

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Socioeconomically disadvantaged smokers' ratings of plain and branded cigarette packaging: An experimental study
AUTHORS	Guillaumier, Ashleigh; Bonevski, Billie; Paul, Chris; Durkin, Sarah; D'Este, Catherine

VERSION 1 - REVIEW

REVIEWER	Jonathan Williman University of Otago, Christchurch New Zealand
REVIEW RETURNED	06-Nov-2013

GENERAL COMMENTS	<p>Minor comments</p> <p>The health warning label on the Plain Packaging (B & D) in Figure 1 is "Smoking causes mouth and throat cancer" whereas the text on page 7 states that "All pack conditions featured the same HWL "Smoking causes peripheral vascular disease". Please update the figure accordingly to prevent confusion.</p> <p>This is a well written paper, with clearly described methodology and results. The difference in effect of plain packaging by brand is interesting and has been covered well in the discussion.</p>
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REVIEWER	Phil Gendall University of Otago, New Zealand
	No competing interests
REVIEW RETURNED	19-Nov-2013

GENERAL COMMENTS	<p>This is an interesting and well written paper, based on a competent piece of research, but there are at least two aspects that reduce its value. First, the study was conducted before the introduction of plain packaging in Australia; consequently, it could be argued that events have overtaken the research. What we are interested in now is how socioeconomically disadvantaged smokers have reacted to plain packaging, not how they might have reacted to it. The authors can't do anything about this, but their abstract should acknowledge the fact by saying 'the potential impact' to avoid giving the impression that they have studied the actual impact of plain</p>
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packaging.

Second, the study involved two brands, Winfield and Benson & Hedges. As the authors themselves note, B&H is a premium brand that socioeconomically disadvantaged smokers are unlikely to smoke. Consequently, it is not particularly surprising that the results for B&H were confounded. The rationale for choosing B&H as one of the brands studied was that it is one of the most popular brands in Australia. However, the authors' justification for their study is that it examines a particular demographic of interest, so their research design (or at least the explanation of it) should reflect this.

Neither of these issues is an absolute barrier to publishing the paper. There is nothing the authors can do about the first problem, but they could be asked to justify their choice of B&H as one of their test brands in the context of the demographic they studied, and to analyse their Winfield results for Winfield smokers.

This is an interesting and well written paper, but there are at least two aspects that reduce its value. First, your study was conducted before the introduction of plain packaging in Australia; consequently it could be argued that events have overtaken the research. What we are interested in now is how socioeconomically disadvantaged smokers have reacted to plain packaging, not how they might have reacted to it. However, there is nothing you can do about this, except to be careful not to give the impression that your study examined the actual effect of plain packaging (for example, in the Objectives section of your Abstract I think you should say "The study aimed to test the potential impact of plain packaging ...").

Second, the study involved two brands, Winfield and Benson & Hedges. As you note, B&H is a premium brand that most socioeconomically disadvantaged smokers are unlikely to smoke (actually, you say the sample 'may have less experience with the B&H brand' but only 1.6% were regular B&H smokers, so I think you can be more definite about this). Consequently, it is not particularly surprising that the results for B&H were confounded. I understand why you chose B&H as one of the brands, but given the population you were studying I'm not sure this was the best choice. Again, there is not much you can do about this, but I do think you need to revisit the rationale for selecting B&H. Perhaps you could argue that the two brands allowed you to demonstrate the importance of branding in the tobacco market. Among the socially disadvantaged, one could argue that plain packaging of B&H is unlikely to have much effect, because this is a premium brand with low penetration in this demographic, but plain packaging of Winfield could be expected to have a significant effect, for the opposite reason. And this is what you observed. You might be able to construct a more compelling argument than this, but my point is that I think you

