PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Waterpipe dependence in university students: effect of normative beliefs
AUTHORS	Salameh, Pascale; Salamé, Joseph; Waked, Mirna; Barbour, Bernadette; Zeidan, Nina; Baldi, Isabelle

VERSION 1 - REVIEW

REVIEWER	Mohammed Jawad Imperial College London United Kingdom
REVIEW RETURNED	02-Dec-2013

GENERAL COMMENTS	This paper is about waterpipe smoking dependence. Although this has been explored previously, assessing waterpipe dependence among university students is novel, and this is the rationale for the study. The introduction is sound and summarises the existing literature appropriately. There are no obvious methodological flaws, and limitations were justified (e.g. non-random sample). Statistical analyses are appropriate and well-described. Results are well-structured and easy to follow. The discussion is insightful and the conclusion is a valid reflection of the study's findings. Waterpipe smoking is common in this population group and is smoked regularly with 2/3 having no intention to quit. The LWDS, previously derived from a slightly different population, is similarly applicable to this sample, and is a validated tool for waterpipe dependence. I have highlighted some concern about the regression analysis, which the authors may wish to address, otherwise my comments are largely minor. The parallel comparison to cigarettes is particularly interesting. I am sure this paper will be a valuable addition to the literature as the lack of information on waterpipe dependence appears to be stagnating the public health importance ascribed to waterpipe. Major comments - The results are lacking sample characteristics – this could be briefly included as text or incorporated into Table 2 as an extra column, and would make interpreting the results more meaningful (e.g. the first thing that struck me was the unknown prevalence of divorced/widowed university students in Lebanon) - I understand the concept of backward stepwise regression, however I am somewhat surprised that gender was not left

	in the multivariate analysis as a constant, especially for current waterpipe smoking. The results are extremely interesting but are not controlled for gender (or higher age class, for that matter), and I feel the model should be re-run but gender (and/or higher age class) be left in as constants. Minor comments - Page 2, Background: The objective of the study is slightly ambiguous and readers may be more informed if the authors considered rephrasing the objective to: “the objective of this study was to measure the correlates, including normative beliefs, associated with waterpipe and cigarette prevalence and dependence.” - Page 3, Line 6: “among youth of Lebanon” – the authors may wish to make this statement more clear i.e. “among young people and university students”, to which the citations refer - Page 3, Line 12: Akl’s review only found significance for lung cancer (not “different types of cancer”) – please review this sentence. - Page 6, Line 19: Reference 24 does not appear to cite any work from “Primack and collaborators” – please review this sentence. - Define the terms ‘current waterpipe’ and ‘current cigarette’ in the methodology (e.g. past 30-day smoking), and whether questions were specifically measuring waterpipe tobacco use (or could students have answered to smoking herbal waterpipe?) - Table 2: What was used to measure socioeconomic status? The authors should perhaps give this variable a footnote to help the table stand alone. - Table 4: There are some variables that have p-values > 0.05 – these should be removed as they are not statistically significant, as described in the methodology - Table 4: Were the LWDS-11 and YACD scores normally distributed among smokers? If so, this should be stated in the methodology. If not, the authors should consider re-running the linear regression models having performing a logarithmic transformation on LWDS-11 and YACD scores (to normally distribute them, which would enhance the model). - Page 12, final line: “spoil cigarette smoking cessation” could be replaced with “interact with cigarette smoking cessation”, which may reflect a broader sensitivity towards the concept of harm reduction for those who support it

REVIEWER	Tarek Tawfik Amin Public Health and Community Medicine, Faculty of Medicine, Cairo University
REVIEW RETURNED	11-Dec-2013

