

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Climatic and community sociodemographic factors associated with remote Indigenous Australian smoking rates – an ecological study of health audit data
AUTHORS	Carroll, Suzanne; Dale, Michael; Bailie, Ross; Daniel, Mark

VERSION 1 - REVIEW

REVIEWER	David Thomas Menzies School of Health Research Ross Bailie was previously at Menzies in the same research division, before he left to take up his current position a few years ago.
REVIEW RETURNED	24-Aug-2018

GENERAL COMMENTS	<p>This is a potentially interesting and well-written study looking at associations between community variables and higher or lower community smoking prevalence in remote Indigenous communities in Australia. However, the variables used may not be suitable in assessing CAUSES of this variation.</p> <p>p.3. l.20. Clinical records may be a poor method to assess smoking prevalence. A weak outcome measure threatens any paper. They need to discuss this more. 42% of their records do not have smoking status recorded. In the text they do not suggest this due to random deficiencies but later in the text this is not described as random but due to deficient health assessment and introducing bias.</p> <p>p.3.l.24. In the text they assert that these findings may be generalisable to other settings but not how. Please describe how and why.</p> <p>p.4.l.24. Please update to latest AIHW estimate: 23%.</p> <p>p.4.l.36-45. Given all these previously asserted causes are likely to vary between communities, and geographic regions, why have the authors largely ignored these social and historical factors, and jumped to climate?</p> <p>p.5.l.39. Seasonality effects within a region does not imply geographic variation between regions. This could be dropped.</p> <p>p.6. l.39. Not sure why excluding those with chronic diseases caused by smoking. This will falsely lower prevalence reported, especially as these diseases are very common in this population.</p> <p>p.7.l.19. Please explain how many ILOCs were so excluded.</p> <p>p. 8.l. 7-19. Why did they not consider age-standardisation of their community prevalence estimates which is standard practice for comparing smoking prevalences between populations? Or some equivalent statistical method. I am uncertain that controlling for median age is sufficient.</p>
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	<p>p.8. l.44. Does this connectivity also measure connectivity with predominantly non-Indigenous centres, eg regional towns? Or are they covered as they also include an Indigenous population?</p> <p>p.8. l. 51. I can't quite follow their definition of 'heat', please re-write how calculated so is clearer.</p> <p>p. 8. l.47-57. If the underlying climate construct they are measuring is climate discomfort causing stress, are there not much better measures to use that work in both deserts and the tropics (rather than just separating these two regions). E.g. rain in the tropics does not cause stress, it is the humid months leading up to seasonal rains. When the rains come stress reduces.</p> <p>p.9. l.18. While the statistical methods look ok to this non-statistician, I wonder why multi-level modelling was not used as is more typical and neater for these sort of analyses. However it would require a quite different analytic approach.</p> <p>p.15. l.36-41. This variation only matters if tobacco control interventions have different effectiveness in high and low prevalence populations. Please describe this evidence.</p> <p>p.15.l.49++. While it is possible that larger communities (with more jobs and resultant income and more access to education) may have greater exposure to negative social factors that lead to smoking initiation and relapse), it is worth also considering the positive factors in smaller communities – that have driven the outstation movement. The smoking differences may also reflect compositional rather than contextual factors. Health-oriented and healthier people may choose to live on outstations.</p> <p>p.18. l.24. Nowhere do the authors adequately discuss historical reasons related to the introduction of tobacco for the difference between the rainy humid top end and hotter drier centre. This is well known in the NT, and recognised as the cause of dramatically smoking prevalence, esp among women. Smoked tobacco was introduced in the top end hundreds of years ago by Macassans and smoked by men and women. In the Centre, native tobaccos were largely chewed, with commercial tobacco introduced much later as part of colonialism, and mainly smoked by men. The association with population size may also in part reflect this history, as desert communities are usually smaller than those in top end.</p> <p>p.18. l.52. The reliance on the blunt prevalence measure is also a limitation: it does not distinguish between initiation and cessation as the drivers. E.g. While soc-ec gradients in prevalence and initiation and prevalence are usually found, they are less commonly found in cessation (at least not in prospective longitudinal research). This is important for policy makers.</p>
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REVIEWER	Dr Alice Richardson Australian National University, Australia
REVIEW RETURNED	08-Oct-2018

GENERAL COMMENTS	<p>In this paper, the authors have conducted secondary analysis of an audit of health centres, in order to examine risk factors influencing smoking rates at a community level. I found the analysis to be at a fairly basic level, with odds ratios from two-by-two tables forming the core of the analysis.</p> <p>Below I will elaborate on the four items in the Review Checklist where my response was "No". They are of a largely minor nature and so I recommend that the paper undergo minor revision before resubmission.</p>
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	<p>Q2: The abstract should mention the number of communities as well as the overall sample size.</p> <p>Q7: Secondly, the sensitivity analysis around dichotomising at the median seems to be dismissed too rapidly. The presence of a zero cell should not automatically remove the possibility of a statistical analysis and the authors should explore methods to deal with small cell counts in order to provide a fuller picture of the analysis conducted.</p> <p>Q10: In Table 1, average annual cooling degree days are not intuitive, and could do with a footnote to explain the units of measurement.</p> <p>Q12: No limitations are given in the “Strengths and limitations” section which seems unlikely. The authors should consider the weaknesses in their design and report them there accordingly. Firstly, nearly a third of the audit records were excluded (page 8 line 17). Secondly, the total dichotomisation of variables, with the attendant loss of information, should also be mentioned at this point.</p> <p>Thankyou for the opportunity to be part of the academic referring system in this way.</p>
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REVIEWER	Kathryn Panaretto Assoc Professor Centre for Chronic Disease Faculty of Medicine University of Queensland
REVIEW RETURNED	18-Oct-2018

GENERAL COMMENTS	<p>There has been a reasonable amount of literature produced on aboriginal tobacco use in recent years in Australia. These papers cover many aspects of tobacco use and its relationship to community well being eg Bond, Lovett, Thomas</p> <p>Arguably this paper does not contribute a lot to what we already know. The major problems seem to be in the methods or definitions used which makes interpretation of the data difficult and conclusions broad and arguably not novel. In addition the data is fairly old.</p> <p>The connectivity is probably the most interesting part and may be better served with a geospatial approach. A map would greatly assist international readers – well Australian also. The references in the introduction to a city then region is not easily comprehended without more context.</p> <p>The paper could be much shorter possibly just a brief report.</p> <p>Specifically: Definitions: Education – what are the standard categorisations for education status? I am not sure the definition used is sensitive enough to detect the graduation in tobacco with increasing levels of education, particularly those with post school qualifications.</p> <p>Heat: is this a standard definition? The number of high heat days seems fairly low. Is using mean temperature which I assume includes night temperatures appropriate – how much smoking occurs between 10 pm and 6 am? Can references be provided? Was advice sought from meterologists? Geographers - desert vs wet vs dry tropics</p> <p>Rain: again this definition seems to be used loosely and in the discussion referred to as frequent heavy rain. Is this consistent BOM definitions? And the rainfall days were pretty low and clearly will be community location dependent</p>
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	<p>Communities with a population of 111: Should these be excluded? There will be a lot of variability in these samples. Say 60% of the population is over 15, and 1 family leaves town, the impact on the number of smokers could be large, the prevalence of smoking may move up and down a lot.</p> <p>Text</p> <p>Results: are hard to read with much use of 'null'. A broad section of the health sector may read such a paper and it would be useful if the text was easy to understand.</p> <p>Discussion: too long for what has been found. There is no discussion about the role of employment – many workplaces are smoke free now in remote areas, so people will smoke less while at work. Most houses are smoke free and depending on the community possibly air conditioned.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1, Reviewer Name: David Thomas