	<p>could turn a potentially limiting feature of your research into a positive one.</p> <p>If you had enough Winfield smokers in your sample, you could analyse the Winfield results for Winfield smokers (as far as I can tell, you had about 67 regular Winfield smokers). You might find larger differences among this sub-sample and, if you did, it would provide some evidence to support your argument on page 12 that removing branding elements is more effective than tapping into negative harm perceptions.</p> <p>Finally, a comment on the items you used in your assessment of responses to the pack images. I know these were taken from other published studies but a number of them are poorly-designed. For example, in the pack characteristics battery, ‘This pack is popular among smokers’ is a factual question not an opinion question like the other three items. The smoker characteristics battery uses the concept of a ‘typical’ smoker. This assumes respondents know, or can imagine’ what a ‘typical’ smoker is; this may or may not be the case. Furthermore, smokers smoke cigarettes or brands not packs. Consequently, a B&H smoker might be regarded as successful regardless of the pack design. In the taste attributes battery, the last item should logically read, ‘I would expect the cigarettes in this pack to be harmful to my health’. Also, the instruction says that the phrases describe the taste of cigarettes from the pack shown, but two of the items are not about taste (one is about nicotine and tar and one is about harm to health), so why not simply remove ‘taste of’ from the instructions.</p> <p>As I say, I know you didn’t design these questions, but using poorly-designed survey items because they have been used before by someone else, no matter eminent, is not a particularly compelling justification (and neither is a high Cronbach’s alpha – poor items can correlate just as easily as good ones). Something you might like to think about for future research.</p>
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REVIEWER	James Thrasher Associate Professor, Department of Health Promotion, Education and Behavior, University of South Carolina, USA
REVIEW RETURNED	08-Dec-2013

GENERAL COMMENTS	<p>This study aims to determine the potential impact of plain packaging among socioeconomically disadvantaged smokers, who are a key population given that smoking is increasingly concentrated in this group. As such, the study addresses a critical gap in the literature on plain packaging. Nevertheless, the paper would be strengthened through a clearer explanation of the methods used, as well as by conducting some additional analyses that address some of the questions raised by the current approach.</p>
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Abstract:

Page 2, Results: it is counterintuitive to talk about lower odds and then show ORs that are greater than 1. The authors should consider talking about higher odds or inverting the coding of variables so that the statistical results match the narrative.

Page 3, article focus: The authors might consider mentioning that plain packaging not only reduces perceptions of brand appeal and cessation intentions, but also reduced demand (as they describe in the literature review for experimental auction studies).

Introduction:

Page 4, lines 30-32: do the authors mean to state that economy packs emphasize “quantity”? That seems more important. Furthermore, since the current study is on packaging, they should consider removing the mention of packaging from parentheses when talking about price marketing.

Page 5, paragraph 3: The authors should consider mentioning that the law also limits pack and stick dimensions, as many researchers do not know about that aspect of the policy. Also, it would be good if the authors provided some more detail about the Wakefield study and how the current one may address issues that that study did not address. Did they examine differences in effects by SES? Did they under-represent low SES smokers? Was novelty or size of warning label content controlled for or addressed?

Methods:

Page 6, Design. The authors would ideally provide a justification for the selection of each brand and for treating them separately in the analysis. Do they have any hypotheses regarding why plain packs would have differential effects across brands? Given the differential effects, did the authors consider looking at whether effects differed by whether people smoked the brand they evaluated?

Figure 1 shows the pack images, which correctly use the same image, but the text for the plain packaging is different and even addresses a different health outcome than the one shown in the image. I hope that these images were not the ones used in the experiment and that the correct ones can be inserted. If these images were used, then the limitation section should address this issue of the incompatibility of the warning text and image in the plain pack conditions, as well as differences in the textual warnings content between plain and branded pack conditions.

Page 6, Design. The dates of data collection include the period when smokers could have already been exposed to and purchased plain packs. In the limitations section, the authors should consider

mentioning any effects they might expect from prior exposure to the policy. For example, that might explain why people did not rate packs as different for some measures.

Page 6, line 32. Suggest eliminating the phrase “of a number of”

Page 7, line 40-42. Correct this if the textual statement was different for plain vs. branded packs. The authors should also clarify that the same image was used for all packs and that it comes from the 1st round or warnings implemented since 2006 (or 2007, if it is from the second set of the first round).

Page 7, outcome measures. To make it easier for the reader, I suggest consolidation of information on these measures to the measurement section, including discussion about combining indicators (i.e., instrument evaluation on page 8, line 49 to page 9, line 8) and alpha results described in the results section (i.e., page 10, 16-24). Suggest using correlations when describing scales that combine two items, and alpha when combining 3 or more items, which is more standard practice. Can the authors cite any information on validation of the Wakefield et al measures, including when items are combined in the ways they are combined in this study? Also, column headings for Table 1 indicate that responses involved a visual analogue scale, which implies that people indicated where they fell between two extremes without reference to any numbers. From the description of the measures, it seems that there were 7 numbered response options, with verbal anchors used at both extreme ends of the response options. If so, then this is not a visual analogue scale. Also, I think that Table 1 could be eliminated without any loss of information.

