

## Peer Review File

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### Reviewer A

The authors present an interesting and large series of patients in with an important problem in a rural setting as well as a novel way of managing these patients.

However, there are a number of issues that need to be addressed before this paper is suitable for publication:

1. The title doesn't accurately reflect the content of the paper. The majority of the paper discusses pinna perichondritis and abscess with only two cases of the PinCH technique. The title needs to be reworded. The objective needs to be modified accordingly
  - a. *Reply 1: Thank you for highlighting this, it is absolutely correct. I have amended the title to better reflect our primary outcome which was to review 10 years of cases. I have also amended the Objective section of the Abstract,*
  - b. *Changes in the text: Title changed to "Pinna perichondritis in a rural setting: a 10-year review of cases and an introduction to the PinCH (Pinna Cot-splint Hack) as a novel technique for improving cosmetic outcomes following pinna abscess" (lines 1-3). Working title changes to "Pinna perichondritis in a rural setting and introduction to the PinCH technique" (line 19). Objective section of abstract reworded (lines 24-27).*
2. It is not clear what the author's focus is on – is it on all types of perichondritis or just piercing related perichondritis? That's because only one fifth of cases in this cohort was piercing related and yet the majority of the discussion in the paper is on this aetiology. A broader discussion needs to be made to review other equally common causes
  - a. *Reply 2: Thank you for this comment, I apologise for the reason for focusing on piercing related cases not being clear. In our 10 year review, cartilaginous piercing was seen to be the most frequently occurring cause for development of pinna perichondritis with and without abscess, and was the identified cause in the two cases that I (Dr Jaensch) was directly involved in. However, due to the low numbers of piercing associated perichondritis in 10 years, I felt that a broader scope was appropriate in our retrospective review to identify how prevalent piercing related infections were compared to all causes. I felt that the scope of the literature review could be appropriately narrowed to mainly focus on piercing associated infections, and have included a note in the abstract to highlight this*
  - b. *Changes in the text: abstract objective (lines 31-32), abstract results (lines 38-40).*
3. Could the authors justify the two case series presented? Perhaps the focus needs to be on the technique. In addition, the outcomes of the second case is over a short period.

- a. *Reply 3: Thank you for this comment. Indeed, the reason for including these two cases was because they were the cases for which the PinCH technique was developed. I have elaborated on the reason for its inception at the end of the methods section (“Use of the PinCH technique”) and added a paragraph in the discussion describing its development*
- b. *Changes in the text: methods lines 161-171, discussion lines 385-399*
4. Line 32 – what do the authors mean by indigenous patients had increased abscess formation? Is this in comparison to which group?
- a. *Reply 4: apologies, the comparator was missing. Has been amended – compared to non-indigenous patients. However as later mentioned, the statistic of abscess formation in ATSI vs non-ATSI was not quite significant, so this statement has been removed, but the comparator in relation to the other statements (pseudomonas and need for surgery) has been amended*
- b. *Changes in the text: lines 34-38*
5. Line 47 – Is this technique only applicable to piercing associated pinna abscess
- a. *Reply 5: Thank you for highlighting this. I had included only piercing associated abscess in this section of the abstract for brevity purposes as this was the cause discussed in our two cases, but it does not reflect the authors’ belief that this splint could be used for any case of pinna collection. This has been amended but I have ensured that the abstract remains under 350 words.*
- b. *Changes in the text: lines 51-52 in conclusion of abstract, discussion lines 337-351*
6. Line 53 – How often does this complication arise?
- a. *Reply 6: apologies for the oversight on this point, statistic has been included.*
- b. *Changes in the text: Introduction line 65*
7. Line 55 – punctuation error
- a. *Reply 7: Amended, thank you*
- b. *Changes in the text: line 60*
8. Line 58 – what is perichondral fibrosis? Is this fibrosis of the perichondrium?
- a. *Reply 8: have reworded sentence, meant to describe fibrosis in place of previous cartilage, near (peri) the perichondrium, poorly worded before, my apologies.*
- b. *Changes in the text: introduction line 63*
9. Line 61 – has there been an increase in the incidence in rural Australia?
- a. *Reply 9: No previous research has looked specifically at rural incidence, I am unfortunately unable to comment on whether rural incidence has increased based on the data in my study compared to what is available*