R1.0 - This is a potentially interesting and well-written study looking at associations between community variables and higher or lower community smoking prevalence in remote Indigenous communities in Australia. However, the variables used may not be suitable in assessing CAUSES of this variation.

We thank the reviewer for the compliment regarding the paper. We have ecologically assessed community level correlates of smoking rates. As identified by Reviewer 3, this includes novel climatic and connectivity measures and, we believe, adds to the scant literature regarding inter-community variation in smoking rates in remote, predominantly Indigenous communities. The current reviewer highlights his interest in causes of such variation, particularly (see comment 1.15) underlying historical causes. These causes are well established and are extensively treated in the literature we cite ¹, and generally, outside the remit of this paper. However, for further detail on our treatment of the factors, please see particularly our response to 1.4 and 1.15.

1. Scollo M, Winstanley M. Tobacco in Australia: facts and issues. Melbourne, Australia: Cancer Council Victoria; 2017 [Available from: www.TobaccoInAustralia.org.au accessed May 30 2018].

R1.1: p.3. l.20. Clinical records may be a poor method to assess smoking prevalence. A weak outcome measure threatens any paper. They need to discuss this more. 42% of their records do not have smoking status recorded. In the text they do not suggest this due to random deficiencies but later in the text this is not described as random but due to deficient health assessment and introducing bias.

Given the sensitive nature of collecting data in Indigenous communities and the consequent lack of information regarding small geographic region (including community-level) smoking rates we believe our approach is warranted. A study recently highlighted the lack of small-geographic scale smoking prevalence data and the need for improved strategies of data collection that would enable small-scale estimation of rates ². Given that the national program for Indigenous Tobacco control uses a regional model approach ³, small-geographic scale estimates are urgently required to understand and track the effect of anti-smoking campaigns ². Indeed, the difficulty of obtaining such information has motivated some researchers to use innovative approaches including the use of retail cigarette sales audit data

(combined with various assumptions including assumed smoking prevalence) in efforts to estimate cigarette consumption and therefore health-risk ^{4 5}.

Whilst we acknowledge the use of audit data is less than ideal, we believe the use of these data increases our current understanding of the extent of the smoking problem in remote Indigenous communities, especially given the dearth of small-geographic scale smoking information. We clearly acknowledge the need for better quality data within the manuscript (Discussion, 2nd para). The lack of smoking information in the health records is indeed concerning, not just due to potential bias that may be introduced in expression of our outcome measure (which is acknowledged in our paper, see the last paragraph of the Discussion), but it implies an inconsistent approach by health practitioners to the tracking and (potentially) treatment of what is a major health concern. This is also highlighted in the paper (Discussion, 9th para) and should be of great concern to public health and health promotion. We believe that the benefit of presenting these analyses outweighs the methodological concerns raised by the reviewer.

2. Wright A, Lovett R, Roe Y, et al. Enhancing national data to align with policy objectives: Aboriginal and Torres Strait Islander smoking prevalence at finer geographic levels. *Australian Health Review* 2017: doi: 10.1071/AH16269

3. Australian Government Department of Health. Tackling Indigenous Smoking 2017 [Available from: <http://www.health.gov.au/internet/main/publishing.nsf/content/indigenous-tis-lp> accessed 18 Dec 2018].

4. Butler R, Chapman S, Thomas DP, et al. Low daily smoking estimates derived from sales monitored tobacco use in six remote predominantly Aboriginal communities. *Australian & New Zealand Journal of Public Health* 2010;34(Suppl 1):S71-S75. doi: 10.1111/j.1753-6405.2010.00557.x.

5. MacLaren D, Redman-MacLaren M, Clough A. Estimating tobacco consumption in remote Aboriginal communities using retail sales data: some challenges and opportunities. *Australian and New Zealand Journal of Public Health* 2010;34:S66-S70. doi: 10.1111/j.1753-6405.2010.00556.x

R1.2: p.3.I.24. In the text they assert that these findings may be generalisable to other settings but not how. Please describe how and why.

As stated on p 3, line 24 (original submission), the study results are generalisable to other Australian remote Indigenous communities not included in the study, that is, other similar communities that were not directly studied. Such similar communities are likely to also be subject to the same associations between environmental exposures and smoking rates. The results may also be broadly generalisable to remote-dwelling indigenous populations in other high-income countries as such populations have similar characteristics and have experienced similar historical exposures. Some details of the exposures are, of course, different, thus "broadly generalisable". These details on how and why we believe the findings may be generalisable to other settings are not provided within the 'Strengths and Limitations' section as this is limited to five short bullet point style statements no longer than one sentence each, expressly relating to the methods of the paper (only). Greater detail has been added in the main manuscript text (final paragraph of the discussion, changes italicised):

"Though specific to Australian remote Indigenous communities, these findings may be broadly generalisable to other remote-dwelling indigenous populations in high-income countries as such populations have similar characteristics and have experienced similar historical exposures."

R1.3: p.4.I.24. Please update to latest AIHW estimate: 23%.

The smoking prevalence rates provided in the manuscript have been updated to reflect the most recent AIHW release ⁶. Thus, the relevant line now reads (Introduction, 1st para, changes in bold):

“In Australia, Aboriginal and Torres Strait Islander peoples (hereafter Indigenous Australians) are 2.7 times as likely to smoke daily as non-Indigenous Australians, with age-standardised prevalence rates of 42% and 15% respectively ⁶.”

We are uncertain as to the source of the reviewer’s ‘latest AIHW estimate of 23%’.

6. AIHW. Australia’s health 2018. Australia’s health series no. 16. AIHW: Canberra, 2018.

R1.4: p.4.I.36-45. Given all these previously asserted causes are likely to vary between communities, and geographic regions, why have the authors largely ignored these social and historical factors, and jumped to climate?