Page 8, Purchase intentions. Can the authors provide any citations showing the validity of purchase intention tasks like this, where people are given two pack alternatives and a “no purchase” option? Does this predict purchase behavior? If not, then this limitation should be mentioned in the discussion.

Page 8, sociodemographic variables. The income and education questions categories used would ideally be mentioned, especially given the focus on this socioeconomically deprived population.

Page 9, analysis. Why not conduct statistical tests of the sample characteristics across conditions? The look generally similar, but there are some for which statistically significant differences are likely (e.g., higher education and income for Plain B&H), which may help explain some of the discrepant results by brand. If this is true, then randomization did not completely work, and a modeling approach may need to be considered that allows for assessing main effects by condition and statistical controls for characteristics that differ across groups. I believe that the Kruskal-Wallis approach allows for statistical controls, but the authors should consult a

statistician to consider options. If there are no non-parametric analogues to multivariate linear regression, then the authors should consider a standard linear regression approach, perhaps as a sensitivity analysis. Unless the authors have a hypothesis based on prior literature regarding differences in plain packaging impact by brand popularity, I think the paper would be stronger if it tested the plain pack condition as a main effect, and included an interaction to test whether brand type matters. I realize that the authors' concerns about power, but I think this is a stronger approach than the exploratory, data driving approach that is currently taken (i.e., page 9, lines 16-20).

Page 10, Results. As described earlier, the authors should test for differences in sample characteristics across conditions to confirm adequate randomization.

Page 10, line 57-59. Were data on manufactured vs. roll your own collected? If so, these percentages should be included in Table 2 and could potentially be treated as control variables, as people who use roll your own may have different responses to plain manufactured packs.

Page 11, purchase intent. As with the primary pack perception variables, it would be stronger if the model included statistical controls, particularly for those variables that differed across conditions.

Page 11, Discussion 2nd paragraph. The authors may want to emphasize that the apparently stronger effects for plain packs with more familiar brands may be what you want, as those are the ones that people are more likely to smoke. They authors may consider an interaction between condition and whether their usual brand was the same as the one shown vs not. This would help make a stronger case for that argument. Pooling data across the two brands may help provide the power needed for this kind of assessment.

Page 12, lines 9 -13. Is there prior evidence for stronger effects among youth compared to adults? This may help explain why effects on perceived harm were not found for this older population.

Page 13, strengths and limitations. The authors should consider the modality of stimulus presentation (computer image instead of actual pack) as a potential limitation. However, experiments that use real plain packs (e.g., Thrasher et al 2011) or real packs with warning labels (Thrasher et al. Cancer Causes and Control. 2012) provide results that are generally consistent with those that present packs on computer screens (e.g., Hammond et al. Cancer Causes and Control. 2012).

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

1. "The health warning label on the Plain Packaging (B & D) in Figure 1 is "Smoking causes mouth and throat cancer" whereas the text on page 7 states that "All pack conditions featured the same HWL "Smoking causes peripheral vascular disease". Please update the figure accordingly to prevent confusion."

We apologise for this error. The correct figure has now been uploaded.

Reviewer 2:

2. "First, your study was conducted before the introduction of plain packaging in Australia; consequently it could be argued that events have overtaken the research. What we are interested in now is how socioeconomically disadvantaged smokers have reacted to plain packaging, not how they might have reacted to it. However, there is nothing you can do about this, except to be careful not to give the impression that your study examined the actual effect of plain packaging (for example, in the Objectives section of your Abstract I think you should say "The study aimed to test the potential impact of plain packaging ..."."

The manuscript has been revised to ensure that it does not give the impression that the study examined the actual effect of plain packaging, (see Abstract: Objectives; page 2, line 5).

3. "Second, the study involved two brands, Winfield and Benson & Hedges. As you note, B&H is a premium brand that most socioeconomically disadvantaged smokers are unlikely to smoke (actually, you say the sample 'may have less experience with the B&H brand' but only 1.6% were regular B&H smokers, so I think you can be more definite about this). Consequently, it is not particularly surprising that the results for B&H were confounded. I understand why you chose B&H as one of the brands, but given the population you were studying I'm not sure this was the best choice. Again, there is not much you can do about this, but I do think you need to revisit the rationale for selecting B&H. Perhaps you could argue that the two brands allowed you to demonstrate the importance of branding in the tobacco market. Among the socially disadvantaged, one could argue that plain packaging of B&H is unlikely to have much effect, because this is a premium brand with low penetration in this demographic, but plain packaging of Winfield could be expected to have a significant effect, for the opposite reason. And this is what you observed. You might be able to construct a more compelling argument than this, but my point is that I think you could turn a potentially limiting feature of your research into a positive one."

