PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Do emotions related to alcohol consumption differ by alcohol type? An international cross-sectional survey of emotions associated with alcohol consumption and influence on drink choice in different settings.
AUTHORS	Ashton, Kathryn; Bellis, Mark; Davies, Alisha; Hughes, Karen; Winstock, Adam

VERSION 1 - REVIEW

REVIEWER	Prof. Avijit Hazra Department of Pharmacology Institute of Postgraduate Medical Education & Research (IPGME&R) Kolkata, India
REVIEW RETURNED	14-Apr-2017

GENERAL COMMENTS	1) Please explain why the upper age limit for the respondents is 34 years. You have stated that the age groups studied are the ones commonly studies in this type of research. However, if someone wishes to use this data for health policy planning, they will then be hampered by the lack of generalizability of the data with respect to age.
	2) It is stated that survey respondents were from countries that contributed at least 250 responses. However, Supplementary Table A indicates that some countries had respondent number less than 250 (e.g. Brazil, Ireland, Mexico, Portugal).
	3) In Table 2, because of the large sample size, it is likely that even small differences in proportions will show up as statistically significant by the chi-square test. Hence it is preferable to present the 95% confidence interval values.
	4) In Tables 3, 4 and 5b, limit adjusted odds ratio values to two decimal places.
	5) In Table 5a, proportions compared are paired proportions. Please specify if the chi-square value is from McNemar's test. For the same reason as stated for Table 2, provide the 95% CI values.
	6) Data in Table 3 and Table 4 suggest that heavier drinkers (as assessed by AUDIT score) are more likely to experience all the positive emotions studied as well as the majority of negative emotions, and this seems to be true for all the individual types of drinks. However, you have stated that "… individuals make the assumption that positive emotions associated with drinking particular

types of alcohol such as spirits will outweigh the negative emotions."
This suggestion therefore appears incongruent. It may be a pharmacological effect of alcohol that it conditions the brain to experience emotional states more intensely with greater exposure. Heavier drinkers may be drinking heavily to satisfy the alcohol craving of their conditioned brain rather than with the intention of experiencing any positive emotions or blanking out negative emotions.

7) The conclusion is repeating what has already been stated earlier in the discussion. Please modify to reduce repetition.

REVIEWER	Nicholas Grahame
	IUPUI, United States
REVIEW RETURNED	12-May-2017

GENERAL COMMENTS	This manuscript compiles data from what the authors claim is the "world's biggest drug survey," the Global Drug Survey (GDS). From the survey, the authors ask readers to rate the effects on emotion of consuming different types of alcoholic beverages. Compiling data only from those reporting their age as 18-34, they ask, for example, how consuming white wine vs. distilled spirits affects mood. There are a number of important limitations to this report.
	1. Although the GDS may be the largest drug survey, it isn't much larger than far more reliable instruments, such as the NESARC, with just over 43,000 respondents, and even if it is larger, it isn't clear whether the added n would increase power for important analyses. Most importantly, the GDS polls a self-selected group, folks willing and interested to fill out a survey on-line for no remuneration and without any supervision. Thus, as the authors indicate, these individuals are quite likely to be those with an interest in drugs, and they also probably skew considerably younger than the mean from the nations and populations that they originate from. Thus, the authors remind us that these data "are not useful to support assessment of general population prevalence," but seek to make an argument that these data, simply based on magnitude, are useful for "analysis of specific populations." It is unclear why a data set that cannot be used epidemiologically CAN be used to analyze specific populations, because the information comes from the same place, and the specific populations are a distillation of the larger dataset. Fundamentally, the question remains as to whether the increase in diversity and number of respondents makes up for the complete loss of supervision of data integrity and sampling validity implied in such a project. Unfortunately, based on what the authors have reported, there is simply no way to know.
	2. One way the authors could deal with this conundrum is to compare GDS results to surveys and other instruments with greater internal validity. That they make no attempt at doing so raises important concerns about the reliability of these findings. For example, if on key metrics (such as comorbidity, age and sex prevalence of drinking, or other endpoints) the GDS respondents showed similar patterns to more reliable surveys, readers concerns about the reliability of the present findings might be assuaged. At times, the authors present their findings when they compare