We agree that the previously asserted causes (i.e., those described in the Introduction, 2nd para) may well vary between communities and geographic regions – that these factors are important is well-established and further highlighting this point is not the objective of the current paper. Rather than ignoring these factors, we cite literature¹ that covers these issues at length and we commit a page complete (Discussion, 5th through 7th para) to a discussion of how these factors and their subsequent effects might interact with or be associated with our observations, and the implications for interventions. In addition to this, we add to the literature by assessing other measures (novel climate and connectivity measures in addition to more established measures including demographic characteristics) to identify community-specific correlates of smoking. Our climate measures are only two of nine variables we assess to investigate these associations.

R1.5: p.5.I.39. Seasonality effects within a region does not imply geographic variation between regions. This could be dropped.

We agree with the reviewer that seasonal variability and geographic variation are not synonymous. Our point seems to have been unclear – that seasonality (in both broad [national] and smaller [state level] geographic areas) demonstrates a pattern consistent with higher temperatures being associated with higher smoking rates (which effect may be attributed in part to stress and anxiety due to higher temperatures). We have edited the text to clarify the point being made (Introduction, paragraph 5, changes italicised):

“Cigarette sales across the US demonstrate seasonality, increasing in the summer months ¹⁹, with this pattern also evident within smaller geographic areas (i.e. in New Jersey) ²⁰. This apparent association between temperature (higher in summer months) and smoking may be the result of the relationship between extreme high temperatures and negative affective states, stress and violence ²¹ ²², as stress is well accepted as being linked to smoking ⁹ ²³ ²⁴. Extreme high temperatures may influence smoking behaviour by increasing stress and anxiety levels.”

R1.6: p.6. I.39. Not sure why excluding those with chronic diseases caused by smoking. This will falsely lower prevalence reported, especially as these diseases are very common in this population.

We agree with the reviewer that this exclusion will likely result in the underestimation of the prevalence of smoking which, given the generally high prevalence, is particularly disturbing but also highlights the importance of this work. It should be clear from the Sampling section of our Methods section (page 6 Line 30-46) that this exclusion is unavoidable as it is a function of the (upstream) ABCD Project protocols, rather than a choice by the authors. Nonetheless, the reviewer is correct to flag the underestimation that flows from this circumstance, and we have thus added the following text within the limitations section of our discussion (final paragraph of discussion section, additional text italicised):

“Individual-level audit record sample loss due to missing smoking information may have introduced bias to the data and be indicative of deficient health assessment and data collection procedures at the local health service level. Similarly, the use of audit records creates a selection bias (e.g., not including records with chronic diseases) so our results likely under-estimate the prevalence of smoking.”

R1.7: p.7.I.19. Please explain how many ILOCs were so excluded. (multiple nearby and very small communities)

Three such ILOCs were excluded. This has been added to the text of the methods section (additional text italicised).

“Some (n=3) ILOCs include multiple nearby and associated very small communities. Communities belonging to such ILOCs were excluded from this study.”

R1.8: p. 8.I. 7-19. Why did they not consider age-standardisation of their community prevalence estimates which is standard practice for comparing smoking prevalences between populations? Or some equivalent statistical method. I am uncertain that controlling for median age is sufficient.

First, we lack the individual level records that would be required for us to construct individual community-level smoking rates by age for our sample (see also our response to 1.12) to enable standardisation to be conducted.

Second, even if we had access to the individual level data, the minimum number of categories for standardisation (above the median split) would be 3 age and 2 smoking status categories (i.e. 6 cells in a 2 x 3 table). Given our sample of 2689 records from 70 communities, the mean number of records per community (38.4) is such that smaller communities or communities with a smaller number of retained records would be likely to suffer substantial data sparseness issues (i.e. a greater frequency of zero or very small cell counts within small community samples).

R1.9: p.8. I.44. Does this connectivity also measure connectivity with predominantly non-Indigenous centres, eg regional towns? Or are they covered as they also include an Indigenous population?

No, this measure is specific to connectivity with other predominantly Indigenous communities and therefore does not include communities that are predominantly non-Indigenous. This is stated in the text:

“Geographic connectivity was expressed as a count of other Indigenous communities within a 250km road-network distance ²⁸ (mainland communities only; n=56).”

For further clarification of the communities used in “Geographic connectivity” we provide (within the manuscript) the reference “28. Department of Human Services. 2013 Australian Government Indigenous Programs & Policy Locations (AGIL). Canberra: Department of Human Services, 2013.”

R1.10: p.8. l. 51. I can't quite follow their definition of 'heat', please re-write how calculated so is clearer.

The definition of 'heat' has been expanded to improve clarity, as shown below (additional text italicised). Additional commentary regarding this measure is also present in our response to comment 3.7.

"Climate profiles were obtained from surface maps sourced from the Australian Bureau of Meteorology for the period 1961-2012 ²⁹. Community-level climate measures representing heat and heavy rain were determined as follows. Heat was operationalised as the average of the annual sum of cooling degree.days in each community. 'Cooling degree.days' is a standard measure ³⁰ defined as the number of degrees by which a day's mean temperature exceeds 18°C. For example, a day with a mean daily temperature of 25°C would attract a cooling degree.days score of 7 degree.days, whereas as day with a mean daily temperature of 16°C would attract a score of 0 degree.days. Heavy Rain was operationalised as the mean of the annual number of days with greater than 25 millimetres of precipitation."

R1.11: p. 8. l.47-57. If the underlying climate construct they are measuring is climate discomfort causing stress, are there not much better measures to use that work in both deserts and the tropics (rather than just separating these two regions). E.g. rain in the tropics does not cause stress, it is the humid months leading up to seasonal rains. When the rains come stress reduces.

Whilst we acknowledge that seasonal variation in temperature and rainfall may influence stress levels (e.g. the example given by the reviewer of the effect of the onset of rains), the data we use here for climatic variables are climate profiles for each community (see 'Methods: Independent Variables', but also our expanded definitions in response to 1.10, above). As we are using the average of the sum of annual values, we are not able to assess the effect of seasonal variation in climate on smoking behaviour. Rather, we limit our comment to the associations between different climatic conditions and smoking rate broadly. Seasonal variation is a nuance to be explored subsequent to establishing the associations we have demonstrated herein.

R1.12: p.9. l.18. While the statistical methods look ok to this non-statistician, I wonder why multi-level modelling was not used as is more typical and neater for these sort of analyses. However it would require a quite different analytic approach.

This study is ecological in design, with the aim being assessment of associations between community-level exposures and community-level smoking rates. A multi-level approach would not suit the stated aims of this study. Additionally, multi-level modelling of these data would require permission to use individual-level records, permission which we do not have.

R1.13: p.15. l.36-41. This variation only matters if tobacco control interventions have different effectiveness in high and low prevalence populations. Please describe this evidence.