Modifications as per the reviewer's suggestion have been made - on page 12 in the discussion section (lines 3-5 and lines 11-33).

4. "If you had enough Winfield smokers in your sample, you could analyse the Winfield results for Winfield smokers (as far as I can tell, you had about 67 regular Winfield smokers). You might find

larger differences among this sub-sample and, if you did, it would provide some evidence to support your argument on page 12 that removing branding elements is more effective than tapping into negative harm perceptions.”

We had n=50 smokers selecting Winfield as their regular brand, with 10 to 16 per experimental condition. This number is too small to undertake a subgroup analysis.

5. “Finally, a comment on the items you used in your assessment of responses to the pack images. I know these were taken from other published studies but a number of them are poorly-designed. For example, in the pack characteristics battery, ‘This pack is popular among smokers’ is a factual question not an opinion question like the other three items. The smoker characteristics battery uses the concept of a ‘typical’ smoker. This assumes respondents know, or can imagine’ what a ‘typical’ smoker is; this may or may not be the case. Furthermore, smokers smoke cigarettes or brands not packs. Consequently, a B&H smoker might be regarded as successful regardless of the pack design. In the taste attributes battery, the last item should logically read, ‘I would expect the cigarettes in this pack to be harmful to my health’. Also, the instruction says that the phrases describe the taste of cigarettes from the pack shown, but two of the items are not about taste (one is about nicotine and tar and one is about harm to health), so why not simply remove ‘taste of’ from the instructions.

As I say, I know you didn’t design these questions, but using poorly-designed survey items because they have been used before by someone else, no matter eminent, is not a particularly compelling justification (and neither is a high Cronbach’s alpha – poor items can correlate just as easily as good ones). Something you might like to think about for future research.”

We agree with the reviewer and have added two sentences to the limitations section of the Discussion on page 14 (lines 11-17).

Reviewer 3:

Abstract

6. “Page 2, Results: it is counterintuitive to talk about lower odds and then show ORs that are greater than 1. The authors should consider talking about higher odds or inverting the coding of variables so that the statistical results match the narrative.”

Reference to the odds ratio results in the abstract has been revised to explain higher odds, as suggested by the reviewer (see Abstract, Results; page 2, lines 45-47).

7. “Page 3, article focus: The authors might consider mentioning that plain packaging not only reduces perceptions of brand appeal and cessation intentions, but also reduced demand (as they describe in the literature review for experimental auction studies).”

Reference to plain packaging also reducing demand has been added to the Article focus section of the ‘Article Summary’ (page 3, line 8).

Introduction

8. "Page 4, lines 30-32: do the authors mean to state that economy packs emphasize "quantity"? That seems more important. Furthermore, since the current study is on packaging, they should consider removing the mention of packaging from parentheses when talking about price marketing."

As is explained in the cited reference, economy packs need to emphasise the quality of the product as low-income smokers are sensitive to the social stigma of smoking very cheap products.(1)

The parentheses have been removed from the information explaining price-marking (see page 4, lines 31-33).

9. "Page 5, paragraph 3: The authors should consider mentioning that the law also limits pack and stick dimensions, as many researchers do not know about that aspect of the policy."

This information has been added to the paragraph (see page 5, line 45).

10. "Page 5, paragraph 3: Also, it would be good if the authors provided some more detail about the Wakefield study and how the current one may address issues that that study did not address. Did they examine differences in effects by SES? Did they under-represent low SES smokers? Was novelty or size of warning label content controlled for or addressed?"

Additional details about the Wakefield study have been provided, as suggested by the reviewer, (see page 5, line 57-58).

Methods

11. "Page 6, Design. The authors would ideally provide a justification for the selection of each brand and for treating them separately in the analysis. Do they have any hypotheses regarding why plain packs would have differential effects across brands? Given the differential effects, did the authors consider looking at whether effects differed by whether people smoked the brand they evaluated?"

As explained in the 'Outcome measure assessment' paragraph of the Statistical Analyses section of the Methods, we needed to treat the brands separately in analysis due to a potential interaction between pack type and brand name that was observed during exploratory data analysis (see page 9, line 36-40).

