PEER REVIEW HISTORY

This paper was submitted to a another journal from BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

(This paper received three reviews from its previous journal but only two reviewers agreed to published their review.)

ARTICLE DETAILS

TITLE (PROVISIONAL)	Why has adolescent smoking declined dramatically? Trend analysis using repeat cross-sectional data from New Zealand 2002-2015.
AUTHORS	Ball, Jude; Sim, Dalice; Edwards, Richard

VERSION 1 – REVIEW

REVIEWER	Benjamin Kuntz Robert Koch Institute, Department of Epidemiology and Health Monitoring, Berlin, Germany
REVIEW RETURNED	12-Nov-2017

GENERAL COMMENTS	Comments to the paper of Ball et al. 2017 (BMJ Open) The present paper is based on the question why adolescent smoking rates in New Zealand (as in many other high income countries) declined dramatically since the late 1990s. The empirical work uses repeat cross-sectional data (2002-2015) from a nationally representative survey among secondary school students aged 14-15 years. First, Ball et al. analysed time trends in adolescent smoking prevalence and exposure to four common risk factors (1. parental smoking, 2. best friend smoking, 3. older sibling(s) smoking, 4. exposure to smoking in the home), second, whether changes in exposure to these known risk factors contribute to the decline in adolescent smoking, and third, whether relationships between these risk factors and adolescent smoking have changed over time. The paper presents original and interesting findings; the whole analysis seems to be sound and elaborated. Points for minor revision: p. 4, l. 7-17: "In New Zealand (NZ), for example, regular smoking (defined as at least monthly) among 14-15 year olds declined from a peak of 29% in 1999 to 6% in 2014, with decreases across all main ethnic and socioeconomic groups, and a convergence between boys and girls over the period. However, as in other countries, ethnic disparities remain pronounced with Māori (indigenous) smoking prevalence in this age group at 13.2% compared 4.2% among non-Māori in 2014."

	Here, it would be interesting to get some information, if not only ethnic disparities, but also socioeconomic disparities in adolescent smoking remained pronounced. In the discussion section, the authors explicate: “We used school decile as proxy for socioeconomic status (SES), since more direct measures were unavailable” (p. 16). Regrettably, the authors do not report prevalences of adolescent smoking and the analysed risk factors stratified by school deciles which are described in the methods section: “We grouped school decile into low (deciles 1-3: most deprived), medium (4-7), and high (8-10: least deprived)” (p. 8). p. 8, l. 13-16: “The outcome variable, ‘regular smoking’ (Y/N) was defined as smoking at least monthly, based on the question ‘How often do you smoke now?’” Could you please add the original response categories as background information? p. 10, l. 22-27: “As shown in Figures 1b and 1c, smoking among older siblings declined slightly more, at an average rate of 0.7% per annum, but only ‘best friend smokes’ declined at a similar rate to regular smoking in 14-15 year olds (1.5% per annum).” The information comes from Figures 1c and 1d; please correct the reference in the manuscript. Other points: How did the authors deal with the presumably increasing fraction of adolescents who don’t have older siblings due to demographic change (more one-child families)? Is the prevalence of older siblings smoking referred to adolescents with older siblings or to adolescents overall – irrespective of they have older siblings or not?

REVIEWER	Jennifer O'Loughlin University of Montreal Canada
REVIEW RETURNED	27-Nov-2017

GENERAL COMMENTS	This study poses two interesting questions: Do declines in risk factor prevalence underpin declines in smoking in adolescents, and has the association between specific risk factors and smoking onset changed over time? While these questions are interesting and important: (i) I don’t think the first question can be answered with the current dataset (i.e., making inferences about cause with cross-sectional data is problematic) (ii) some of the analytic choices made by the authors need further reflection (iii) the authors argue that because the declines in some risk factors align with the declines in smoking, then these risk factors must be the cause. This ecologic argumentation is flawed (i.e., the ecologic fallacy) It is not a given that because the curves for declines resemble one to the other, that these risk factors are the cause of the decline in adolescent smoking. (iv) the authors appear to be looking for a single or a most important cause of the decline in smoking in adolescents, but a more likely explanation is that there are multiple causes which interact one with the other (v) As presented, the authors have not interpreted the data correctly in my view. In particular, there is no evidence supporting the

	statement “Our findings do not support the idea that reduced smoking prevalence among parents was a significant driver of adolescent smoking decline”. Parental smoking is intimately linked with smoking in the home and parsing out which risk factor has more or less influence is likely not possible with the given dataset. Introduction 1. Has the decline in smoking in Maori been equivalent to the decline in non-Maori? Are the rates of decline similar? 2. Have other tobacco products replaced cigarettes? 3. What is a low decile student? 4. Could the low response influence prevalence or the measures of association? 5. What is meant by “the methods are reasonably consistent” over time? 6. What was the mean age of participants before and after 2010? Methods 7. The question “During the past 7 days, on how many days have people smoked around you in your home?” may have resulted in some misclassification due to “around you”. If people smoked in the household yet not “around the participant”, the student may have been exposed to SHS but would have responded “no.” 8. How did the exclusion criteria affect the number of participants? 9. Why specifically was weighted linear regression used in the analysis of change in risk factor prevalence over time 10. Why not simply test an interaction term (Maori vs others) in a pooled model rather than stratifying? 11. Inclusion of all risk factors for smoking in the same model may have attenuated the estimates since several were likely on the causal pathway between other risk factors and regular smoking. For example, exposure to smoking in the home was no doubt on the causal pathway between parents smoking and regular smoking. Is inclusion of possible intermediate variables in the modeling problematic? The authors may need to think more carefully about how a reduction in mothers smoking for example, might influence the other risk factors, and how this might influence regular smoking in adolescents. Exploration of these issues in directed acyclic graphs for example might clarify thinking. 12. Is the decline in regular smoking because fewer adolescents initiated and/or because fewer adolescents attained the regular smoking status? 13. While the %s of no exposure to SHS and of any exposure did not change from 2003 to 2015, the % with daily exposure declined from 21.6% to 12.9%. Thus it seems that parents and other smokers in the home reduced smoking in the home from daily to less than daily. What might be the underpinning then of this decline in less than daily exposure to SHS in the home? 14. Why separate mothers and fathers? Is there a specific hypothesis regarding parent-specific smoking? DO the authors think a decline in mother smoking might have more impact than a decline in father smoking? 15. The authors suggest that the decline in parental smoking is modest compared to the decline in regular smoking among adolescents. Why compare these two levels of decline? In and of itself the decline in parental smoking (7% over the study period) is notable and could very well have had an important influence on adolescent smoking, especially in conjunction with the other declines observed. It is likely that the decline in adolescent smoking cannot be attributed to a single cause……but is the net result of multiple influences.

	16. As indicated above, the finding that parental smoking had a weak association with the outcome could relate to inclusion of intermediate variables in the modeling. Alternatively, perhaps the influence of parental smoking declines with age in adolescents. 17. Should these analyses be corrected for multiple testing? 18. The analyses for Maori seem to provide little additional information, and could probably be summarized in a single sentence 19. Are there analyses that can test ORs for trends over time in friends smoking and exposed to smoke in the home (these figures are presented in an appendix). Discussion 20. While interesting, the discussion strays quite far from the data.