We respectfully disagree with the reviewer on this point. If the intervention effectiveness is invariant, understanding the geographic variation in smoking rates would only be irrelevant if sufficient intervention resources were available for the deployment of these resources to remote communities to

be spatially uniform (i.e. ubiquitous). However, due to consistent under-resourcing of tobacco control programs for Indigenous Australians⁹, understanding the geographic variation retains utility regardless of any prevalence-based effect variations, insofar as it can inform the utilisation of scarce resources in the most efficient manner. Moreover, given the sparseness of community-level smoking rate data available within the literature, the identification of community-level smoking rate correlates (and the variation thereof) has value both in providing potential levers to effect change but also as these correlates may assist in identifying communities at heightened risk of elevated smoking rates until more complete community level smoking data become available.

9. Power J, Grealy C, Rintoul D. Tobacco interventions for Indigenous Australians: a review of current evidence. *Health Promotion Journal of Australia : official journal of Australian Association of Health Promotion Professionals* 2009;20(3):186-94. [published Online First: 2009/12/03]

R1.14: p.15.l.49++. While it is possible that larger communities (with more jobs and resultant income and more access to education) may have greater exposure to negative social factors that lead to smoking initiation and relapse), it is worth also considering the positive factors in smaller communities – that have driven the outstation movement. The smoking differences may also reflect compositional rather than contextual factors. Health-oriented and healthier people may choose to live on outstations.

The reviewer raises an important point: health-oriented individuals may self-select to move to smaller communities thus potentially confounding our study findings. Though we think this is unlikely to contribute greatly to the reported associations we have added a comment on this to the limitations section (final paragraph of the discussion, additional text italicised):

“Limitations in the assessment of community-level smoking have been noted in other studies³²⁻⁴². Potential confounding due to residential self-selection toward smaller and potentially healthier communities could not be accounted for.”

R1.15: p.18. l.24. Nowhere do the authors adequately discuss historical reasons related to the introduction of tobacco for the difference between the rainy humid top end and hotter drier centre. This is well known in the NT, and recognised as the cause of dramatically smoking prevalence, esp among women. Smoked tobacco was introduced in the top end hundreds of years ago by Macassans and smoked by men and women. In the Centre, native tobaccos were largely chewed, with commercial tobacco introduced much later as part of colonialism, and mainly smoked by men. The association with population size may also in part reflect this history, as desert communities are usually smaller than those in top end.

The reviewer makes a good point. We did not previously discuss the history of tobacco and smoking within the submitted manuscript though this history is well described in literature cited within the manuscript¹. We now note this fact in the discussion section (2nd para, additional text italicised):

“This variation has important implications for intervention strategies, suggesting the need for localised approaches targeting communities according to smoking prevalence. It is, however, important to note that regional variation in smoking rates has a basis in the history of tobacco usage among Indigenous Australians, with exposure to smoked tobacco (in contrast to the custom of chewing native, nicotine-containing flora) preceding Western colonisation and occurring in littoral regions of the Northern Territory via trade¹⁰.”

R1.16: p.18. l.52. The reliance on the blunt prevalence measure is also a limitation: it does not distinguish between initiation and cessation as the drivers. E.g. While soc-ec gradients in prevalence and initiation and prevalence are usually found, they are less commonly found in cessation (at least not in prospective longitudinal research). This is important for policy makers.

We agree with the reviewer that our measure does not distinguish between initiation and cessation nor assess the drivers of such. While this is indeed important information for policy makers, assessing initiation and cessation and their drivers was not the intent of the study. Given the distinct lack of data regarding smoking rates in small remote Indigenous communities and their ecological correlates we believe that our paper still provides important information to policy makers and health practitioners, particularly in regard to the scope of the problem and that it varies between communities – a fact little regarded in previous research. We have however added a statement to the limitations section highlighting the reviewer's point (final paragraph of the discussion, additional text italicised):

"Finally, this study is ecological and associations between community smoking rates and community factors should not be inferred at the individual level. The environmental correlates of smoking rates stand to differ from the predictors of individual smoking initiation and cessation."

Reviewer: 2, Reviewer Name: Dr Alice Richardson

In this paper, the authors have conducted secondary analysis of an audit of health centres, in order to examine risk factors influencing smoking rates at a community level. I found the analysis to be at a fairly basic level, with odds ratios from two-by-two tables forming the core of the analysis.

Below I will elaborate on the four items in the Review Checklist where my response was "No". They are of a largely minor nature and so I recommend that the paper undergo minor revision before resubmission.

We thank the reviewer for her time and consideration in reviewing our paper.

R2.1 - Q2: The abstract should mention the number of communities as well as the overall sample size.

The abstract includes both the number of communities and overall sample size (highlighted in bold below). Please refer to Abstract, Setting and sample (p 2, lines 12-17):

"Records (n=2689) from an audit of community health centres in the Northern Territory and Queensland were used to estimate smoking rates dichotomised at the median for 70 predominantly Indigenous remote communities."

R2.2 - Q7: Secondly, the sensitivity analysis around dichotomising at the median seems to be dismissed too rapidly. The presence of a zero cell should not automatically remove the possibility of a statistical analysis and the authors should explore methods to deal with small cell counts in order to provide a fuller picture of the analysis conducted.

As we state in our response to comment 1.8, other statistical methods were considered and discarded as not appropriate to our data or relevant to the aim of the study. We used 'exact' options (mid-p and mid-p confidence intervals) estimated using software (WinPEPI) commonly used in epidemiology studies with particularly small cell counts and consequent issues in calculating confidence intervals.

Despite this approach, we still encountered situations where upper limit CI's were inestimable as a result of small cell counts even when dichotomising at the median in order to maximise counts per cell. Given the sparseness of the data with a sample of 70 communities (56 communities with geographic connectivity information) a more 'advanced' statistical treatment is unlikely to provide additional insight.

The approach used in this manuscript is recommended by some of the foremost authorities within the field ¹⁰ as appropriate to the data situation we encountered.

10. Rothman KJ. Modern Epidemiology. Boston: Little, Brown & Co 1986.

R2.3 - Q10: In Table 1, average annual cooling degree days are not intuitive, and could do with a footnote to explain the units of measurement.

Please see our response to comment 1.10, above, for our expansion of the definition of this variable. We have also included a footnote to the table including the definition.

R2.4 - Q12: No limitations are given in the "Strengths and limitations" section which seems unlikely. The authors should consider the weaknesses in their design and report them there accordingly. Firstly, nearly a third of the audit records were excluded (page 8 line 17). Secondly, the total dichotomisation of variables, with the attendant loss of information, should also be mentioned at this point.