favorably to other data types, for example, the effect of AUDIT score on stimulation achieved after drinking. But they don't discuss other outcomes that do not accord with more reliable instruments. One of the most common findings across laboratory tasks and cultures is that those with a heavy drinking history become tolerant to the sedative effects of alcohol. Yet consulting Table 2 shows clearly that self-reported AUDIT score has no effect on feelings of tiredness after drinking. Since the authors don't cite other's work sufficiently, readers may not understand how unexpected such a finding is, or that it contradicts much other evidence. The fact that such findings show up in these data, indeed, starts to claw at the entire foundation of the purpose of this type of survey.

3. The findings here aren't anchored in any theoretical constructs about the causes of differences in emotional responses to different alcoholic beverages. Of course, all beverages contain alcohol, the primary pharmacological cause of these drinks' effects on the CNS. They may also contain other compounds, but I don't think the authors are arguing that these are mediating the differences between beverages (for example, reliable lab data exist indicating differences in hangover severity between whiskey and vodka related to differences in trace toxins between these drinks). Thus, the differences in emotional effects must primarily relate to expectancies, a contention I think the authors would agree with. But they make no attempt at understanding what, in turn, drives these expectancies, and why there might be nation of origin differences in the pattern of outcomes. There are a number of odd patterns in the data, to wit:

a. Distilled spirits are said to make folks feel sexy, more so than other alcoholic drinks. These feelings are highest in South America. They are very low in Scandinavian countries, where (especially in Norway) distilled spirits are associated with aggression. The authors don't even cite the fact that Scandinavian countries have strict controls on advertising, while those in South America are much looser. Doesn't it seem probable that the consistent use of sexual themes in liquor advertisements drives respondents' expectancies in South America as compared to Norway? If so, what are we actually measuring in these surveys that is useful, other than as marketing tools? Is that explicitly what the authors desire to obtain with these data, and if so, to what end? What, really, is the purpose of these data?

b. Other oddities stand out that aren't discussed here. Two very similar beverages, white wine and red wine, are said to cause very different effects in the drinker. Sixty percent of red wine drinkers say it causes them to be tired, while only 18% of white wine drinkers say this is the case. Such findings are unexpected in the extreme. Might they have to do with the fact that white wine is consumed earlier in the day than red wine, which might be more likely to be consumed with dinner? Without ANY theoretical basis for understanding these data, the reader is rudderless, and tables of findings become difficult to relate to anything scientifically or sociologically relevant.

4. The authors repeatedly use causal language not justified by the methods. For example, the first sentence of the discussion reads: "our study found that different types of alcohol make people feel differently." This statement is unsupportable, because the alcohol wasn't administered by an experimenter in a controlled and blinded setting. The MOST the authors could say is that peoples' BELIEFS about the effects of alcohol are PREDICTED by the type of beverage, in this self-selected sample. Language needs some cleaning up here.

REVIEWER	Anthony Mwinilanaa Tampah-Naah University for Development Studies Ghana
REVIEW RETURNED	14-May-2017

| GENERAL COMMENTS | Abstract

Line 20 - "... red and white wine ...". Apart from the colour of wine, are there any difference in alcoholic volume? If no difference, the authors should just state 'wine' instead of 'red and white wine'. If different, then the statement should be revised to reflect that. See line 40 of Results section, and Table 1

Line 28 - '... sexy and confident ...' I believe these are separate issues. Authors should revise this. In the analysis, they are analysed separately.