REVIEWER	Helen Sweeting MRC/CSO Social & Public Health Sciences Unit, University of Glasgow, UK
REVIEW RETURNED	14-Dec-2017

GENERAL COMMENTS	Why has adolescent smoking declined dramatically? An investigation of changing exposure to risk factors using analysis of repeat cross-sectional data from new Zealand 2002-2015 Thank you for asking me to review this clearly written paper, based on a large New Zealand dataset with information on adolescent smoking, obtained via school-based surveys 2002-15. I have a number of comments (some very small), plus some questions around the analyses, outlined below. ABSTRACT Probably worth saying here that surveys were annual. I know words are limited, but separate analyses for Maori pop up as a surprise – could these be introduced in objectives (just as separate due to higher rates)? INTRODUCTION P4, main para – might be worth looking at this paper re potential impact of policies on adolescent smoking trends – Green et al Socioeconomic position and early adolescent smoking development: evidence from the British Youth Panel Survey (1994-2008). Tobacco Control, 25(2), pp. 203-210. (doi:10.1136/tobaccocontrol-2014-051630). P5 line 17 – a tiny thing, but suggest school 'ethos' rather than climate – which I first mis-read as climate (change) generally – ie the weather. P5-6 – re explaining the impact of changes in LEVELS of vs changes in strength of ASSOCIATION with potential explanatory factors, a paper / way of looking at it that I find useful is this one, even though the topics are different - Rutter M, Caspi A, Moffitt T: Using sex differences in psychopathology to study causal mechanisms: unifying issues and research strategies. Journal of Child Psychology and Psychiatry 2003, 44:1092-1115. It might also be worth clearly stating at the start that the analysis is really about 'explaining' the changes in stats terms rather than being totally sure about causality.

	P6 second para – is there any research to suggest associations might differ for Maori adolescents (despite differences in overall levels)? Again – just to set this separate analysis up. P6 line19 – typo 'New NZ'. And then spelt out as New Zealand on line 31 instead of acronym. METHODS P6 line 29 – maybe worth clarifying that this is ASH NZ (as there are lots of ASHs). P7 first line – 'low decile' isn't explained until later – so unclear if this refers to socioeconomic status, academic achievement or something else. P7 – Table 1 – is there any explanation for the rather lower response rate in 2015 (maybe as a footnote)? P7 lines 34-40 – could the earlier fieldwork and so slightly younger ages (how much younger?) from 2011 have had any impact on trends? P8 – family smoking status and past week exposure in the home – is the inclusion of each of these potential double-counting? Most home exposure will be due to parent / sibling smoking, so it's not really surprising that home exposure mops up the effects of family member smoking in the adjusted analyses. I also wondered whether two separate variables representing daily and any home exposure were also double counting, since all those with 'yes' to daily' will also be 'yes' to ever. It would be possible to create a 3-category variable: 0; 1-6 days a week; and 7 days a week. (Perhaps this was done, since table 2 includes 0 days, but then 1-7 and 7 days, which seems confusing / impossible.) P8 line 47 – is the school decile variable based on student self-report (ie via the ASH surveys and so based on those who responded – then aggregated), or is it based on some sort of routinely collected data on the whole school (eg proportion receiving free school meals)? P9 top para – were there any differences in cross-sectional associations carried out on those with complete data (which formed the basis of the analyses) vs those with some missing? P9 middle para – I assume the weighted linear regression method is correct to quantify mean changes (ie don't have this stats expertise). P9 3rd para – analyses were conducted for all, then for Maori only, and generally found few differences. But this would be including Maori in BOTH samples, so another form of double-counting. If looking for differences, wouldn't it make more sense to look for interaction effects for Maori vs all other ethnic groups? P9 3rd para – I'm wondering whether the multivariable analyses to assess independent effects were actually necessary? In a way, they're answering a different question (about relative importance of different factors). Are the bivariate associations enough to answer whether the impact of any of the potential explanatory factors

	changed over time? And would it have been possible to add the 2003 and 2015 datasets in order to look for an interaction between each factor and year to see if there were significant changes over time? (At the moment we need to look at each set of 95% Cis.) RESULTS P10 first para – overall smoking dropped by 1.2% per year – what did Maori adolescent smoking levels drop by? P10 middle para – This is inconsistent in what is reported for overall and what for Maori – would it be possible to add another table so we can see the numerical results (rather than as figures) for both groups? P10 middle para – And typo - I think the second ref to figures should be C and D (not B and C) P13 top para – final sentence says Maori results aren't shown on Table 2, but they are. There is an un-labelled figure – perhaps that is Fig 2, which only seems to be one group. DISCUSSION – Might be worth going back to the Intro section p4 re policies – just to say that these analyses didn't capture any of these – and to make the point that there might be other reasons for the drops in youth smoking which, as the authors point out, results in a rather circular argument that it could be friends causing drops in smoking among each other. SUPPLEMENTARY TABLE It would be useful to have the school decile variable described here (eg as footnote) as well as in the methods. It would also be useful to have clarification of the source of the population stats – I assume these are high level publically available data?

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 : Benjamin Kuntz

	Comment	Response
1.	The paper presents original and interesting findings; the whole analysis seems to be sound and elaborated.	Thank you.
2.	p. 4, l. 7-17: "In New Zealand (NZ), for example, regular smoking (defined as at least monthly) among 14-15 year olds declined from a peak of 29% in 1999 to 6% in 2014, with decreases across all main ethnic and socioeconomic groups, and a convergence between boys and girls over the period. However, as in other countries, ethnic disparities remain pronounced with Māori (indigenous) smoking prevalence in this age group at 13.2% compared 4.2% among non-Māori in 2014." Here, it would be interesting to get some information, if not only ethnic disparities, but also socioeconomic	Thank you for this query. Socioeconomic disparities are also pronounced, and this has been acknowledged in the introduction (para 2), which now reads: "However, as in other countries, ethnic and socioeconomic disparities remain pronounced, for example Māori…" We have also added a figure (Fig 2) to the results section, which shows ethnic and socioeconomic patterning in 2015.

	disparities in adolescent smoking remained pronounced.	
3.	In the discussion section, the authors explicate: "We used school decile as proxy for socioeconomic status (SES), since more direct measures were unavailable" (p. 16). Regrettably, the authors do not report prevalences of adolescent smoking and the analysed risk factors stratified by school deciles which are described in the methods section: "We grouped school decile into low (deciles 1-3: most deprived), medium (4-7), and high (8-10: least deprived)" (p. 8).	Unfortunately the word count and table/figure limit precludes detailed presentation of findings by subgroup in the main paper (as this was not the primary focus of our paper), however we have added stratified findings to the supplementary material. These show smoking prevalence by ethnicity, school decile and gender (Figures S1 to S3) and exposure to risk factors by ethnicity and school decile (Figures S4 to S10).
4.	p. 8, l. 13-16: "The outcome variable, 'regular smoking' (Y/N) was defined as smoking at least monthly, based on the question 'How often do you smoke now?'" Could you please add the original response categories as background information?	The response categories have been added to first paragraph the 'Variables' section of the methods.
5.	p. 10, l. 22-27: "As shown in Figures 1b and 1c, smoking among older siblings declined slightly more, at an average rate of 0.7% per annum, but only 'best friend smokes' declined at a similar rate to regular smoking in 14-15 year olds (1.5% per annum)." The information comes from Figures 1c and 1d; please correct the reference in the manuscript.	Thank you for picking up this error. It has been corrected.
6.	How did the authors deal with the presumably increasing fraction of adolescents who don't have older siblings due to demographic change (more one-child families)? Is the prevalence of older siblings smoking referred to adolescents with older siblings or to adolescents overall – irrespective of they have older siblings or not?	This is an interesting point, but not directly relevant to our analyses. We were interested in whether exposure to older sibling smoking and associations with adolescent smoking had changed over time. Identifying the *causes* of changing exposure to older sibling smoking (whether due to less smoking among older siblings, or due to fewer 14-15 year olds having an older sibling at all) was not the aim of our analysis.

