which the data is analysed and conclusions are drawn because the authors appear to be reporting common themes – whereas it might be that different types of participants saw different (and perhaps opposite) enablers and barriers

We assume the reviewer means "performance measurement" when they write "performance management."

With regard to “performance measures” we consider these findings are consistent. We have reviewed the relevant interviews and have identified that participants referred to the use of data in decision-making by setting KPIs and reporting progress against it, which they saw as an important step in the quality improvement process. This is well recognised in the QI literature – with the use of quality indicators or performance indicators/measures.

We have also re-edited the results section in the abstract to make this clear.

9.
The authors make a point about the ‘disconnect between research evidence and its implementation’ on p24. I don’t quite understand the point though. Are the authors saying that the Southern Alliance didn’t do what Canterbury did? If so, what is it, specifically, that they didn’t do that was important. I would caution against thinking of the Canterbury Clinical Network as an ‘intervention’ that any other health system can take off the shelf and ‘implement’ simply because researchers have said that Canterbury ‘worked’. Isn’t local context significant? I think this point could be reworked and made clearer.

We agree this is an important point. In fact in previous research by two members of this team we used the CFIR to unpack why simply taking HealthPathways from Canterbury into the Southern health region without contextualisation was a failure (see reference 37).

We have therefore re-worded this section to make this point clearer.
10.
p.5, the point about Alliancing being used in UK and Australia – is that in the health sector, or more generally?

We have clarified this to state “in the health sector”
11.
p.13, ‘The silo mentality did not provide opportunities’. Would it be more elegant to say ‘the silo mentality was not conducive to….’

We agree with the proposed re-wording and have actioned this.
12.
p.16, First paragraph quote- should ‘poll’ be ‘pool’?

Thank you. Yes – this has been changed to “pool”
13.
p. 18, Participants would have appreciated a detailed implementation plan. Whose job would/should it have been to provide this?

The participants’ responses were not consistent regarding this specific question, but all were clear a detailed implementation plan was missing/lacking. We have reworded this section as follows for clarity:

“There was a strategy and action plan, but a detailed implementation plan which provided enough direction for the execution of the strategy implementation was missing”

14.
p. 19 community consultation (line 35) – who (which manifestations of community) were participants referring to as not having been consulted?

What we have written is correct – the issue participants highlighted was that a broader consultation could have been conducted with more community stakeholders – they did not specify the detail of which stakeholders had not been consulted.

We have reworded this section for clarity:
“a few participants highlighted that a broader range of community groups could have been consulted.”

***Reviewer 2***

1.
The method section lacks information on methodology. The section should include information on the researcher's theoretical or paradigm position. For example, are the researchers approaching this study using logical empiricist, critical theory, interpretivist, pragmatic or critical realist ontology and epistemology? It is not sufficient to state that you undertook rapid thematic analysis using CFIR. That statement describes the method used but not the methodology.

We agree. We have addressed this point by concisely stating our methodological position – we use a pragmatic epistemology – together with an appropriate reference (reference 21). We have also amended the COREQ supplementary file to cover this issue.

2.
Having added a Methodology paragraph, the Method section should be expanded to include standard content in qualitative studies, including sampling method, reflexivity and evaluative rigour.

We have revised this section to concisely cover these issues. We now state the sampling method and address a key aspect of reflexivity (researcher positionality) in the text. We refer to the now revised COREQ supplementary file for further detail on evaluative rigour and reflexivity.

3.
The analysis section should include more information on the coding approach. Was coding open or closed? What mode of analytical reasoning was used? Was the reasoning only inductive or did to use reproductive, retrodictive, abductive and deductive modes of analysis? The mode of analysis will relate to the methodology and philosophical approach taken.

We have extensively revised the analysis section to make clear that we used Gale and colleagues' framework-guided deductive rapid analysis approach (reference 24). We set out in detail the steps we used. This approach, as we further note in the discussion, is consistent with a pragmatic epistemology – as it allows for the rapid production of actionable knowledge.

4.
From the COREQ checklist, there does not appear to have been any checking back with the participants. This will impact the evaluative rigour. This should be commented on in the limitations section.

We have amended the COREQ supplementary file to state that "no formal member or participant checking was carried out. We did, however, share our preliminary findings in periodic colloquiums targeted to participants from the Southern health system."

We have further added a sentence in the limitations section regarding this.

5.
Results
It is observed that the Health Care Home initiative was seen as being successful. In the paper, it should be noted that this "new" initiative had new funding and did not require the "actors" to modify or change their resource allocation to achieve the implementation goals.

We agree. We present this information in the results section and we further refer to this in our linked study (Reference 29).

6.
You note in the paper the shortcomings of the "rapid approach" and observe that a realist-informed approach might have enabled you to identify the underlying mechanisms that contributed to success or non-success in achieving the desired outcomes. There were at least four "mechanisms" identified

in the analysis including: "trust", "willingness to share power", "communication" and "leadership". They may have been others. You have identified these without formally using a realist retroductive or abductive analysis. You might consider reflecting on these possible underlying mechanisms in your discussion.
We agree. This is a very interesting point which unfortunately, due to word count constraints, we can cover only briefly in this paper.

We have added a reflection on this at the end of the limitations section of the Discussion.

7.
In the New Zealand context, you might reflect on the apparent sector emphasis on "management" and "managerialism" and the possible weakness, as evident from your results, of sector "leadership".

We have addressed this by adding a couple of sentences, with two new references (43 & 44), on the influence of managerialism as a guiding principle for organisation in the implications for health policy and practice section of the Discussion.

8.
You might consider emphasizing the importance of leadership in the Discussion, Conclusion and Abstract. I believe that there is a need in New Zealand during the current reform process to highlight the importance of leadership.

We have now made specific reference to the importance of the need for a stable and committed leadership in the discussion (implications for health policy and practice), conclusion and abstract.
9.
Minor
Page 17, line 10 should poll be "pool"
Thank you. We have corrected this.

***Editor's comments:***

1.
Please include all information regarding informed consent in the main manuscript. Please clarify whether consent was written or verbal.
We have added this additional sentence to the methods section: “Written informed consent was obtained from all participants”.

**VERSION 2 – REVIEW**

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| **REVIEWER** | Tenbensel, Tim<br>University of Auckland, Health Systems |
| **REVIEW RETURNED** | 13-Oct-2022 |

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| **GENERAL COMMENTS** | I am very satisfied with the changes made. All issues have been addressed comprehensively. I look forward to seeing the publication of this paper |

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| **REVIEWER** | Eastwood, John<br>The University of Sydney |
| **REVIEW RETURNED** | 29-Sep-2022 |

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| **GENERAL COMMENTS** | Thank you for the opportunity to review this important study |