Introduction

Authors should be consistent with the usage of 'e.g.' or 'for example'. See line 9 and line 20. The the usage of 'for example' is preferred |

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name
Prof. Avijit Hazra

Comments from Referee

The authors have reported the results of an online multicountry survey to profile emotional states associated with consumption of major varieties of alcoholic drinks and explored associations between these states and sociodemographic variables and alcohol use disorder risk. Despite the limitations of non-probability sampling, this is a commendable effort and the findings from this survey have the potential to help plan public health interventions, taking into consideration drinking patterns, to reduce abuse of alcohol and related health risks. This should apply also to countries not represented in the survey.

The following issues may be noted / clarified to improve the report.

1) Please explain why the upper age limit for the respondents is 34 years. You have stated that the age groups studied are the ones commonly studies in this type of research. However, if someone wishes to use this data for health policy planning, they will then be hampered by the lack of generalizability of the data with respect to age.

We agree with the referee's comment and in fact limiting the age range was undertaken to strengthen the robustness of the effect estimates within a defined age range. In higher age groups in the GDS numbers drop off rapidly and inference would be based on relatively small numbers. However, internationally many alcohol interventions deal with consumption especially in younger people and we feel results presented here are an important consideration for this demographic.

2) It is stated that survey respondents were from countries that contributed at least 250 responses. However, Supplementary Table A indicates that some countries had respondent number less than 250 (e.g. Brazil, Ireland, Mexico, Portugal).

This is an error on our part and the report has been amended to state 200 responses as opposed to 250 responses (Abstract and pg 5 Methods section).

3) In Table 2, because of the large sample size, it is likely that even small differences in proportions will show up as statistically significant by the chi-square test. Hence it is preferable to present the 95% confidence interval values.

Whilst this is true we feel that adding the 95%CIs to Table 2 would take up considerable space yet have little benefit, as these are binary variables with large sample sizes and the 95%CIs are very small. We already include 95% CIs in the adjusted analysis (Table 3). Thus we have not added them to Table 2 at this point but are happy to do so at the editor's instruction .

4) In Tables 3, 4 and 5b, limit adjusted odds ratio values to two decimal places.

The Tables noted above have been amended to reflect this comment.

5) In Table 5a, proportions compared are paired proportions. Please specify if the chi-square value is from McNemar's test. For the same reason as stated for Table 2, provide the 95% CI values.

The footnote of Table 5a states that the chi-square values are from McNemar's test. We feel there is limited value to including the 95% CIs (see point 3) as they are percentages calculated on binary variable with large sample sizes and we have included 95% CIs in the adjusted analyses (Table 5b). For consistency we would prefer not to include them here, but we are happy to so if the editor prefers.

6) Data in Table 3 and Table 4 suggest that heavier drinkers (as assessed by AUDIT score) are more likely to experience all the positive emotions studied as well as the majority of negative emotions, and this seems to be true for all the individual types of drinks. However, you have stated that "… individuals make the assumption that positive emotions associated with drinking particular types of alcohol such as spirits will outweigh the negative emotions." This suggestion therefore appears incongruent. It may be a pharmacological effect of alcohol that it conditions the brain to experience emotional states more intensely with greater exposure. Heavier drinkers may be drinking heavily to satisfy the alcohol craving of their conditioned brain rather than with the intention of experiencing any positive emotions or blanking out negative emotions.

The discussion has been amended to reflect this comment to take into account the affect that, for example, advertising and the media's perception of alcohol can have on the drinker, reminding individuals of the positive emotions which can be perceived to be experienced when drinking alcohol:

pg 18:
"The continued selection of particular types of alcohol with negative emotional outcomes may in part rely on positive emotions being emphasised by almost ubiquitous advertising [26-27] and negative emotions framed as infrequent and largely a result of abuse. he continued selection of particular types

of alcohol as a preferred drink and consumption behaviours may in part rely on the positive emotions being emphasised by almost ubiquitous advertising [26-27]. Product placement and other promotion measures that focus on the positive emotions associated with consumption and often frame negative emotions as infrequent and largely a result of abuse."