It was not possible to investigate whether differences in outcomes for plain versus branded packs varied according to whether or not participants smoked the brand they evaluated, due to the small numbers of individuals smoking each of Winfield (n=50 in total, 10-16 per pack condition) or B&H (n=4 in total, 0-2 per condition) and thus the extremely low power.

12. "Figure 1 shows the pack images, which correctly use the same image, but the text for the plain packaging is different and even addresses a different health outcome than the one shown in the image. I hope that these images were not the ones used in the experiment and that the correct ones can be inserted. If these images were used, then the limitation section should address this issue of the incompatibility of the warning text and image in the plain pack conditions, as well as differences in the textual warnings content between plain and branded pack conditions."

See response to point 1. The incorrect image was uploaded during submission – all pack images displayed the same health warning label in the survey.

13. "Page 6, Design. The dates of data collection include the period when smokers could have already been exposed to and purchased plain packs. In the limitations section, the authors should consider mentioning any effects they might expect from prior exposure to the policy. For example, that might explain why people did not rate packs as different for some measures."

The overlap of survey period and plain packaging policy roll-out phase has been acknowledged in the limitations section, with a brief reference to expected effects this prior exposure may have had on study results, (see page 14, line 33-40).

14. "Page 6, line 32. Suggest eliminating the phrase "of a number of"

This phrase has been deleted (see page 6, line 38).

15. "Page 7, line 40-42. Correct this if the textual statement was different for plain vs. branded packs. The authors should also clarify that the same image was used for all packs and that it comes from the 1st round or warnings implemented since 2006 (or 2007, if it is from the second set of the first round)."

It has been clarified that the health warning labels on each pack displayed the same graphic images. Additionally, it is now explained that this health warning label first appeared on Australian packs as part of the National Tobacco Campaign from 2006 (see page 7, line 48-52).

16. "Page 7, outcome measures. To make it easier for the reader, I suggest consolidation of information on these measures to the measurement section, including discussion about combining indicators (i.e., instrument evaluation on page 8, line 49 to page 9, line 8) and alpha results described in the results section (i.e., page 10, 16-24)."

As suggested by the reviewer, all information on the outcome measures has been consolidated in the measurement section, (see page 8, line 25-47).

17. "Page 7, outcome measures. Suggest using correlations when describing scales that combine two items, and alpha when combining 3 or more items, which is more standard practice."

If the reviewer's suggestion is to provide spearman correlation (non-parametric) when describing scales that combine two items, we are not sure how this is preferable to the Cronbach's alpha we have used and seek further advice.

18. "Page 7, outcome measures. Can the authors cite any information on validation of the Wakefield et al measures, including when items are combined in the ways they are combined in this study?"

The items are combined in the current study in the same way they are combined in the Wakefield et al. study. We cannot cite any further validation information, and this limitation has been added to the discussion section of the manuscript (page 14, line 11-17).

19. "Page 7, outcome measures. Also, column headings for Table 1 indicate that responses involved a visual analogue scale, which implies that people indicated where they fell between two extremes without reference to any numbers. From the description of the measures, it seems that there were 7 numbered response options, with verbal anchors used at both extreme ends of the response options. If so, then this is not a visual analogue scale.

References to "visual analogue" scale have been removed from Table 1.

20. Also, I think that Table 1 could be eliminated without any loss of information."

In order to include individual rating items in the results, our preference is to include Table 1 in some way either in the manuscript or online as supplementary material.

21. "Page 8, Purchase intentions. Can the authors provide any citations showing the validity of purchase intention tasks like this, where people are given two pack alternatives and a "no purchase" option? Does this predict purchase behavior? If not, then this limitation should be mentioned in the discussion."

This limitation has been acknowledged in the manuscript (see page 14, line 11-17).

22. "Page 8, sociodemographic variables. The income and education questions categories used would ideally be mentioned, especially given the focus on this socioeconomically deprived population."

The income and education categories are presented in Table 2. Providing the categories for each of

the individual sociodemographic variables in the method section would be repetitive.