Reviewer 2: Jennifer O'Loughlin

7.	This study poses two interesting questions: Do declines in risk factor prevalence underpin declines in smoking in adolescents, and has the association between specific risk factors and smoking onset changed over time? While these questions are interesting and important: (i) I don't think the first question can be answered with the current dataset (i.e., making inferences about cause with cross-sectional data is problematic) (ii) some of the analytic choices	Thank you for these comments. We agree that the current dataset and study design does not allow us to make causal inferences, and the analysis in this paper did not seek to do so. Rather, our study builds on a large body of previous research (using a range of study designs) which has established that parental, sibling and peer smoking and exposure to second hand smoke are strong predictors of adolescent smoking, with a causal relationship highly likely in the case of parental and sibling smoking according to the Surgeon General's

	made by the authors need further reflection	report referenced in the introduction (p5, line 29 and following). Even in the absence of certainty regarding causality, analysis of changes in exposure to risk factors over time (while individual-level associations are shown to remain constant or increase) can *account for* (in statistical terms) changes over time in outcomes. The analyses presented in this paper aim to do just that. We have amended the abstract and methods section to provide greater clarity about our approach, for example highlighting that the focus of the study is on *known predictors,* and making the distinction between accounting for change over time in statistical terms, and assertion of causal explanation. We have also amended the language used throughout to ensure that causality is not inappropriately applied to the methods and findings of our study, e.g. 'predictor' has been changed to 'risk factor.' We had already noted in the discussion of study limitations (p 16 in the original MS) that "it is not possible to draw causal inferences based on cross-sectional data." We have amended the 'strengths and limitations' section at the beginning of the paper (p3) so that this is further highlighted: "Repeat cross sectional data does not enable causal inferences to be drawn, however our study builds on existing knowledge about the predictors of adolescent smoking initiation and, in the absence of certainty about causality, accounts for change over time in statistical terms."
9.	(iii) the authors argue that because the declines in some risk factors align with the declines in smoking, then these risk factors must be the cause. This ecologic argumentation is flawed (i.e.,	Thank you for this comment. This interpretation of our argument is not correct, suggesting that the clarity of our explanation needs improvement.

	the ecologic fallacy) It is not a given that because the curves for declines resemble one to the other, that these risk factors are the cause of the decline in adolescent smoking.	Our study is based on individual level data and associations (not ecological data), and this has been clarified in the abstract and 'strengths and limitation' section (p2-3). As explained in the introduction (p6, line 6-12 in the original MS): "Assuming that an observed individual-level relationship between a risk factor and adolescent smoking is causal, then declining exposure to that risk factor over time at the population level (while the strength of association is maintained or increased at the individual level) must contribute to a population decline in adolescent smoking." As noted above, even in the absence of certainty regarding causality, analysis of changes in exposure to risk factors over time (while individual-level associations are shown to remain constant or increase) can *explain* (in statistical terms) changes over time in outcomes.
10.	(iv) the authors appear to be looking for a single or a most important cause of the decline in smoking in adolescents, but a more likely explanation is that there are multiple causes which interact one with the other	We agree that there are likely to be multiple causes at various levels, and respectfully note that a wide range of possible contributing factors were mentioned in the introduction (p4-5) (e.g. tobacco control measures, changes in technology, changes in parenting practices, changes in school environment, changes to the economy and labour market etc) and discussion (e.g. virtuous cycle of peer influence; policy response to rise in teen smoking in the 1990s; denormalisation). Our original manuscript concludes: "It is likely that factors other than those in our model are at play, with changes in the social meaning of smoking, the policy context and broader socio-cultural changes all potential contributors. Further research is needed to identify other contributing factors and determine their relative importance. " In the analyses presented, our aim is to explore one particular hypothesis - that changing exposure to key established risk factors accounts for the decline

		observed. This is explained in the introduction which states: "But before exploring these novel explanations for smoking decline, it is important to determine the extent to which the observed trends can be explained by changing exposure to known predictors of smoking initiation." (p5 line 22-26 in the original MS).
11.	(v) As presented, the authors have not interpreted the data correctly in my view. In particular, there is no evidence supporting the statement "Our findings do not support the idea that reduced smoking prevalence among parents was a significant driver of adolescent smoking decline". Parental smoking is intimately linked with smoking in the home and parsing out which risk factor has more or less influence is likely not possible with the given dataset.	Thank you for this feedback. We agree that parental smoking is strongly associated with smoking in the home, and agree that some of our comments around parental smoking were not well worded. In the unadjusted model both SHS exposure in the home and parental smoking were strongly associated with adolescent smoking. Parental smoking remained significantly associated in an adjusted model that excluded smoking in the home (see comment 22 below for an explanation of our amended modelling approach). However, in the fully adjusted model, parental smoking became a weak/nonsignificant risk factor whereas the relationship between exposure to smoking in the home (and other risk factors) and adolescent smoking remained highly significant. This, together with the strong association between parental smoking and SHS exposure in the home, suggests that exposure to smoking in the home mediates or confounds (or both) the relationship between parental smoking and adolescent smoking. It is not possible to determine whether confounding or mediation is the dominant phenomenon. However, we can say that if declines in parental smoking have influenced the decline in adolescent smoking it is probably *via* declining exposure to other risk factors in the model, in particular exposure to smoking in the home. We have amended the discussion to reflect this line of argument. We have also amended the abstract and discussion section to remove the statements about parental smoking and adolescent smoking that were poorly phrased. For example we have deleted the statement "changes in parental smoking do not account for smoking decline among adolescents" from the abstract, and also "Our findings do not support the idea that reduced smoking prevalence among parents was a significant driver of adolescent smoking decline"

		from the discussion.
12.	Has the decline in smoking in Maori been equivalent to the decline in non-Maori? Are the rates of decline similar?	In response to comment 29, and to sharpen the focus of the main paper, we have decided to focus on adolescents as a whole, and move ethnic-specific findings to the supplementary material. We have added a series of tables to the supplementary material show smoking prevalence and exposure to risk factors by ethnicity and school decile. As Supplementary Figure S1 illustrates, the rate of decline among Māori was higher than other ethnic groups.
13.	Have other tobacco products replaced cigarettes?	This is very unlikely to be the case. Smokeless tobacco products are illegal in New Zealand (as was retail sale of nicotine-containing e-cigarettes in 2015) and use of other forms of smoked tobacco (e.g. bidis, cigarillos, hooka) is very rare in this country.
14.	What is a low decile student?	Thank you for noting that the explanation of 'school decile' needs to come earlier in the manuscript. This error has been corrected.
15.	Could the low response influence prevalence or the measures of association?	Thank you for this query. The supplementary table provides a comparison between the sample and the Year 10 population, and shows that the sample closely resembles the Year 10 population in all years, and therefore response bias was unlikely to influence prevalence estimates or measures of association. Māori and low decile students were modestly under-represented in the sample and therefore adolescent smoking in the population is likely to be slightly underestimated (since smoking rates are higher in these under-represented groups). However, because the main focus of our paper is on trends over time, and because the under-representation of Māori and low decile students is consistent over time, it is unlikely to affect our key findings.
16.	What is meant by "the methods are	Thank you for this query. The MS has been

	reasonably consistent" over time?	amended to be more specific: "Excluding the timing of fieldwork and changes to non-core questions, there has been consistency in survey instruments, administration and data management across included years."
17.	What was the mean age of participants before and after 2010?	Unfortunately we cannot calculate mean age as the survey did not include date of birth (only age at the time of the survey). We have amended the manuscript (p7) to clarify that students were 2-3 months younger, on average, as a result of change in fieldwork date.
18.	The question "During the past 7 days, on how many days have people smoked around you in your home?" may have resulted in some misclassification due to "around you". If people smoked in the household yet not "around the participant", the student may have been exposed to SHS but would have responded "no."	Yes, we acknowledge this is possible, though the focus of our analysis was on change over time and one would expect such misclassification to be reasonably consistent from year to year and therefore to have little impact on trends. We acknowledged the limitations of self-report questionnaire data (in general terms) in the limitations section of the original manuscript, and have amended this to give misinterpretation of questions as a specific example: "The study was based on self-report questionnaire data, with its inherent limitations (e.g. potential for social desirability bias, misinterpretation of questions resulting in misclassification)" (p17).
19.	How did the exclusion criteria affect the number of participants?	Thank you for this query. We have added a column to Table 1 showing the number of valid responses (i.e. with complete data for age (14 or 15), sex, ethnicity and smoking status) received each year, and the number of responses included in our study after applying exclusion criteria. This indicates that 96% of valid responses were included in our study.
20.	Why specifically was weighted linear regression used in the analysis of change in risk factor prevalence over time	Regression and ANOVA assume homoscedasticity (i.e. error variance and width of the confidence intervals is the same for all years), but our data does not meet this assumption. Therefore weighted linear regression was used so that the more precise estimates of prevalence are given more weight than