GENERAL COMMENTS	Abstract: Line 34-39: *Although the role of parents was not visible in decreasing the risk of smoking WP, their protective influence seemed more important on WP dependence, a more deleterious behavior.* This statement is confusing and should be rephrased in a simpler fashion. What the rationale for the authors to judge the parents' behavior as deleterious in the second case of dependence rather than the smoking behavior itself? This needs clarification in abstract and in the conclusion section. Line 44: I think that there are intensive and sometime discriminating anti-cigarettes smoking on the global level, so no need to mention them in the conclusion section, rather than mentioning that most anti-smoking efforts are directed chiefly to cigarette neglecting waterpipe and other form of tobacco use. So consider rephrasing of the first sentence, furthermore, initiate you conclusion to reflect your findings instead of recommendations which should be properly cited elsewhere in the MS. Introduction: Page 3, Line 29: starting from in this score to and young adulthood. This section should be placed in the methods section as It did not add nothing to the introduction instead it may confuse the reader, please consider re-placement. Page 3, Line 40: remove the word 'in fact' In many places the authors mentioned drivers and motives for smoking behavior, please clarify the existence of the difference if any and these vocabulary should be consistent all through the MS. Page4, line 9: For cigarette dependence, earlier onset of monthly cigarette smoking, heavier overall consumption and peers' smoking were associated with higher nicotine dependence in Saudi young students [20]. Here what do you mean by monthly cigarette smoking? Page 4, line 17: No studies have been conducted to our knowledge regarding WP dependence. I think that the previous statement is incomplete; in addition to you need to mention the scarcity of studies that tackled the effect of normative belief in Lebanon and even in the Middle East.

	Page 4, line 21: It would be very solid if the authors try to formulate a hypothesis about effect of normative beliefs on nicotine dependence. Methods: How the authors calculated the sample size? What were the total populations? How many universities were included? Page 6, line 5: I think the authors mentioned the LWDS-11 rather than the YACD, composed of 16 items and loaded over six factors, while they mentioned the four loaded factors for LWDS-11, revision is needed. Page 6, line 13: how the normative belief responses were scored? Statistical analysis: Page 6, line 23: data entry was performed by independent lay persons. What were the qualifications of these persons and what do meant by lay persons, was there any inter-rater agreement, was the process of data entry checked for errors and by whom and how? In factor loading using principal component analysis, why the authors used cutoff of .30 for retention? Have the authors test their data for normality? Page 7, line 17: other forms of smoking and normative beliefs as independent variables, what were the other forms encountered? Results: Page 8, line 10: Redundant statement with repetition: Among WP smokers, 760 (97.6%) answered to all questions of the LWDS-11 scale. Among cigarette smokers, 595 (91.7%) answered to all questions of the YACD scale. Page 9: in reporting Cronback's alpha no zero (would be .77 rather 0.77). What was the rationale for loading the income with the psychological instead the physiological dependence, this step may inflate the squared loading and may falsely increase the amount of variance explained? This may give a room to violate the LWDS-11 by other validity studies (in the future). Page 10, line 37: Waterpipe dependence was significantly higher in widow or divorced individuals (p=0.006), and among individuals who have higher numbers of smokers at home (p<0.001) (table 2) Neither the results section nor table 2 gave the number of those divorced/widowed!! I think it would be very informative if the authors

	depict such information to better understand their findings. Overall, there are numerous P values; some of the data shown in tables were repeated in the results section. Page 10, line 47: We note that performing the analysis on dual smokers gave similar results of dependence correlates for both WP and cigarettes, except for a visible association of dual dependence (OR=5.10[2.83; 9.19]; p<0.001), confusing statement to be rewritten and consider the past sentence all through your MS in the results and methods sections. Footnotes for all tables missed the type of statistical tests used, abbreviations used should be mentioned. Discussion: Please start this section with your research question. The MS was primarily focusing on WP dependence rather LWDS-11 validity. Page The explanation given by the authors regarding the shifting of the income item from the physiological to psychological was not convincing.

VERSION 1 – AUTHOR RESPONSE

Reviewer Name Mohammed Jawad
Institution and Country Imperial College London
United Kingdom
Please state any competing interests or state 'None declared': None declared

This paper has been submitted by authors well-published in the field of waterpipe dependence. They present interesting and novel results and its likely this paper will add to the existing literature. I do hope they address my concerns regarding the regression analysis, as this forms a critical part of the paper. There are some minor grammatical errors that I'm sure will be picked up by typesetters.

Summary
This paper is about waterpipe smoking dependence. Although this has been explored previously, assessing waterpipe dependence among university students is novel, and this is the rationale for the study. The introduction is sound and summarises the existing literature appropriately. There are no obvious methodological flaws, and limitations were justified (e.g. non-random sample). Statistical analyses are appropriate and well-described. Results are well-structured and easy to follow. The discussion is insightful and the conclusion is a valid reflection of the study's findings.
Waterpipe smoking is common in this population group and is smoked regularly with 2/3 having no intention to quit. The LWDS, previously derived from a slightly different population, is similarly applicable to this sample, and is a validated tool for waterpipe dependence. I have highlighted some concern about the regression analysis, which the authors may wish to address, otherwise my comments are largely minor. The parallel comparison to cigarettes is particularly interesting. I am sure this paper will be a valuable addition to the literature as the lack of information on waterpipe dependence appears to be stagnating the public health importance ascribed to waterpipe. Thank you.