One of the 5 brief bullet points in the short strengths and limitations bullet point section is focused on limitations (relating to high sample loss, which is 42.5% at the point the reviewer identifies). Further discussion of limitations is provided in the Discussion section (final para), where such limitations can be presented more fully with appropriate discussion. We add to the limitations section of the Discussion a note on the data variation loss highlighted by the reviewer, as below (additional text italicised):

"Limitations in the assessment of community-level smoking have been noted in other studies ^{32 42}. Potential confounding due to residential self-selection toward smaller and potentially healthier communities could not be accounted for. Given the small sample size and the desire to assess simple associations, the common epidemiological and recommended ⁴³ approach of dichotomising the data at the median was utilised. We acknowledge that the categorisation of these data results in some information loss. Finally, this study is ecological and associations between community smoking rates and community factors should not be inferred at the individual level. The environmental correlates of smoking rates stand to differ from the predictors of individual smoking initiation and cessation."

Reviewer: 3, Reviewer Name: Kathryn Panaretto

R3.1 - There has been a reasonable amount of literature produced on aboriginal tobacco use in recent years in Australia. These papers cover many aspects of tobacco use and its relationship to community well being eg Bond, Lovett, Thomas Arguably this paper does not contribute a lot to what we already know.

We do not believe that the currently available literature in relation to Indigenous smoking sufficiently characterises the scope of the problem nor does the current literature include a focus on community level factors that may influence community smoking rates. For example, there is little information on actual smoking rates, especially in remote regions and, importantly, between remote communities. Our paper highlights that there are substantial differences in rates between remote communities – an important piece of information for public health and health promotion. Additionally, our paper includes assessment of novel BoM climatic variables, and, as the reviewer herself states, novel connectivity measures as potentially important to smoking rate. Finally, it is interesting to note that at least one of the authors cited by the reviewer (i.e. Thomas, Reviewer 1) as covering many aspects of tobacco use and its relationship to community well-being considers this work to be ‘potentially interesting’ – from which phrase we infer ‘of some merit’.

R3.2: The major problems seem to be in the methods or definitions used which makes interpretation of the data difficult and conclusions broad and arguably not novel.

Responses to the reviewer’s concerns regarding definitions are presented below (see our responses to points 3.6-3.8).

We disagree with the reviewer that the conclusions are broad and arguably not novel. There are very limited data available on remote Indigenous community-level smoking rates. This paper provides evidence of geographic variation in remote Indigenous community smoking rates. Remote Indigenous communities are very often considered as if homogenous. Geographic variation in remote smoking rates highlights that this is indeed not the case. This variation in smoking rate is important to public health policy practice, suggesting the need to prioritise targeting communities with the greatest smoking rates, especially given the limited funding available. It also highlights the need to tailor interventions specifically to each community.

Few papers have assessed community-level factors in relation to smoking, particularly remote Indigenous smoking rates. A lack of considering such factors when designing and implementing interventions will likely negatively impact the success of any such intervention. Identifying such factors, a focus of this paper, is therefore imperative, particularly in relation to remote Indigenous communities, where there is a dearth of such information. Indeed, this paper assessed associations of community smoking rate with novel environmental indicators, namely heat and heavy rain, and community geographic connectivity.

Finally, our paper highlights the lack of quality smoking rate information available on this topic – quality data and tracking of rates are needed to assess the effectiveness of health interventions. These data should be at the local community level or at least regional as the national campaign (see earlier response to point 1.1 [above]) is regional in its approach ².

2. Wright A, Lovett R, Roe Y, et al. Enhancing national data to align with policy objectives: Aboriginal and Torres Strait Islander smoking prevalence at finer geographic levels. Australian Health Review 2017. doi: 10.1071/AH16269

R3.3: In addition the data is fairly old.

The data range from ~3.5-7.5 years old at the time of submission of this manuscript. Even accepting the reviewer's premise that this represents 'fairly old' (which we do not), the lack of research reporting between-community smoking rates is such that this study represents a substantial contribution to the literature. Moreover, for the 'age' of the data to be a critical issue, the smoking rates and associations identified within this study would have to be variable over relatively short time frames. Given the relative stability of the overall Indigenous smoking prevalence ⁶, we consider it unlikely that the smoking rates and associations we identify vary substantially within such time frames.

6. AIHW. Australia's health 2018. Australia's health series no. 16. AIHW: Canberra, 2018.

R3.4: The connectivity is probably the most interesting part and may be better served with a geospatial approach. A map would greatly assist international readers – well Australian also.

Given the sensitivities around identifying participating communities, we do not consider that providing a visual representation of the sample area is appropriate. Ethical approvals and project agreements preclude any reporting that could enable identification of communities.

R3.5: The references in the introduction to a city then region is not easily comprehended without more context.

We assume the reviewer refers to p 4, lines 48-50 ("Adult Indigenous Australian smoking rates vary from 39% in Major Cities to 49% in Remote and 56% in Very Remote areas ^{11 12.}"). We believe this to be clear and precise. Given that the other two reviewers had no concerns with the comprehensibility of the paragraph we have made no changes.

R3.6: The paper could be much shorter possibly just a brief report.

We believe that the manuscript length is appropriate for conveying the details of the study. Indeed, given requests from reviewers for more detail regarding measures and acknowledgement of limitations, it would seem that the paper was not actually long enough to adequately convey all important information.

R3.7: Specifically: Definitions: Education – what are the standard categorisations for education status? I am not sure the definition used is sensitive enough to detect the graduation in tobacco with increasing levels of education, particularly those with post school qualifications.

We highlight that analyses were conducted at the community level not at the level of the individual, that is, community factors were assessed against community smoking rates. Consequently, a community-level expression of education was needed. Using a proportion is indeed an established standard for expressing an aggregate population measure ¹¹. Area education is commonly expressed as the proportion of residents who are university educated. Given the generally lower level of

education both in remote areas and in Indigenous populations, using a proportion definition based on university education is not appropriate thus a lower education threshold, proportion grade 10 schooling or greater, was used. Given the ecological nature of the study, that is, assessment of community-level factors in relation to community smoking rate, gradations of education level have not been assessed in relation to gradations of smoking amount as would possibly be done with analyses conducted at the individual level. To enable the simple epidemiological analytic approach (stratified cross-tabulations) conducted due to the inclusion of a relatively small sample (n=70 communities), measures were dichotomised as is described under “Independent Variables” in the methods (also see ‘Methods: Data preparation’).

11. Lynch J, Kaplan G. Socioeconomic Position. In: Berkman LF, Kawachi I, eds. *Social Epidemiology*. New York: Oxford University Press 2000:13-35.