- b. *Changes in the text: n/a*
10. Line 71 – it is worth discussing other causes of perichondritis
- a. *Reply 10: I feel that the scope of the retrospective analysis should include all causes of perichondritis, as it enabled us to demonstrate that piercing was the most common cause. Once it was shown that piercing was the most common cause (as well as being the cause in the two cases where the PinCH could be used), I feel that it was appropriate to narrow the scope of the literature review/discussion to mainly focus on piercing causes. Language has been amended throughout abstract, methods and discussion to reflect this.*
- b. *Changes in the text: abstract, methods, discussion*
11. Line 85 – grammar error - ? Compared to
- a. *Reply 11: Thank you for highlighting this, I have reworded the primary aims to clarify.*
- b. *Changes in the text: introduction lines 91-92*
12. Line 213 – recommend “without a general anaesthetic” rather than awake
- a. *Reply 12: Thank you for this suggestion, this has been amended*
- b. *Changes in the text: line 234*
13. Line 330 – recommend “evaluation” rather than “experience”
- a. *Reply 13: Thank you for this suggestion, this has been amended*
- b. *Changes in the text: line 368*
14. Line 341 – Can the authors clarify what does this sentence mean? There doesn't appear to be a clear basis to suggest abscess formation is commonly seen in indigenous patients who in fact comprised of only 23% of the cohort. Whilst 70% of these patients had an abscess the difference compared to non-indigenous patients is only just approaching significance. Given the small numbers firm conclusions can't be drawn from this.
- a. *Reply 14: Thank you for this comment. The P-value of 0.05 was tantalizingly close to significance but I have since amended this in the results section to state non-significance and removed the statement from the conclusion section*
- b. *Changes in the text: Conclusion line 380, results 168-170*

## **Reviewer B**

The authors have presented a retrospective analysis of perichondritis (with or without a complicating auricular abscess) cases requiring admission to their institution over a 10-year period. Their most recent 2 cases of pinna abscesses were treated using the delayed use of a moulded finger cot splint after surgical drainage.

The paper requires a number of changes.

1. The paper is that of a retrospective analysis of perichondritis cases requiring admission (as outlined above). This is the stated primary aim (line 83) and should be reflected in the title. The title should read "The management of perichondritis and pinna abscesses in a regional setting: A 10-year retrospective analysis" or similar.
  1. *Reply 1: Thank you for highlighting this, it is absolutely correct. I have amended the title to better reflect our primary outcome which was to review 10 years of cases. I have also amended the Objective section of the Abstract,*
  2. *Changes in the text: Title changed to "Pinna perichondritis in a rural setting: a 10-year review of cases and an introduction to the PinCH (Pinna Cot-splint Hack) as a novel technique for improving cosmetic outcomes following pinna abscess" (lines 1-3). Working title changed to "Pinna perichondritis in a rural setting and introduction to the PinCH technique" (line 19). Objective section of abstract re-ordered (lines 29-30).*
2. How can an abscess be treated without surgical management? And the converse - how can an simple pericondritis be be managed surgically in the absence of an abscess? This needs to better explained, and I suspect the explanation included procedures performed under LA that might otherwise be counted as non-surgical cases.
  1. *Reply 2: Thank you for highlighting this oversight. I have provided better definition for surgical management in the methods section of the paper. I have also mentioned that needle aspirate was not considered adequate surgical management. I have removed the statistical statement re: abscess increasing need for surgical intervention as it was misleading and not clinically relevant.*
  2. *Changes in the text: 142-146, results 168-170.*
3. The actual use of the finger cot dressing in their cases presented should be succinctly explained. A possible example follows. "our most recent two cases of pinna abscesses were treated (after surgical drainage) using a moulded finger cot splint, applied during the post-operative period, although importantly not immediately post-operatively during which time a pressure dressing was initially placed".
  1. *Reply 3. Thank you for this comment. The section titled "Use of the PinCH technique" appears in the methods section before the results, and details the technique itself. Wording has been amended to clarify recommended use*
  2. *Changes in the text: lines 161-171 at end of methods section revised.*
4. Less emphasis should be placed on the novel use of the finger cot dressing overall, because it was only used twice in 10 years, and only really appears to be used as a delayed adjunct to the initial surgical drainage and pressure dressing application. It might also be worthwhile to state the obvious "we plan to continue the use of the finger cot dressing where appropriate as our early experience of it shows promise. Over time, with more experience, it may be possible to compare the outcomes of cases in which it has been used to the outcomes of cases in which it was not used. Having used it in only 2 cases to date, however, this analysis is clearly not possible at this current time.

1. *Reply 4: This is a very valid point, thank you. The reason for emphasizing its use is that I (Dr Jaensch) was directly involved in the care of these two patients, as they were the most recent of the 30 patients included in the 10 year review. The technique was developed with these two patients in mind as they were both young female patients with concerns for cosmesis and who were not able to travel to the hospital for regular compression bandage changes nor were they willing to wear said bandage for the required period of time in public. The splint represented a much more physically acceptable compression method which they were compliant with. The splint was used in these patients following conventional head bandaging after they expressed concerns regarding physical appearance of bandaging. A new paragraph regarding the inception of the splint has been included in the discussion*
2. *Changes in the text: discussion lines 385-399*

### **Editorial Comments**

1. Please highlight the type of this study in the Title / Abstract. This should include not only the reported “retrospective” reporting but also whether the article is a cohort or a case-control or a cross-sectional study.