		the less precise estimates in calculation of the best fit regression line.
21.	Why not simply test an interaction term (Maori vs others) in a pooled model rather than stratifying?	We stratified in order to describe and understand, in detail, the profile of Māori students with regard to adolescent smoking and risk factors, since Māori are an important priority group for prevention research and action in NZ. We acknowledge that detailed findings for Māori may be of limited relevance to some international audiences, and as noted above, have moved these analyses to the supplementary material.
22.	Inclusion of all risk factors for smoking in the same model may have attenuated the estimates since several were likely on the causal pathway between other risk factors and regular smoking. For example, exposure to smoking in the home was no doubt on the causal pathway between parents smoking and regular smoking. Is inclusion of possible intermediate variables in the modeling problematic? The authors may need to think more carefully about how a reduction in mothers smoking for example, might influence the other risk factors, and how this might influence regular smoking in adolescents. Exploration of these issues in directed acyclic graphs for example might clarify thinking.	We understand that our risk factors are related to one another and some may be intermediates on the causal pathway (see response to point 11 above). We have amended the MS so that this is explicitly acknowledged (methods p 11 and discussion p17). In order to further clarify this point and provide more detailed data on the adjusted associations between the studied exposures and adolescent smoking we have amended out modelling approach and now present four models: 1. Unadjusted 2. Adjusted for demographic factors only 3. Adjusted for demographic factors, parental, sibling and best friend smoking 4. Adjusted for all variables in model 3 plus exposure to smoking in the home. This approach allows us to present adjusted estimates for parental and sibling smoking including and excluding the potential mediating (or confounding) variable of smoking in the home. It seems implausible that parental smoking or sibling smoking could be on the causal pathway between smoking in the home and adolescent smoking.
23.	Is the decline in regular smoking because fewer adolescents initiated and/or because fewer adolescents attained the regular smoking status?	Thank you for raising this interesting question which, unfortunately, was outside the scope of our study. We have, however, added to the introduction a brief comment on trends in prevalence of “never smoking”, which rose from 32% in 1999 to 78% in 2014. This suggests that fewer adolescents are initiating smoking by 14/15 years of age.

24.	While the %s of no exposure to SHS and of any exposure did not change from 2003 to 2015, the % with daily exposure declined from 21.6% to 12.9%. Thus it seems that parents and other smokers in the home reduced smoking in the home from daily to less than daily. What might be the underpinning then of this decline in less than daily exposure to SHS in the home?	Thank you for this query. To address it, we have added the following sentence to the discussion section (p 16): "Mass media campaigns from 2000 focusing on second hand smoke and a 2004 ban on smoking in pubs and all other indoor workplaces likely contributed to the ongoing denomalisation of smoking (in particular indoor smoking), and may have promoted the observed decline in daily exposure to smoking in the home."
25.	Why separate mothers and fathers? Is there a specific hypothesis regarding parent-specific smoking? DO the authors think a decline in mother smoking might have more impact than a decline in father smoking?	Yes, previous research has suggested maternal smoking is more strongly correlated with adolescent smoking than paternal smoking. We have added a comment about our rationale for looking at maternal and paternal smoking separately in the methods section, with a reference to these previous findings (p8).
26.	The authors suggest that the decline in parental smoking is modest compared to the decline in regular smoking among adolescents. Why compare these two levels of decline? In and of itself the decline in parental smoking (7% over the study period) is notable and could very well have had an important influence on adolescent smoking, especially in conjunction with the other declines observed. It is likely that the decline in adolescent smoking cannot be attributed to a single cause……but is the net result of multiple influences.	We agree, the decline in adolescent smoking is likely to be the net result of multiple influences. Our aim here is to investigate the relative strength of a small number of possible influences by looking at their independent association with adolescent smoking, and changing exposure over time. We have amended the paragraph (p13) that the reviewer is referring to, so as not to compare declines in adolescent smoking with declines in exposure to risk factors.
27.	As indicated above, the finding that parental smoking had a weak association with the outcome could relate to inclusion of intermediate variables in the modeling. Alternatively, perhaps the influence of parental smoking declines with age in adolescents.	We agree that the weak association between parental smoking and adolescent smoking in the fully adjusted model suggests mediation or confounding by other variables included in the model. This is now acknowledged in the revised manuscript (see response to point 11 above). Regarding the second point, we do not have data on smoking and potential risk factors in younger children, so cannot comment on that point. We note that smoking at young ages is likely to be extremely rare, particularly by 2015 – given the very low smoking prevalence by age 14/15 years and the consistent evidence that smoking prevalence increases steadily from around 12 years upwards.

28.	Should these analyses be corrected for multiple testing?	Our study includes only a small number of comparisons, so correction for multiple testing is not necessary.
29.	The analyses for Maori seem to provide little additional information, and could probably be summarized in a single sentence	As previously noted, we have moved the analyses for Māori to the supplementary file.
30.	Are there analyses that can test ORs for trends over time in friends smoking and exposed to smoke in the home (these figures are presented in an appendix).	Thank you for this query. We re-ran the multivariable analysis using the data from years 2003 and 2015 only, and tested whether there was an interaction effect between year and the risk factors in the model. The results are presented in Supplementary Table S2. They show there was a statistically significant difference in fully adjusted odds ratios between 2003 and 2015 for 'best friend smokes' ($p < 0.0001$) and 'past week exposure to smoking in the home' ($p < 0.0001$) but not parental smoking ($p=0.8$ or sibling smoking ($p=0.06$).
31.	While interesting, the discussion strays quite far from the data.	We consider that the question of how and why adolescent smoking has declined is a broad but important question. Our study provides a (modest) contribution to the answer, while the introduction and discussion address the question in broader terms, placing our findings in context.

Reviewer 3: Helen Sweeting

	Abstract	
32.	Probably worth saying here that surveys were annual.	Thank you, this change has been made.
33.	I know words are limited, but separate analyses for Maori pop up as a surprise – could these be introduced in objectives (just as separate due to higher rates)?	In response to comment 29 (by Reviewer 2) we have opted to focus on the findings for adolescents as a whole in the body of the paper and the abstract. Separate analysis for Māori (including the rationale for this) is introduced in the methods section as a supplementary analysis, with results presented in Supplementary Table S3.
	Introduction	
34.	P4, main para – might be worth looking at this paper re potential impact of policies on adolescent smoking trends – Green et al Socioeconomic position and early adolescent smoking development: evidence from the British Youth Panel Survey (1994-2008). Tobacco Control, 25(2), pp. 203-210.	Thank you for bringing this relevant paper to our attention. We have referenced it in the introduction.

	(doi:10.1136/tobaccocontrol-2014-051630).	
35.	P5 line 17 – a tiny thing, but suggest school 'ethos' rather than climate – which I first mis-read as climate (change) generally – ie the weather.	Thank you for this suggestion, which we have followed.
36.	P5-6 – re explaining the impact of changes in LEVELS of vs changes in strength of ASSOCIATION with potential explanatory factors, a paper / way of looking at it that I find useful is this one, even though the topics are different - Rutter M, Caspi A, Moffitt T: Using sex differences in psychopathology to study causal mechanisms: unifying issues and research strategies. Journal of Child Psychology and Psychiatry 2003, 44:1092-1115.	Thank you for this helpful suggestion regarding framing and wording. We also found Rutter et al's paper useful, and appreciate your drawing our attention to it.
37.	It might also be worth clearly stating at the start that the analysis is really about 'explaining' the changes in stats terms rather than being totally sure about causality.	Thank you for this helpful suggestion, which we agree clarifies what we are attempting to do in this paper. We have amended the 'strengths and limitations' section (p3) and the penultimate paragraph of the introduction (p 6) based on this suggestion.
38.	P6 second para – is there any research to suggest associations might differ for Maori adolescents (despite differences in overall levels)? Again – just to set this separate analysis up.	Thank you for this query. No, we are not aware of previous research to suggest associations might differ for Māori adolescents. Our approach of looking at Māori separately was not theory-driven, but was more of a "check" to ensure the drivers of smoking decline in Māori were not markedly different from those for the adolescent population as a whole. As such we believe (on reflection) that, since this is supplementary to our main research question and analysis, separate analysis for Māori is best appended in supplementary material.
39.	P6 line19 – typo 'New NZ'. And then spelt out as New Zealand on line 31 instead of acronym.	Thank you for bringing this error to our attention. This typo has been corrected.
	Methods	
40.	P6 line 29 – maybe worth clarifying that this is ASH NZ (as there are lots of ASHs).	The suggestion has been incorporated in the first paragraph of the methods section.