R3.7: Heat: is this a standard definition? The number of high heat days seems fairly low. Is using mean temperature which I assume includes night temperatures appropriate – how much smoking occurs between 10 pm and 6 am? Can references be provided? Was advice sought from meteorologists? Geographers - desert vs wet vs dry tropics

Cooling degree days (and related measures such as heating degree days) are indeed standard measures¹², calculated, used and provided by the Australian Bureau of Meteorology⁸. Cooling degree days (and heating degree days) have been used to characterise climate¹³ and climate change^{14 15}, urban design and heat island effects^{16 17}, and are used in economics including models estimating weather insurance premiums for farmers¹⁸. These measures have been assessed in relation to life satisfaction¹⁹, health outcomes (e.g., body mass index)²⁰ and health behaviours (e.g., physical activity)²⁰, and incorporated in summary measures representing physical environment deprivation²¹.

Cooling degree days incorporates both intensity and frequency of heat exposure and is therefore ideal for use in our study as we are interested in the effect of chronic thermic discomfort (heat exposure), rather than acute response to exposure (e.g., heat wave).

The cooling degree days measure (‘heat’) does indeed use the mean daily temperature and thus is unavoidably influenced by the (typically night-time) minima. We do not perceive this to be a limitation as the literature on heat stress commonly utilises measures that include the daily minimum temperature^{22 23} – this value has an obvious effect on the degree of heat stress experienced over a 24 hour period.

Given that we are not assessing timing of smoking, nor frequency nor amount, but are assessing community smoking rates in relation to climate, the comment regarding how much smoking occurs between 10pm to 6pm would appear to be unrelated to our study and methods. Similarly, given that the heat measure does not directly represent the number of high heat days, the reviewers comment on the number of high heat days does not appear to be relevant.

8. Australian Government Bureau of Meteorology. Annual and monthly heating and cooling degree days 2018 [Available from: <http://www.bom.gov.au/climate/map/heating-cooling-degree-days/documentation.shtml> accessed 17/12/2018 2018].

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14. Sailor DJ. Risks of summertime extreme thermal conditions in buildings as a result of climate change and exacerbation of urban heat islands. *Building and Environment* 2014;78:81-88. doi: <https://doi.org/10.1016/j.buildenv.2014.04.012>
15. Mishra V, Lettenmaier DP. Climatic trends in major U.S. urban areas, 1950–2009. *Geophysical Research Letters* 2011;38(16) doi: 10.1029/2011GL048255
16. Santamouris M. On the energy impact of urban heat island and global warming on buildings. *Energy and Buildings* 2014;82:100-13. doi: <https://doi.org/10.1016/j.enbuild.2014.07.022>
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20. Ewing R, Meakins G, Hamidi S, et al. Relationship between urban sprawl and physical activity, obesity, and morbidity – Update and refinement. *Health & Place* 2014;26:118-26. doi: <http://dx.doi.org/10.1016/j.healthplace.2013.12.008>
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22. Pattenden S, Nikiforov B, Armstrong BG. Mortality and temperature in Sofia and London. *J Epidemiol Community Health* 2003;57:628–33.
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R3.8: Rain: again this definition seems to be used loosely and in the discussion referred to as frequent heavy rain. Is this consistent BOM definitions? And the rainfall days were pretty low and clearly will be community location dependent

As the variable 'rain' refers to instances of daily rainfall of 25mm or greater, we have renamed the variable 'heavy rain', to improve clarity (replacements throughout the methods and results are highlighted with track changes in the resubmitted manuscript). The phrase 'frequent' is used in conjunction with heavy rain throughout the discussion (8th para) to differentiate communities categorised as 'high' for this variable (i.e. those communities experiencing more incidences of rain above 25mm / day from those not). To the question 'is this [the definition of heavy rain] consistent with BOM definitions?', we clearly state the data source as BoM (just above this point) and BoM commonly report the number of days in a calendar month or year with at least 1, 10 or 25 mm of precipitation (the latter clearly being our selected measure). It is difficult to respond to the reviewers' contention that 'the rainfall days are pretty low' – given that these data are sourced from the Bureau of Meteorology, we believe that the number of days of rain of 25mm or greater are accurately presented. Perhaps the comment stems from a previous lack of clarity regarding the definition of the measure

'rain', that is, as heavy rain days as opposed to days of any rain. In renaming the variable 'heavy rain' we have sought to reduce any such lack of clarity.

That frequency of heavy rainfall is community location dependent is entirely the point. Quantifying the extent to which the variation by community associates with smoking rates is part of the aim of this study.

R3.9: Communities with a population of 111: Should these be excluded? There will be a lot of variability in these samples. Say 60% of the population is over 15, and 1 family leaves town, the impact on the number of smokers could be large, the prevalence of smoking may move up and down a lot.

Given that this study is on remote, predominantly Indigenous communities which are very often small, we are hesitant to incur the kind of sample loss that would result from an arbitrary minimum population criterion over and above the one implicit within the exclusion of ILOC's containing multiple small communities.

R3.10: Text: Results: are hard to read with much use of 'null'. A broad section of the health sector may read such a paper and it would be useful if the text was easy to understand.

We do hope that our paper might be read broadly within the health sector; we believe it conveys important information regarding inter-community variations in smoking rates and its community-level correlates. Regarding the readability of the Results text, we feel that non-statistically significant results can be as important as statistically significant ones and thus we ensure that these results are fully reported. Nonetheless, the body of the results section text has been edited to improve readability and reduce the use of the term 'null'. Multiple changes within the results section are highlighted using track changes in the resubmitted manuscript.

R3.11: Discussion: too long for what has been found. There is no discussion about the role of employment – many workplaces are smoke free now in remote areas, so people will smoke less while at work. Most houses are smoke free and depending on the community possibly air conditioned.

We disagree with the reviewer regarding the length of the discussion but would note that adding more discussion regarding the role of employment would make it longer; the issue is balancing the focus areas against their relative importance. Given that the employment rate within this sample is so low (median proportion in the labour force 34.6%, IQR 27.6-52.0%), the potential influence of smoke-free workplaces on behaviour referred to by the reviewer is relatively more limited than might be the case in other contexts (i.e. where employment rates are higher). Similarly, evidence (co-authored by reviewer 1, Thomas) exists that indicates that NT legislation regarding smoke-free workplaces around the time of data collection was lacking, being characterised as 'far behind other States and this was perceived as undermining other tobacco control efforts'²⁴. Further, challenges to enforcement of smoke-free workplaces included 'few government staff employed to enforce the legislation and provide education', 'reticence by the community to change the status quo' and 'generally few non-smoking role models in leadership positions who can take charge in this area'²⁴. The same article highlights that the uptake of smoke-free housing practices in remote Indigenous communities is not nearly as common as the reviewer's comment suggests, with 'some' residents displaying no smoking signs about their residences, and 'several' instituting smoke free practices. Similarly, the combination of high smoking rates and limited contact with a broader community of non-smokers (as a result of remoteness) leads to a high degree of social normalisation of smoking²⁵. We believe that these

citations make it clear progress towards meaningful smoke-free practices in homes and workplaces in the remote NT is unlikely to be rapid due to the aforementioned social normalisation, legislative and enforcement issues. Consequently, and given the low employment rate we observe in this study, we consider it unlikely that the status of home- or workplace-related smoking policies and practices would have a substantial impact of smoking rates within the sampled communities.