Major comments
- The results are lacking sample characteristics – this could be briefly included as text or incorporated into Table 2 as an extra column, and would make interpreting the results more meaningful (e.g. the first thing that struck me was the unknown prevalence of divorced/widowed university students in

Lebanon): Ok, done. We added the whole sample characteristics to the table 2 for descriptive purposes.

- I understand the concept of backward stepwise regression, however I am somewhat surprised that gender was not left in the multivariate analysis as a constant, especially for current waterpipe smoking. The results are extremely interesting but are not controlled for gender (or higher age class, for that matter), and I feel the model should be re-run but gender (and/or higher age class) be left in as constants. Ok, done; we forced gender and age class into the model; results were of course essentially unchanged, because these factors were previously removed from the analysis due to non significance.

Minor comments
- Page 2, Background: The objective of the study is slightly ambiguous and readers may be more informed if the authors considered rephrasing the objective to: "the objective of this study was to measure the correlates, including normative beliefs, associated with waterpipe and cigarette prevalence and dependence." Ok, done.
- Page 3, Line 6: "among youth of Lebanon" – the authors may wish to make this statement more clear i.e. "among young people and university students", to which the citations refer. OK, done.
- Page 3, Line 12: Akl's review only found significance for lung cancer (not "different types of cancer") – please review this sentence. Ok, done.
- Page 6, Line 19: Reference 24 does not appear to cite any work from "Primack and collaborators" – please review this sentence. Ok, done.
- Define the terms 'current waterpipe' and 'current cigarette' in the methodology (e.g. past 30-day smoking), and whether questions were specifically measuring waterpipe tobacco use (or could students have answered to smoking herbal waterpipe?). Ok, done; just to mention that, based on previous epidemiological studies, we know that herbal waterpipe is not used in Lebanon.
- Table 2: What was used to measure socioeconomic status? The authors should perhaps give this variable a footnote to help the table stand alone. Ok, done.
- Table 4: There are some variables that have p-values > 0.05 – these should be removed as they are not statistically significant, as described in the methodology: this is in contrast with what you are asking in the major comments; in fact, gender and age class were sometimes removed by the stepwise procedure because of no significant association, and we forced them back on your demand (although they are far from significance); as for the factors that were kept by the stepwise procedure, they could not be removed because this would affect the validity of the model.
- Table 4: Were the LWDS-11 and YACD scores normally distributed among smokers? If so, this should be stated in the methodology. If not, the authors should consider re-running the linear regression models having performing a logarithmic transformation on LWDS-11 and YACD scores (to normally distribute them, which would enhance the model). Both variables were normally distributed; we clarified this in the methods section.
- Page 12, final line: "spoil cigarette smoking cessation" could be replaced with "interact with cigarette smoking cessation", which may reflect a broader sensitivity towards the concept of harm reduction for those who support it. Ok, done.

Reviewer Name Tarek Tawfik Amin
Institution and Country Public Health and Community Medicine, Faculty of Medicine, Cairo University.
Please state any competing interests or state 'None declared': None declared

Abstract:
Line 34-39: Although the role of parents was not visible in decreasing the risk of smoking WP, their protective influence seemed more important on WP dependence, a more deleterious behavior. This statement is confusing and should be rephrased in a simpler fashion. What the rationale for the authors to judge the parents' behavior as deleterious in the second case of dependence rather than the smoking behavior itself? This needs clarification in abstract and in the conclusion section. Ok, clarified in the abstract and the discussion.

Line 44: I think that there are intensive and sometime discriminating anti-cigarettes smoking on the global level, so no need to mention them in the conclusion section, rather than mentioning that most anti-smoking efforts are directed chiefly to cigarette neglecting waterpipe and other form of tobacco use. So consider rephrasing of the first sentence, furthermore, initiate you conclusion to reflect your findings instead of recommendations which should be properly cited elsewhere in the MS. Ok,

conclusion was changed.