41.	P7 first line – 'low decile' isn't explained until later – so unclear if this refers to socioeconomic status, academic achievement or something else.	Thank you for noting that the explanation of 'school decile' needs to come earlier in the manuscript. This error has been corrected.
42.	P7 – Table 1 – is there any explanation for the rather lower response rate in 2015 (maybe as a footnote)?	A brief parenthetical explanation has been provided in the second paragraph of the methods section: "School response rates range from 44-67% [42] (with a lower school response rate in 2015 due to limited resources for liaising with schools that year)."
43.	P7 lines 34-40 – could the earlier fieldwork and so slightly younger ages (how much younger?) from 2011 have had any impact on trends?	Respondents were 2-3 months younger, on average from 2011. This has been clarified in the manuscript (p8). (As noted in response to comment 17 above, we do not have DOB data so cannot calculate mean age, unfortunately.) Any impact on trends would be seen as a one-off step change between 2010 and 2011. Such a step change is not evident in any of the figures presented, so it appears any impact on trends was negligible. We can be confident that changes in the age and ethnic structure of the sample over time did not affect measures of association because age and ethnicity were included in multivariate models.
44.	P8 – family smoking status and past week exposure in the home – is the inclusion of each of these potential double-counting? Most home exposure will be due to parent / sibling smoking, so it's not really surprising that home exposure mops up the effects of family member smoking in the adjusted analyses. I also wondered whether two separate variables representing daily and any home exposure were also double counting, since all those with 'yes' to daily' will also be 'yes' to ever. It would be possible to create a 3-category variable: 0; 1-6 days a week; and 7 days a week. (Perhaps this was done, since	Thank you for these comments. We have amended our modelling approach to aid interpretation of which variables are mediating or confounding others. Please see comments 11 and 22 above for further explanation. In our original modelling approach, days exposed to smoking in home (0, 1-2, 3-4, 5-6, 7) was the primary variable used. The exception was the model for "Any exposure" which included a variable coded "Any (1-7 days)" or "None (0 days)" instead of the 'days exposed' variable. This was explained in the notes for Table 2 (p 12 of the original MS).

	table 2 includes 0 days, but then 1-7 and 7 days, which seems confusing / impossible.)	This was not 'double counting' (since only one or other of the two 'smoke in house' variables was used in each model) but rather allowed us to look at the effects of different levels of exposure. However, your feedback makes apparent that our approach was not intuitively easy to understand, and therefore we have taken your suggestion and used the three level variable you have suggested (none, daily and less than daily) throughout in the amended paper.
45.	P8 line 47 – is the school decile variable based on student self-report (ie via the ASH surveys and so based on those who responded – then aggregated), or is it based on some sort of routinely collected data on the whole school (eg proportion receiving free school meals)?	We have added further detail to the explanation of school decile in the methods section (p 10), which we hope clarifies this query. "School decile is calculated by the Ministry of Education for purposes of funding allocation, and is a school-level measure of the socioeconomic position of a school's student community. Details of how school decile is calculated are available from the Ministry of Education.[43]"
46.	P9 top para – were there any differences in cross-sectional associations carried out on those with complete data (which formed the basis of the analyses) vs those with some missing?	Thank you for this query about possible sampling bias. We have added a column to Table 1 which shows that only 4% of valid responses were excluded as a result of our inclusion/exclusion criteria. The characteristics of the included sample are described and compared with the population in Supplementary Table S1. This comparison shows that, aside from a modest but consistent underrepresentation of Māori and students from low decile schools, our sample strongly represented the Year 10 population. Because the underrepresentation is consistent, and the focus of our study is on trends over time, it is unlikely to significantly affect our findings.
47.	P9 middle para – I assume the weighted linear regression method is correct to quantify mean changes (ie don't have this stats expertise).	We used weighted linear regression because our data does not conform to the assumptions of linear regression and ANOVA in that it is heteroscedastic (i.e. the width of the confidence intervals is different from year to year). Therefore weighted linear regression has been used so that the more precise estimates of prevalence are given more weight than the less precise estimates in determining the best fit regression line.

48.	P9 3rd para – analyses were conducted for all, then for Maori only, and generally found few differences. But this would be including Maori in BOTH samples, so another form of double-counting. If looking for differences, wouldn't it make more sense to look for interaction effects for Maori vs all other ethnic groups?	Looking for differences by ethnicity was not the focus of our paper, and this has been clarified by moving sub-group analyses to the appendix (i.e. supplementary material). Because Māori are an important priority group, we opted to carry out stratified analyses (which provide greater detail) and present these findings in the supplementary tables, rather than test for interaction.
49.	P9 3rd para – I'm wondering whether the multivariable analyses to assess independent effects were actually necessary? In a way, they're answering a different question (about relative importance of different factors). Are the bivariate associations enough to answer whether the impact of any of the potential explanatory factors changed over time?	Thank you for this query. Because the exposure variables are associated with each other, there is potential for confounding effects and 'overlap' in their influence on adolescent smoking. It is therefore important to identify the *independent* association of each (using multivariable analysis). For example, as you noted in comment 44 above, parental smoking and exposure to smoking in the home are strongly related. Looking at bivariate associations alone would overestimate the individual and combined contribution of these two variables on adolescent smoking decline since they are strongly associated (see previous responses e.g. to comment 11).
50.	And would it have been possible to add the 2003 and 2015 datasets in order to look for an interaction between each factor and year to see if there were significant changes over time? (At the moment we need to look at each set of 95% Cis.)	Thank you for this suggestion. We re-ran the multivariable analysis using the data from years 2003 and 2015 only, and tested whether there was an interaction effect between year and the risk factors in the model. The results are presented in Supplementary Table S2. They show there was a statistically significant difference in odds ratios between 2003 and 2015 for 'best friend smokes' ($p < 0.0001$) and 'past week exposure to smoking in the home' ($p < 0.0001$) but not parental smoking ($p=0.8$ or sibling smoking ($p=0.06$).
	Results	
51.	P10 first para – overall smoking dropped by 1.2% per year – what did Maori adolescent smoking levels drop by?	See comment two above. We have added figures to the supplementary material showing smoking prevalence and exposure to risk factors over time by ethnicity and school decile. Supplementary figure S1 shows prevalence of regular smoking in Māori adolescents fell from 37% in 2002 to 11% in 2015 (approx. 2% per year in

		absolute terms).
52.	P10 middle para – This is inconsistent in what is reported for overall and what for Maori – would it be possible to add another table so we can see the numerical results (rather than as figures) for both groups?	Thank you for drawing our attention to this inconsistency. As previously noted, we have decided to remove results for Māori from the main paper, and instead we have presented changes in exposure to risk factors over time by ethnicity and school decile in the supplementary material (Figures S4 – S10).
53.	P10 middle para – And typo - I think the second ref to figures should be C and D (not B and C)	Thank you for bringing this error to our attention. It has been corrected.
54.	P13 top para – final sentence says Maori results aren't shown on Table 2, but they are. There is an un-labelled figure – perhaps that is Fig 2, which only seems to be one group.	As noted above, we have decided to move results for Māori into the supplementary material.
	Discussion	
55.	Might be worth going back to the Intro section p4 re policies – just to say that these analyses didn't capture any of these – and to make the point that there might be other reasons for the drops in youth smoking which, as the authors point out, results in a rather circular argument that it could be friends causing drops in smoking among each other.	We have amended the relevant paragraph of the discussion to read: "It is possible that changes in the social meaning of smoking[45 46] and the policy context[10 47] - factors that were not included in our analysis - may have played a role in triggering adolescent smoking decline." (p19)
	Supplementary table	
56.	It would be useful to have the school decile variable described here (eg as footnote) as well as in the methods. It would also be useful to have clarification of the source of the population stats – I assume these are high level publically available data?	Thank you for this suggestion. We have included an explanatory note, as suggested, and a hyperlink to the Ministry webpage with more information about school deciles and how they are calculated.