24. Johnston V, Thomas DP. What works in Indigenous tobacco control? The perceptions of remote Indigenous community members and health staff. *Health Promotion Journal of Australia* 2010;21:45-50.

25. Passey ME, Gale JT, Sanson-Fisher RW. "It's almost expected": rural Australian Aboriginal women's reflections on smoking initiation and maintenance: a qualitative study. *BMC Women's Health* 2011;11(1):1-12. doi: 10.1186/1472-6874-11-55

VERSION 2 – REVIEW

REVIEWER	David Thomas Menzies School of Health Research One of the authors (RB) was previously employed at Menzies, and we did collaborate on research.
REVIEW RETURNED	24-Jan-2019

GENERAL COMMENTS	<p>The authors confirm previous research showing neighbourhood level variation in the reported smoking prevalence in remote Indigenous communities based on (albeit very incomplete) clinic records.</p> <p>R1.0. However, the causes of this variation which they have tested remain very problematic, and have the potential for over-stating the importance of possibly minor factors, due to the failure to include or control for accepted causes. Some of the accepted factors not included are likely to be correlated with the 9 factors included. They include income, education, employment and overcrowding, but not the most important local factor which should have been known to the authors (but would not be known to a general reader of <i>BMJ Open</i>): whether the community was in Central or Northern Australia, with their very different contact histories with tobacco and so quite different smoking prevalences. This is now only mentioned in passing in the limitations, rather than being included in the design of the study.</p> <p>R1.1 The poor recording of smoking status will only bias results to the null (as they state) if this variation is random, rather than reflecting attention to smoking (which they state it is).</p> <p>R1.4. While they have provided some justification for their selection of the two climate measures, they do not justify the selection of the other measures used, or their exclusion of other measures. It remains problematic that they include no measure of tobacco control activity, as this is surely one of the most amenable factors to prompt intervention.</p> <p>R1.13. Their reference to the under-resourcing of Indigenous tobacco control is very old and no longer holds. The Australian government recently allocated \$184m for another 4 years of its Tackling Indigenous Smoking program, which is mainly spent of</p>
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	<p>regional teams, and continues funding commenced 10 years before.</p> <p>Minor R1.3. (23%) based on the latest AIHW Burden of Disease study. In passing, they should also base their comparisons of Indigenous and non-Indigenous daily smoking using the ABS's most recent National Aboriginal and Torres Strait Islander Social survey (2014-15).</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer’s comment 1. The central point of contention here appears to be that we do not include in our analysis ‘the most important local factor which should have been known to the authors (but would not be known to a general reader of BMJ Open): whether the community was in Central or Northern Australia, with their very different contact histories with tobacco and so quite different smoking prevalences. This is now only mentioned in passing in the limitations, rather than being included in the design of the study.’

Related to this point is the reviewer’s original comment R1.15 ‘Nowhere do the authors adequately discuss historical reasons related to the introduction of tobacco for the difference between the rainy humid top end and hotter drier centre. This is well known in the NT, and recognised as the cause of dramatically smoking prevalence, esp among women. Smoked tobacco was introduced in the top end hundreds of years ago by Macassans and smoked by men and women. In the Centre, native tobaccos were largely chewed, with commercial tobacco introduced much later as part of colonialism, and mainly smoked by men. The association with population size may also in part reflect this history, as desert communities are usually smaller than those in top end.’

Rebuttal: As can be seen from these two comments (Reviewer’s Comment 1 and the original comment R1.15), the reviewer changes the objection from ‘Nowhere do the authors adequately discuss historical reasons (for different smoking prevalence by region) ...’ to ‘This (different prevalence by region) is now only mentioned in passing in the limitations, rather than being included in the design of the study.’. Despite the fact that we had cited literature which discussed the history of Indigenous smoking extensively, we did in our revised submission attend to the objection of the original comment R1.15 – to discuss the potential influence of the history of tobacco usage on regional variations in smoking prevalence. We have provided a brief comment flagging this issue to the reader. We maintain our original citation to allow readers to more fully explore this issue at their leisure. We did not perceive in the reviewer’s original comment (‘Nowhere do the authors adequately discuss ...’) a request or requirement to bring a dichotomous geographic variable (Central vs Northern location) to the forefront of our analysis.

Reviewer’s Comment 1 misrepresents our commentary on different prevalences by region as new: ‘now only mentioned in passing in the limitations’. A review of our originally submitted manuscript (page 18, from line 5 onwards) would indicate that we discuss that we discuss the possibility of a geographic influence on climate associations with smoking rates in our original submission.

Reviewer's Comment 2: The poor recording of smoking status will only bias results to the null (as they state) if this variation is random, rather than reflecting attention to smoking (which they state it is).

Rebuttal: Reviewer's Comment 2 misrepresents our text regarding deficiencies in smoking prevalence surveillance. Nowhere do we state definitively that data loss reflects attention (or a lack thereof) to smoking in the clinical records. Our text (page 48, line 8-13) indicates the possibility that this loss indicates deficient record keeping. Similarly, in Strengths and Limitations (page 6 line 21-23) we indicate that we believe that data loss is likely random rather than systematic.

Reviewer's Comment 3. While they have provided some justification for their selection of the two climate measures, they do not justify the selection of the other measures used, or their exclusion of other measures. It remains problematic that they include no measure of tobacco control activity, as this is surely one of the most amenable factors to prompt intervention.

This comment is a continuation of a first-round comment that refers to page 4 line 36-45 of our original manuscript, wherein we identify a range of socioeconomic, sociocultural and stress related factors that have previously been associated with Indigenous smoking. We include in our analysis several of these factors (income, education, unemployment, and overcrowding). The exclusion of other measures from those highlighted is a function of the fact that our analysis is based on an audit of clinical records for smoking rates and census data for socioeconomic measures – neither of which source captures issues like boredom, incarceration, removal from family, exposure to racism, dislocation from the land, loss of traditional diet, etc. Capturing these broader sociocultural and stress related factors is outside the remit of our manuscript.

Reviewer's Comment 3 also raises a new concern (unidentified in the first round of the review) that 'It remains problematic that that they include no measure of tobacco control activity, as this is surely one of the most amenable factors to prompt intervention'. However, the language used here implies ('it remains problematic') that this issue is continued from earlier in the review, which is not the case. Notwithstanding this, we refer the editors to Figure 1 in our rebuttal of the Reviewer's Comment 4, which indicates that the tobacco control activity which is so clearly 'most amenable to intervention' have been utterly ineffectual in attenuating remote Indigenous smoking across an extended period. Given this lack of effect, we do not consider the lack of a tobacco control measure as problematic for this study.