*Reply 1: Thank you for this suggestion, retrospective cohort study has been included in the abstract.*

*Changes in the text: Abstract line 24.*

2. It’s recommended to describe the name of the setting, instead of only stating “our rural hospital”.

*Reply 2: Thank you, this has been amended in the abstract (and in the methods as per comment 5)*

*Changes in text: Abstract line 29*

3. The statistical methods could also be briefly described here. Please also include the details of the methodology: what variables of interest were collected?

*Reply 3: Thank you for this recommendation. This has been amended to briefly outline the data collection methods in the abstract. Abstract 29-33*

4. The authors could consider moving some statements in lines 289-313 to the Introduction. This would elaborate the advantages or disadvantages about the current management of piercing-associated pinna abscesses, which could affirm the clinical significance for using this new technique.

*Reply 4: Thank you for this suggestion, it absolutely does affirm the need for this new technique. Changes have been made.*

*Changes in text: intro 84-89.*

Further details are required to assess the adequacy of methods.

5. Similar to comment 1, authors are suggested to describe the name of the hospital.

*Reply 5: Thank you, this has been amended*

*Changes in text: methods line 100.*

6. Please further provide more detailed eligibility information on the target population.

*Reply 6: Thank you for this suggestion, I have outlined that cases were screened for eligibility using a review of EMR and included in analysis if consistent with perichondritis.*

*Changes in text: Methods 111-117*

7. Please also include the cause of perichondritis in lines 100-101.

*Reply 7: Thank you, this has been included*

*Changes in text: Methods line 114.*

8. Line 105, “ $\chi^2$  tests of association for discrete variables”, we recommend the authors change “discrete” to “continuous”. In addition, there should be “ $\chi^2$  or Fisher’s exact tests” in this manuscript.

*Reply 8: Thank you for this recommendation, statistical analyses have been repeated with Fisher exact tests and updated throughout results. “Discrete” has been replaced with “categorical” – I understand that  $\chi^2$  and fisher’s exact tests are designed to analyse frequencies of categorical variables and not continuous.*

*Changes in text: Results lines 123-126*

9. If available, we would like to see more detail as to the comfort levels of the two cases.

*Reply 9: thank you for this recommendation. I have included comfort and physical appearance of splint in the case series.*

*Changes in text: Case series, lines 207, 229.*

10. It's suggested that the authors perform normality test to determine which statistical analysis to be applied. Normal data distribution in such a small group seems unlikely. It appears that the publication requires statistical re-analysis using non-parametric tests. Meanwhile, the authors should make it clear that which statistical analysis was applied in the Methods.

*Reply 10: Thank you for identifying this, indeed Shapiro Wilk test revealed neither age nor symptom time were normally distributed, as a result these have been re-analysed with Mann-Whitney U tests. Table 1 has been updated to reflect median being used*

*Changes in text: Methods 123-128, Results 153-191, Table 1*

11. Please provide the associated parameters of measurement uncertainty as appropriate (e.g., confidence intervals) and avoid providing only the results of statistical hypothesis tests (e.g., P values). This applies to the results in pages 6-7.

*Reply 11: Thank you for this recommendation. As mentioned earlier, the small sample sizes are not normally distributed, so mann-whitney tests have been used for re-analysis in place of chi-square. As a result, no 95% confidence intervals for means have been presented because median has instead been used as a measure of central tendency*

*Changes in text: n/a*

12. It would be better to show the figure by 4 months after using PinCH of Case 2. The authors could consider replace it with a new one, if available.

*Reply 12: Thank you, yes I agree, unfortunately a clinical photograph was not taken at 4 months and the patient moved back to the Philippines and was not able to be contacted.*

*Changes in text: n/a*

13. We note only two cases were treated by this technique. It seems it was not broadly used in your setting in the last ten years. Then how about the applicability or popularity in other rural area of Australia, if there were available data? What has been the barrier for adoption in rural area: Risk of contrast?

*Reply 13: This method was developed by Dr Jaensch in 2021 in Dubbo Base Hospital while treating the two patients included in the case series. We have not yet had the opportunity to implement this method in a wider setting, but look forward to the opportunity when it arises and will continue to collect data in order to analyse its effectiveness compared to traditional methods.*

*Changes in text: n/a*

14. There is a significant finding on abscess formation in Indigenous patients in the Results, yet it was not well organized in the Discussion (only mentioned in the Conclusion). The authors could add some additional text to elaborate this point.

*Reply 14: unfortunately, once re-analysed with Fisher exact test, this result was not significant  $p=0.08$ , so it was removed from the results section and not discussed further.*

*Changes in text: n/a*