VERSION 2 – REVIEW

REVIEWER	Benjamin Kuntz Robert Koch Institute, Berlin (Germany)
REVIEW RETURNED	20-Apr-2018

GENERAL COMMENTS	The authors have responded satisfactorily to all my comments from my first report. I welcome the additional provision of relevant information in the form of supplementary figures in the Annex. In my opinion, the paper has improved significantly as a result of the revision. Thank you very much!

REVIEWER	Jennifer O'Loughlin University of Montreal, Canada
REVIEW RETURNED	18-Mar-2018

GENERAL COMMENTS	Overall comments • The authors have worked hard to respond to the reviewers' comments. However I think the manuscript is now trying to address too many issues and perhaps the content warrants 2-3 manuscripts. The authors should focus on the central question. • This manuscript asks an interesting and important question on whether declines in smoking mirror declines in risk factors for smoking, and these data could eventually shed light on trends in risk factors for smoking onset. As presented, the comparison of declines is ecologic (but still compelling). If the article recognized that the declines are ecologic and focused on this, the work could contribute to an interesting dialogue on some of the possible causes of the recent declines on smoking. • Some of the analytic choices are better justified in this version but it is not clear that the causal modeling is done correctly, and it is still not clear how the authors modeled change over time in individuals. • The manuscript requires considerable editing to tighten the wording, remove redundancy, make the meaning clear and improve logic Strengths and limitations • "accurate" implies both precision and validity. While precision is clear, how do we know there is no bias? • It is not clear what "accounts for change over time in statistical terms" means. This still sounds causal……I think that the data presented do not speak to causality and causal wording should be mitigated. Use of the word "driver" for example is misleading. While it may be that some of the risk factors investigated are in fact causal, these data do not inform that hypothesis. I think these data speak to associations and that should be made clear. Introduction • The introduction is thoughtful and interesting • The last sentence of the second paragraph may make readers think the study will address risk factor profiles….. Methods • The response proportions are low – given that the sample under-represent Maori and low SES students, could this bias the estimates? • Is non-response entirely related to refusals? • It is not clear why the analyses were restricted to 14 and 15 year-olds • Why were schools with less than 20 students excluded? How many schools overall were there? • Tables need more explicit titles and column/row titles that explain clearly what is included. For example, in Table 1 "responses included in the study" likely refers to the number of participants included in the analytic database • A ref is needed for the sentence stating that mother smoking is more strongly related to child smoking. • Why was number of days smoked at home re-coded. With the large sample size, this would seemingly result in losing important variability. Same comment for school decile • Modeling of causal pathways can be complex – see VanderWeele's (Harvard) work, which is the "go to" these days for causal modeling. • I remain unclear on the modeling. If the purpose is to understand how changes in risk factor prevalence relate to changes in prevalence of smoking, should the outcome not be change in prevalence of smoking and the independent variables should be

	changes in risk factors? Graphically depicting declines in risk factors against declines in smoking is compelling, but it is ecologic in that there is no direct link between decreases in exposure and outcome within individuals in the depiction. Results • How specifically did the authors make decisions on whether the strength of an association differed over time? Differences in odds over time is another interesting story in these data Discussion • It is possible that declines in cigarette smoking "caused" declines in friends smoking such that what causes what is very difficult to sort out. Bidirectional associations may be at play • The changes in the strengths of associations over time is compelling and worth reflecting on • The discussion brings up interesting issues but wanders somewhat from the central issues. • A possible way to help the authors focus this manuscript on the central issue is to limit words to 3000 for example. The reviewer believes that the declines in risk factors juxtaposed against the declines in smoking tells the main story in these data. The article should be focused on this.

REVIEWER	Helen Sweeting University of Glasgow, UK
REVIEW RETURNED	02-Apr-2018

GENERAL COMMENTS	I think the authors' decision to focus only on factors associated with the decline across the whole population and to place the Maori analyses in the supplementary tables was a good one, and has both strengthened and clarified the paper. My comments below are all very small, mainly suggesting changes of wording which I think would make it even clearer. ABSTRACT Given the change of emphasis from causal to statistical explanation, I think the first objective would be better phrased as '… predictors are associated with …' rather than 'account for'. Methods – typo – 'based ON annual survey …'. Conclusions – best friend smoking as risk factor is a result, so belongs in that para. INTRODUCTION P4, lines 24-34 – The focus on ethnic/socioeconomic disparities and rates of Maori smoking seems like a hang-over from the previous version and not the focus of this analysis – including the importance of 'understanding the risk factor profile of priority groups', which is a different question. I think this is distracting. Is it needed? METHODS P6, line 50 - I'm still going on about ASH New Zealand – can I suggest that here it's 'from the Action on Smoking (ASH) NZ Year 10 …' and thereafter consistently referred to as ASH-NZ. P9, line 34 – think it would be good to reference previous research suggesting maternal smoking more strongly associated with

	adolescent smoking initiation than paternal smoking. P10, ethnicity – states the four groups were dichotomised into Maori/non-Maori – but Fig 2 shows smoking rates for all four groups and most of the supplementary analyses were done on all four. RESULTS Table 2– I found this a bit confusing. If I've understood each model correctly, then would it be clearer to say: Model 1 Unadjusted odds ratio for each variable; Model 2 Odds ratio for each variable, adjusted for demographic factors*; Model 3 Mutually adjusted odds ratio for parental, sibling and best friend smoking, adjusted for demographic factors*; Model 4 Mutually adjusted odds ratio for all variables, adjusted for demographic factors*'. Plus * demographic factors = sex, age, ethnicity, school decile. And given the three-category variables include OR = 1.0 for the contrast category, could this also be added for older sib and best friend (ie doesn't / does) – I know this isn't strictly necessary, but it would make the table easier to read. DISCUSSION P16, line 35 – typo, needs an apostrophe plus S for year old's. P16, last couple of lines – would this be clearer? '.. smoking and two risk factors – exposure to smoking in the home and best friend smoking – increased markedly over the study period, while relationships with both parental and older sibling smoking remained unchanged', P17 – lines 10-14 – again, would this be clearer? '.. as prevalence of exposure to all three declined over the study period and each was independently associated with regular smoking (with previous research suggestive of a causal relationship).' P17 – and the following para? 'The marked differences between unadjusted and adjusted ORs suggest that the risk factors in our study were strongly related to one another. This could result from the fact that adolescents tend to experience life within smoking or non-smoking social circles, and therefore the smoking status of family members and friends are strongly correlated. This is consistent with research that shows that smoking clusters within occupations and neighbourhoods, and is becoming increasingly concentrated in particular communities (refs).' P17 – and the next one? '… to interpret. Parental smoking was strongly associated with adolescent smoking in the model adjusting only for demographic variables and (somewhat less so) in mutually adjusted models that excluded smoking in the home.' P17, line 39 – SHS exposure – this is a new term and sounds almost objectively measured. Is it just what's described elsewhere as 'exposure to smoking in the home'? P19, line 7 – typo – 'may have influenced ...'.

	P19, line 9 – maybe worth adding the Green et al paper to these refs on policy impacts. P20, lines 41-45 – uses socioeconomic position, SEP and socioeconomic status within a couple of sentences. CONCLUSIONS The summary sentence (which might be all some readers look at!) doesn't mention the contribution of declining best friend smoking.