Reviewer's Comment 4. Their reference to the under-resourcing of Indigenous tobacco control is very old and no longer holds. The Australian government recently allocated \$184m for another 4 years of its Tackling Indigenous Smoking program, which is mainly spent of regional teams, and continues funding commenced 10 years before.

We note that the reviewer appears to concede the main point of our response to his original comment, i.e. that variation in prevalence only matters if effectiveness varies by prevalence. He now focusses on the age of the reference we provide in support of our comment that tobacco control has been consistently under-resourced, despite this being only part of our counterargument to the initial comment.

We are clear in the methods section that our data were collected between 2010 and 2014. Thus, at the time that our data were collected, Power, Grealy and Rintoul's characterisation (made in 2009) is accurate as it pertains to the period leading up to our study. Whether tobacco control is currently under-resourced, or not, is profoundly irrelevant to the question at hand. The recent allocation of \$184 million to the Tackling Indigenous Smoking (TIS) program can have no effect on data collected up to 10 years prior to this allocation. With regard the statement that this current round of funding 'continues funding commenced 10 years before', we provide, below, a graphic of smoking prevalence between 1994 and 2014 in the indigenous population by remoteness of residence (i.e. before and during the period of data collection for our study). These data are sourced from the Australian Bureau of Statistics.

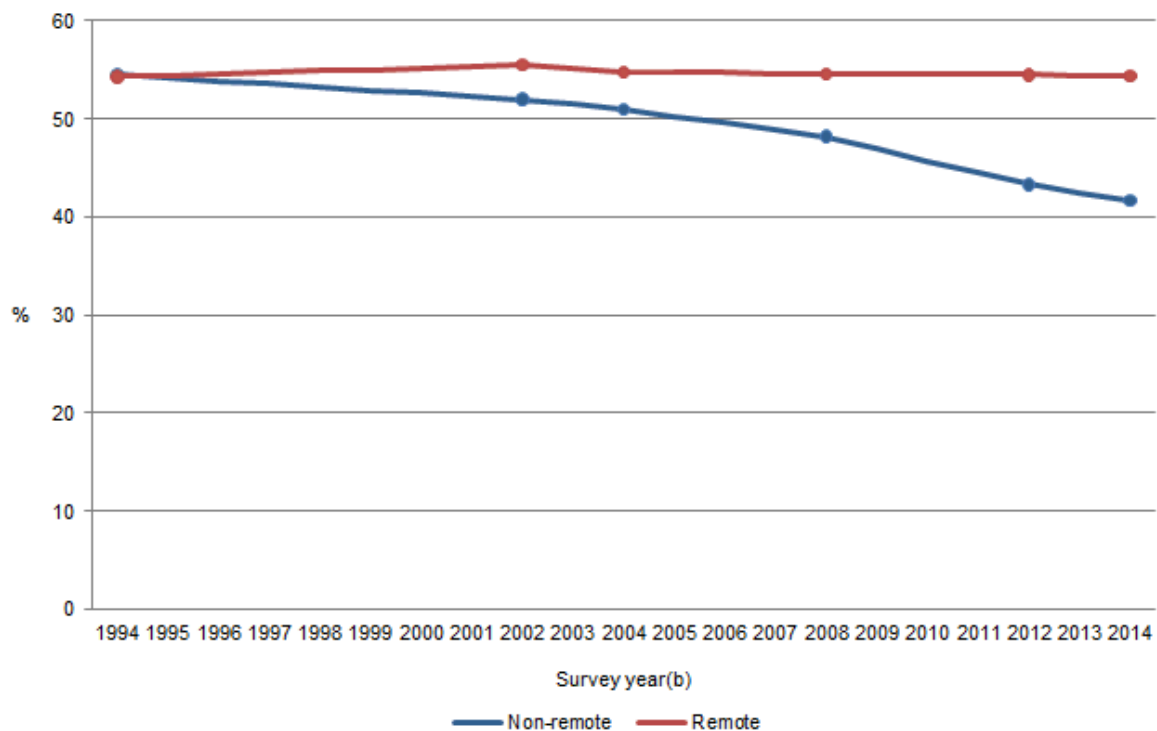


Figure 1. Indigenous smoking prevalence, by remoteness of residence, between 1994 and 2014. Source: 4737.0 - Aboriginal and Torres Strait Islander Peoples: Smoking Trends, Australia, 1994 to 2014-15, <http://www.abs.gov.au/ausstats/abs@.nsf/Lookup/by%20Subject/4737.0~1994%20to%202014-15~Main%20Features~Smoking%20Prevalence~10>

Given our analysis deals exclusively with remote communities, it appears to us that the reviewer's point (which we dispute) is moot – if substantial funding has been applied to the problem of remote indigenous smoking, it has been ineffectual over the time period relevant to our work.

Reviewer's Comment 5. They should update their comment about smoking's contribution to the health gap (23%) based on the latest AIHW Burden of Disease study.

Rebuttal: The reviewer's initial comment on this issue was (in reference to page 4, line 24) 'Please update to latest AIHW estimate: 23%.'. We interpreted this to be a comment regarding the 'age-standardised prevalence rates of 42% and 16% respectively' and were thus unable to determine the provenance of the statistic the reviewer provided. Given the additional information that the reviewer provides in his subsequent comment, we can now see the point he was trying to make initially. We are prepared to revise this section of the manuscript to reflect the reviewer's comment, although, as the reviewer states, this is a minor point.

Reviewer's Comment 5. (cont) In passing, they should also base their comparisons of Indigenous and non-Indigenous daily smoking using the ABS's most recent National Aboriginal and Torres Strait Islander Social survey (2014-15).

Rebuttal: On this point, we disagree with the reviewer. NATSISS 2014-2015 reports a lower daily smoking rate (39% vs 42%) than the 2018 AIHW citation that we use, the source data for which is the contemporaneous National Health Survey (2014-15). The reviewer is on record stating that (referring to the National Aboriginal and Torres Strait Islander health and Social Surveys):

'As sampling is based on the geographic distribution of the Aboriginal and Torres Strait Islander population, rather than the whole Australian population, they include a more adequate sample from remote and very remote areas.'^[1]

At this point of the manuscript, we are contextualising Australian values for the difference between overall Indigenous smoking behaviour and the non-Indigenous population by comparison with the same variation in other nations (New Zealand, Canada, the United States of America). It is more important here to report statistics from within the same source than it is to bias the sample towards the remote and very remote portion of the indigenous population. However, as the reviewer himself indicates, this is a minor point.

[1]. Thomas, D.P. & Scollo, M. (2018) Should a smoking question be added to the Australian 2021 census? Australian and New Zealand Journal of Public Health, 42 (3) 225-226.