23. "Page 9, analysis. Why not conduct statistical tests of the sample characteristics across conditions? They look generally similar, but there are some for which statistically significant differences are likely (e.g., higher education and income for Plain B&H), which may help explain some of the discrepant results by brand. If this is true, then randomization did not completely work, and a modeling approach may need to be considered that allows for assessing main effects by condition and statistical controls for characteristics that differ across groups. I believe that the Kruskal-Wallis approach allows for statistical controls, but the authors should consult a statistician to consider options. If there are no non-parametric analogues to multivariate linear regression, then the authors should consider a standard linear regression approach, perhaps as a sensitivity analysis. Unless the authors have a hypothesis based on prior literature regarding differences in plain packaging impact by brand popularity, I think the paper would be stronger if it tested the plain pack condition as a main effect, and included an interaction to test whether brand type matters. I realize that the authors' concerns about power, but I think this is a stronger approach than the exploratory, data driving approach that is currently taken (i.e., page 9, lines 16-20)."

Author Catherine d'Este is a statistician and a Professor of Biostatistics at the University of Newcastle. Professor d'Este advised on all aspects of the manuscript data analysis, and during that time many of the suggestions raised by the reviewer were considered.

It is not appropriate to undertake formal statistical tests to determine whether there are differences in baseline characteristics among groups randomised in a trial. Since we randomise individuals, we expect baseline characteristics to be evenly distributed among groups; any imbalance is due to chance. A formal statistical test which shows that there are "statistically significant" differences in characteristics amongst groups means that any differences are greater than we would expect by chance alone. However, because of randomisation, we already know that any differences are due to chance; thus formal hypothesis testing does not make sense.

Additionally, as we should not be undertaking any hypothesis tests, the decision to include any covariates in the analysis should be determined a priori, and generally written into the study protocol; anything else is a post hoc analysis. As we did not specify any variables to include in our study design there is concern about including now.

24. "Page 10, Results. As described earlier, the authors should test for differences in sample characteristics across conditions to confirm adequate randomization."

Please see response to Point 23 above.

25. "Page 10, line 57-59. Were data on manufactured vs. roll your own collected? If so, these percentages should be included in Table 2 and could potentially be treated as control variables, as people who use roll your own may have different responses to plain manufactured packs."

Data on manufactured cigarettes vs. roll-your own tobacco was collected and has been added to Table 2 (highlighted text). In reference to using this variable in further analysis, please see response to Point 23 above.

26. "Page 11, purchase intent. As with the primary pack perception variables, it would be stronger if the model included statistical controls, particularly for those variables that differed across conditions."

Please see response to Point 23 above.

27. "Page 11, Discussion 2nd paragraph. The authors may want to emphasize that the apparently stronger effects for plain packs with more familiar brands may be what you want, as those are the ones that people are more likely to smoke. They authors may consider an interaction between condition and whether their usual brand was the same as the one shown vs not. This would help make a stronger case for that argument. Pooling data across the two brands may help provide the power needed for this kind of assessment."

The second paragraph of the discussion has been revised to emphasise the positive finding of stronger effects for plain packs with brands that are more familiar to the individual smoker (see page 12, line 3-33). In regards to the reviewer's suggestion concerning additional analysis, see response to Point 11.

28. "Page 12, lines 9 -13. Is there prior evidence for stronger effects among youth compared to adults? This may help explain why effects on perceived harm were not found for this older population."

This consideration has been clarified in the text (see page 12, line 46-48).

29. "Page 13, strengths and limitations. The authors should consider the modality of stimulus presentation (computer image instead of actual pack) as a potential limitation. However, experiments that use real plain packs (e.g., Thrasher et al 2011) or real packs with warning labels (Thrasher et al. Cancer Causes and Control. 2012) provide results that are generally consistent with those that present packs on computer screens (e.g., Hammond et al. Cancer Causes and Control. 2012)."

As suggested by the reviewer stimulus presentation modality has been listed as a potentially limiting factor in the limitations section of the manuscript (see page 14, line 8-11).

We hope you find these modifications and explanations satisfactory. We look forward to publishing in BMJ Open.

Regards,
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References

1. Anderson S, Hastings G, MacFadyen L. Strategic marketing in the UK tobacco industry. *The Lancet Oncology*. 2002;3(8):481-6.

VERSION 2 – REVIEW

REVIEWER	Phil Gendall University of Otago New Zealand
REVIEW RETURNED	07-Jan-2014

GENERAL COMMENTS	<p>The authors have responded to the issues I raised in my original review and I would be happy to see the paper published. As I mentioned, the research has been overtaken by events, but the paper is nicely done, the population studied is of high importance in tobacco control, and the results provide some insight into the way in which plain packaging influences smokers.</p> <p>One minor editorial comment. The second to last sentence in the Strengths and Limitations section should read: "Finally, the last two to three months of the survey ..."</p>
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