VERSION 2 – AUTHOR RESPONSE

	Reviewer 1: Benjamin Kuntz	
	Comment	**Response**
1.	The authors have responded satisfactorily to all my comments from my first report. I welcome the additional provision of relevant information in the form of supplementary figures in the Annex. In my opinion, the paper has improved significantly as a result of the revision. Thank you very much!	Thank you. Reviewer 1 will be pleased to see that the supplementary material (added at his recommendation) has been retained in the latest version.
	Reviewer 2: Jennifer O'Loughlin	
	Comment	**Response**
	Overall:	
2.	The authors have worked hard to respond to the reviewers' comments.	Thank you for acknowledging this.
3.	However I think the manuscript is now trying to address too many issues and perhaps the content warrants 2-3 manuscripts. The authors should focus on the central question.	We agree that (partially due to our efforts to respond to reviewers' suggestions) the manuscript had become rather diffuse in its focus. We have tightened the revised paper considerably, removing material that is not central to our research question and referring readers to our secondary paper (now published) which explores changes in strength of association between risk factors and adolescent smoking over time and the increasing importance of 'exposure to smoking in the home' as a risk factor. See: Ball J, Sim D, Edwards R. Addressing ethnic disparities in adolescent smoking: Is reducing exposure to smoking in the home a key? Nicotine Tob Res 2018. doi:10.1093/ntr/nty053

4.	This manuscript asks an interesting and important question on whether declines in smoking mirror declines in risk factors for smoking, and these data could eventually shed light on trends in risk factors for smoking onset. As presented, the comparison of declines is ecologic (but still compelling). If the article recognized that the declines are ecologic and focused on this, the work could contribute to an interesting dialogue on some of the possible causes of the recent declines on smoking Some of the analytic choices are better justified in this version but it is not clear that the causal modeling is done correctly, and it is still not clear how the authors modeled change over time in individuals.	Thank you for this comment. Our intention was never to do 'causal modelling', but rather to investigate the extent to which changes in *known* risk factors could account for the decline in adolescent smoking over time. We agree that the approach we originally took did not fully exploit the potential of our individual level data and, after further consideration, we have substantially amended our analytical approach. We now provide a trend analysis which models regular smoking as a function of year, and tests the extent to which our risk factors account for the observed trend of declining smoking prevalence. As noted in the revised manuscript (discussion p16): "Definitively establishing the reason(s) for the decline in adolescent smoking is not possible using repeat cross-sectional data (or indeed via any single study). However, trend analysis using statistical modelling allowed us to explore the relationships between survey year, risk factors and outcomes, and thereby (potentially) account for changes over time in statistical terms. This approach has allowed us to rule out hypothesised explanations for population level change over time, and adds to the evidence base about the most likely explanations for the decline of smoking in young people.
5.	The manuscript requires considerable editing to tighten the wording, remove redundancy, make the meaning clear and improve logic	As noted in comment 3, the manuscript has been considerably tightened.
	Strengths and limitations:	
6.	"accurate" implies both precision and validity. While precision is clear, how do we know there is no bias?	Thank you for noting that this was the wrong choice of word. It has been changed to "precise."
7.	It is not clear what "accounts for change over time in statistical terms" means. This still sounds causal……I think that the data presented do not speak to causality and causal wording should be mitigated. Use of the word "driver" for example is misleading. While it may be that some of the risk factors investigated	Thank you for this comment, which (given the change in analytical approach) may no longer be relevant. This wording was suggested by Reviewer 3 who commented: "It might also be worth clearly stating at the start that the analysis is really about 'explaining' the changes in stats terms rather than being totally

	are in fact causal, these data do not inform that hypothesis. I think these data speak to associations and that should be made clear.	sure about causality." The choice of wording in the amended manuscript is consistent with other studies which use similar methods of trend analysis, and talk about "accounting for" or "explaining" trends in adolescent behaviour using these methods e.g. de Looze et al (2017), Grucza et al (2016).
	Introduction	
8	The introduction is thoughtful and interesting	Thank you
9	The last sentence of the second paragraph may make readers think the study will address risk factor profiles…..	This sentence has been deleted, as it distracts from the central focus of the manuscript.
	Methods	
10	The response proportions are low – given that the sample under-represent Maori and low SES students, could this bias the estimates?	Our study includes approximately half New Zealand's 14-15 year olds each year, with non-response almost entirely at the school level. Our comparison between socio-demographic characteristics of the whole NZ Year 10 population (based on Ministry of Education data) and the characteristics of the individuals included in our analysis by year (supplementary table S1) shows that absolute differences were small in percentages of Māori and children from low decile schools. This suggests response bias was unlikely to be a substantial issue in this study. As we noted in our previous response to reviewers (and in the penultimate paragraph of the discussion section) Māori and low decile students were modestly under-represented in our study and therefore adolescent smoking in the population is likely to be slightly underestimated (since smoking rates are higher in these under-represented groups). However, because the main focus of our paper is on trends over time, and because the under-representation of Māori and low decile students is consistent over time, it is unlikely to affect our key findings. In addition, as non-response occurred largely at school level rather than at individual level, it seems less likely that non-response will have resulted in substantial bias in the estimates of prevalence of in the assessment of the association between potential

		determinants and smoking behaviour.
11	Is non-response entirely related to refusals?	As noted in the methods section, paragraph two: "School response rates range from 44-67% (with a lower school response rate in 2015 due to limited resources for liaising with schools that year)." As noted above, non-response is almost entirely at the school level, and this has been further clarified in the manuscript. As explained later in the methods section, the questionnaire is completed in class time. Data on individual level refusal is not collected, since the survey is administered by schools, but based on ASH NZ's response rate calculations (which take into account 'normally present students') and anecdotal evidence from schools, we understand that refusal is rare. Individual non-participation is low and is generally due to absence.
12	It is not clear why the analyses were restricted to 14 and 15 year-olds	Almost all Year 10 students are aged 14 or 15 at the time of the survey. Restricting the analyses to this age group adds to ease of reporting, and removes outliers many of whom may have given inaccurate responses erroneously or deliberately.
13	Why were schools with less than 20 students excluded? How many schools overall were there?	Our modelling approach involves entering School ID as a random effect (to account for clustering at the school level), and imprecision of prevalence estimates at the school level due to low numbers of respondents could lead to instability of our models. Overall, 377 schools participated in the survey (185 – 258 per year).
14	Tables need more explicit titles and column/row titles that explain clearly what is included. For example, in Table 1 "responses included in the study" likely refers to the number of participants included in the analytic database	A note has been added to Table 1 to clarify that 'valid survey responses' means those with completed data for age, sex, ethnicity and smoking (the ASH NZ criteria for inclusion). The fourth column has been relabelled 'Valid survey responses that met study inclusion criteria' to further clarify that this is the number of participants included in our study.

15	A ref is needed for the sentence stating that mother smoking is more strongly related to child smoking.	A reference to a recent systematic review and meta-analysis has been added.
16	Why was number of days smoked at home re-coded. With the large sample size, this would seemingly result in losing important variability. Same comment for school decile	We re-coded exposure to smoking in the home and school decile only for descriptive analysis, for simplicity of reporting. For trend analyses using logistic regression, these variables were entered into the model in their original numerical (un-grouped) form. This has been clarified in the methods section.
17	Modeling of causal pathways can be complex – see VanderWeele's (Harvard) work, which is the "go to" these days for causal modeling.	Thank you for drawing our attention to VanderWeele's work on causal modelling. However, our revised manuscript focuses on trend analysis, so this work is not relevant to our revised approach.
18	I remain unclear on the modeling. If the purpose is to understand how changes in risk factor prevalence relate to changes in prevalence of smoking, should the outcome not be change in prevalence of smoking and the independent variables should be changes in risk factors? Graphically depicting declines in risk factors against declines in smoking is compelling, but it is ecologic in that there is no direct link between decreases in exposure and outcome within individuals in the depiction.	We now model (at the individual level) relationships between year, independent variables and adolescent smoking. This approach, we believe, provides a more direct and compelling answer to our research question. Trend analysis is an established method for assessing the extent to which an independent variable "explains" or "accounts for" change over time in an outcome – see de Looze et al (2017) and Grucza (2016). Full references are provided under the 'References' heading below.
	Results	
19	How specifically did the authors make decisions on whether the strength of an association differed over time?	This was explained on p 17 of the submitted manuscript: "The statistical significance ($p<0.0001$) of these changes in OR was confirmed by modelling the interaction effect between risk factors and year" with results detailed in the supplementary material. However, due to our change of approach, the investigation of strength of associations has been removed from the amended manuscript. Instead, we refer readers to the findings of our recently accepted *Nicotine &Tobacco Research* paper accepted for publication which includes these analyses.
20	Differences in odds over time is another interesting story in these data	We agree, and (as noted in comment 3) we have recently published a paper focusing on these findings.
	Discussion	
21	It is possible that declines in cigarette	We agree that bidirectional associations are likely at