Introduction:
Page 3, Line 29: starting from in this score to and young adulthood. This section should be placed in the methods section as It did not add nothing to the introduction instead it may confuse the reader, please consider re-placement. Ok, done.
Page 3, Line 40: remove the word 'in fact' Ok, done.
In many places the authors mentioned drivers and motives for smoking behavior, please clarify the existence of the difference if any and these vocabulary should be consistent all through the MS. Ok, done.
Page4, line 9: For cigarette dependence, earlier onset of monthly cigarette smoking, heavier overall consumption and peers' smoking were associated with higher nicotine dependence in Saudi young students [20]. Here what do you mean by monthly cigarette smoking? Clarified; it is "at least once a month".

Page 4, line 17: No studies have been conducted to our knowledge regarding WP dependence. I think that the previous statement is incomplete; in addition to you need to mention the scarcity of studies that tackled the effect of normative belief in Lebanon and even in the Middle East. OK, done.
Page 4, line 21: It would be very solid if the authors try to formulate a hypothesis about effect of normative beliefs on nicotine dependence. Yes; the objective statement was changed according to the suggestion of the first reviewer. It is now much clearer.

Methods:
How the authors calculated the sample size? What were the total populations? How many universities were included? All these details were included in another descriptive publication [3]; these details were added back here.
Page 6, line 5: I think the authors mentioned the LWDS-11 rather than the YACD, composed of 16 items and loaded over six factors, while they mentioned the four loaded factors for LWDS-11, revision is needed. Ok, done.

Page 6, line 13: how the normative belief responses were scored? They were scored on Likert scale questions (0 indicates strongly disagree; 1, disagree; 2; maybe; 3, agree; and 4, strongly agree); these were further grouped into (yes, maybe and no) in bivariate analysis.

Statistical analysis:
Page 6, line 23: data entry was performed by independent lay persons. What were the qualifications of these persons and what do meant by lay persons, was there any inter-rater agreement, was the process of data entry checked for errors and by whom and how? Details were added.

In factor loading using principal component analysis, why the authors used cutoff of .30 for retention? Sorry, there was an error here; it should be 0.4, as usual.
Have the authors test their data for normality? Yes, and this was added to the methods; both scales were normal.
Page 7, line 17: other forms of smoking and normative beliefs as independent variables, what were the other forms encountered? We meant to look at the effect of cigarette smoking on WP smoking and vice versa; this was clarified.

Results:
Page 8, line 10: Redundant statement with repetition: Among WP smokers, 760 (97.6%) answered to all questions of the LWDS-11 scale. Among cigarette smokers, 595 (91.7%) answered to all questions of the YACD scale. Ok, arranged.

Page 9: in reporting Cronback's alpha no zero (would be .77 rather 0.77). Ok, corrected.

What was the rationale for loading the income with the psychological instead the physiological dependence, this step may inflate the squared loading and may falsely increase the amount of variance explained? This may give a room to violate the LWDS-11 by other validity studies (in the future). The loading was not made on purpose; these are the results that we obtained, where it seems that income was more adequately loading on the psychological craving factor in university students (while in adults it loaded better on the physiological dependence). This was further discussed.

Page 10, line 37: Waterpipe dependence was significantly higher in widow or divorced individuals (p=0.006), and among individuals who have higher numbers of smokers at home (p<0.001) (table 2). Neither the results section nor table 2 gave the number of those divorced/widowed!! I think it would be very informative if the authors depict such information to better understand their findings. Yes, these results were added to table 2.
Overall, there are numerous P values; some of the data shown in tables were repeated in the results section. Ok, removed.

Page 10, line 47: We note that performing the analysis on dual smokers gave similar results of dependence correlates for both WP and cigarettes, except for a visible association of dual dependence (OR=5.10[2.83; 9.19]; p<0.001), confusing statement to be rewritten and consider the past sentence all through your MS in the results and methods sections. Ok, clarified.

Footnotes for all tables missed the type of statistical tests used, abbreviations used should be mentioned. Ok, done.

Discussion:
Please start this section with your research question. The MS was primarily focusing on WP dependence rather LWDS-11 validity. Ok, done.

Page The explanation given by the authors regarding the shifting of the income item from the physiological to psychological was not convincing. This was further clarified in the discussion; anyway, an clear-cut explanation for this could only be given by qualitative studies; this was also added.

VERSION 2 – REVIEW

REVIEWER	Mohammed Jawad Imperial College London United Kingdom
REVIEW RETURNED	25-Dec-2013

- The reviewer completed the checklist but made no further comments.

REVIEWER	Tarek Tawfik Amin Public Health, Cairo University
REVIEW RETURNED	04-Jan-2014

GENERAL COMMENTS	Minor revision is required: Page 6 line 16: OR for Odds ratio