	smoking “caused” declines in friends smoking such that what causes what is very difficult to sort out. Bidirectional associations may be at play	play. Further we argue it is nonsensical to suggest that declining best friend smoking ‘explains’ declining smoking in adolescents at the population level, since both respondents and their best friends are part of the same adolescent population. We have made these points clearer in the discussion section of the revised manuscript.
22	The changes in the strengths of associations over time is compelling and worth reflecting on	We agree, and (as noted in comment 3) we have recently published a paper focusing on these findings.
23	The discussion brings up interesting issues but wanders somewhat from the central issues.	The discussion has been tightened, and reflects on the question of why adolescent smoking is declining, what our findings contribute, and what remains unknown.
24	A possible way to help the authors focus this manuscript on the central issue is to limit words to 3000 for example. The reviewer believes that the declines in risk factors juxtaposed against the declines in smoking tells the main story in these data. The article should be focused on this.	The word count has been substantially reduced to 3 874 words. We believe our trend analysis answers our research question compellingly, and the paper is much improved as a result of focusing on these findings.
	Reviewer 3: Helen Sweeting	
	Comment	**Response**
25	I think the authors’ decision to focus only on factors associated with the decline across the whole population and to place the Maori analyses in the supplementary tables was a good one, and has both strengthened and clarified the paper. My comments below are all very small, mainly suggesting changes of wording which I think would make it even clearer	Thank you. In response to criticisms from Reviewer 2, we have substantially amended our analytical approach and have re-written much of the abstract, results and discussion sections. We appreciate the time Reviewer 3 has taken to make wording suggestions, however many of the passages she refers to have now been removed or substantially revised.
	Abstract	
26	Given the change of emphasis from causal to statistical explanation, I think the first objective would be better phrased as ‘… predictors are associated with …’ rather than ‘account for’.	The revised paper presents a trend analysis, modelling the extent predictors account for the trend in adolescent smoking over time. We think the Reviewer 3 will agree that the phrase ‘account for’ is appropriate in the context of trend analysis, and our terminology is consistent with that used in similar studies (e.g. Grucza et al, 2016; de Looze et al, 2017).

27	Methods, typo: ‘based ON annual survey …’.	Thank you for picking up this typo, which has been corrected.
28	Conclusions – best friend smoking as risk factor is a result, so belongs in that para	This part of the abstract has been rewritten.
	Introduction	
29	P4, lines 24-34 – The focus on ethnic/socioeconomic disparities and rates of Maori smoking seems like a hang-over from the previous version and not the focus of this analysis – including the importance of ‘understanding the risk factor profile of priority groups’, which is a different question. I think this is distracting. Is it needed?	We have removed the final sentence (about ‘understanding the risk factor profile of priority groups’) which we agree is a distraction. However we have retained a brief discussion about disparities in the second paragraph of the introduction as we see this as important background information, and it provides a lead in to the stratified descriptive analysis (provided in the supplementary file) which Reviewer 1 recommended.
	Methods	
30	P6, line 50 - I’m still going on about ASH New Zealand – can I suggest that here it’s ‘from the Action on Smoking (ASH) NZ Year 10 …’ and thereafter consistently referred to as ASH-NZ.	We have made this amendment.
31	P9, line 34 – think it would be good to reference previous research suggesting maternal smoking more strongly associated with adolescent smoking initiation than paternal smoking	A systematic review and meta-analysis on this topic has been referenced.
32	P10, ethnicity – states the four groups were dichotomised into Maori/non-Maori – but Fig 2 shows smoking rates for all four groups and most of the supplementary analyses were done on all four.	The submitted manuscript stated that ethnicity was prioritised (Māori, Pacific, Asian, NZEO) and dichotomised into Māori and non-Māori. The dichotomisation was solely for the purposes of a supplementary analysis, so to avoid confusion, in the revised manuscript it is discussed alongside description of methods for that supplementary analysis.
	Results	
33	Table 2– I found this a bit confusing. If I’ve understood each model correctly, then would it be clearer to say: Model 1 Unadjusted odds ratio for each variable; Model 2 Odds ratio for each variable, adjusted for demographic factors*;	This table has been removed. Readers are referred to our recently published paper which establishes associations between the included risk factors and adolescent smoking:

	Model 3 Mutually adjusted odds ratio for parental, sibling and best friend smoking, adjusted for demographic factors*; Model 4 Mutually adjusted odds ratio for all variables, adjusted for demographic factors*'. Plus * demographic factors = sex, age, ethnicity, school decile. And given the three-category variables include OR = 1.0 for the contrast category, could this also be added for older sib and best friend (ie doesn't / does) – I know this isn't strictly necessary, but it would make the table easier to read.	Ball J, Sim D, Edwards R. Addressing ethnic disparities in adolescent smoking: Is reducing exposure to smoking in the home a key? *Nicotine Tob Res* 2018 doi: 10.1093/ntr/nty053 [published Online First: 2018/03/20]
	Discussion	
34	P16, line 35 – typo, needs an apostrophe plus S for year old's.	This sentence was deleted because ASH NZ have now published their findings on 2015 smoking prevalence in adolescents (based on the same data we have used in our study), making our 'update' redundant. The ASH NZ publications are now referenced in the introduction.
35	P16, last couple of lines – would this be clearer? '.. smoking and two risk factors – exposure to smoking in the home and best friend smoking – increased markedly over the study period, while relationships with both parental and older sibling smoking remained unchanged',	We have deleted this paragraph.
36	P17 – lines 10-14 – again, would this be clearer? '.. as prevalence of exposure to all three declined over the study period and each was independently associated with regular smoking (with previous research suggestive of a causal relationship).	We have deleted this paragraph.
37	P17 – and the following para? 'The marked differences between unadjusted and adjusted ORs suggest that the risk factors in our study were strongly related to one another. This could result from the fact that adolescents tend to experience life within smoking or non-smoking social circles, and therefore the smoking status of family members and friends are strongly correlated. This is	We have deleted this paragraph.

	consistent with research that shows that smoking clusters within occupations and neighbourhoods, and is becoming increasingly concentrated in particular communities (refs).'	
38	P17 – and the next one? '… to interpret. Parental smoking was strongly associated with adolescent smoking in the model adjusting only for demographic variables and (somewhat less so) in mutually adjusted models that excluded smoking in the home.'	We have deleted this paragraph.
39	P17, line 39 – SHS exposure – this is a new term and sounds almost objectively measured. Is it just what's described elsewhere as 'exposure to smoking in the home'?	Yes, it is. Thank you for picking up this inconsistency.
40	P19, line 7 – typo – 'may have influenced ...'.	This sentence has been amended.
41	P19, line 9 – maybe worth adding the Green et al paper to these refs on policy impacts.	This reference has been added.
42	P20, lines 41-45 – uses socioeconomic position, SEP and socioeconomic status within a couple of sentences.	Thank you for picking up this inconsistency. We now refer to socioeconomic position (abbreviated to SEP) throughout.
	Conclusions	
43	The summary sentence (which might be all some readers look at!) doesn't mention the contribution of declining best friend smoking.	The concluding paragraph has been amended, and now makes explicit reference to best friend smoking.

VERSION 3 – REVIEW

REVIEWER	Helen Sweeting MRC/CSO Social & Public Health Sciences Unit, University of Glasgow, UK
REVIEW RETURNED	29-Jul-2018

GENERAL COMMENTS	I have had another good look through this paper and am happy with it as it now stands. My R2 comments were all fairly small and have been addressed. I think this V3 paper is much stronger, clearer and more useful to readers than the V1 which we first saw.