7) The conclusion is repeating what has already been stated earlier in the discussion. Please modify to reduce repetition.

The conclusion has been edited to avoid repetition:

pg 19:
" This research adds international evidence to a limited number of studies undertaken on the feelings associated with drinking different types of alcohol and how such relationships may influence what alcohol is being consumed in different settings. Findings show that individuals associate different emotional responses with different alcohol types and identifies variation in such emotions between demographic groups. Feeling positive emotions may in part be related to the promotion of positive experiences by advertising and the media, but the case for experiencing negative emotions is less well founded given that negative emotions are generally not promoted. Emotions experienced could also be related to when the alcohol is drunk, the levels of alcohol within each beverage type and the different compounds found in different drinks. Consequently, this study represents an initial exploration of alcohol's perceived relationship with emotions on an international basis across a large sample of young people. Moreover, alcohol already plays a large part in violence in many countries, but the concept that consumption of different alcohol products may be more likely to result in violence is rarely reflected in public health responses. Results from these analyses can be used by public health bodies to better understand alcohol consumption behaviour and to inform strategies and interventions to promote changes in consumption, particularly amongst heavier drinkers."

Reviewer: 2
Reviewer Name
Nicholas Grahame

This manuscript compiles data from what the authors claim is the "world's biggest drug survey," the Global Drug Survey (GDS). From the survey, the authors ask readers to rate the effects on emotion of consuming different types of alcoholic beverages. Compiling data only from those reporting their age as 18-34, they ask, for example, how consuming white wine vs. distilled spirits affects mood. There are a number of important limitations to this report.

1. Although the GDS may be the largest drug survey, it isn't much larger than far more reliable instruments, such as the NESARC, with just over 43,000 respondents, and even if it is larger, it isn't clear whether the added n would increase power for important analyses. Most importantly, the GDS polls a self-selected group, folks willing and interested to fill out a survey on-line for no remuneration and without any supervision.

Thus, as the authors indicate, these individuals are quite likely to be those with an interest in drugs, and they also probably skew considerably younger than the mean from the nations and populations that they originate from. Thus, the authors remind us that these data "are not useful to support assessment of general population prevalence," but seek to make an argument that these data, simply based on magnitude, are useful for "analysis of specific populations." It is unclear why a data set that cannot be used epidemiologically CAN be used to analyze specific populations, because the

information comes from the same place, and the specific populations are a distillation of the larger dataset. Fundamentally, the question remains as to whether the increase in diversity and number of respondents makes up for the complete loss of supervision of data integrity and sampling validity implied in such a project. Unfortunately, based on what the authors have reported, there is simply no way to know.

Within the paper, we do not make any suggestion that we are measuring the proportions of any population, other than the study sample, that undertake a particular behaviour or hold a particular view. We are however, able with confidence to examine within the sample how answering one question may be related to another. For example, how AUDIT C relates to answers associated with any drink type. This is common in vast amounts of literature where people have not been attempting to describe the behaviour of entire populations, but look at the association between views and behaviours in often an opportunistic sample. For example:

To T, Stanojevic S, Moores G, et al. Global asthma prevalence in adults: findings from the cross-sectional world health survey. BMC Public Health 2012:12(204).

Siegel MB, Tanwar KL and Wood KS. Electronic Cigarettes as a smoking-cessation tool: results from an online survey. American Journal of Preventive Medicine 2011:40(4):472-475.

A strength of the GDS is that it allows initial relationships to be identified and more specific surveys which are perhaps limited for instance only one country (e.g. NESARC) can examine these issues in more details within a more tightly defined respondent group.

The methods and limitations section within the discussion section of the paper have been amended to reflect the above:

pg5:
"While the GDS non-probability methodology does not allow for the assessment of general population prevalence, the GDS sample enables examination of drug and alcohol behaviours and perceptions across age groups, gender, sexual preferences, place of residence, or mental health status within the sample."

pg 20:
"A strength of the GDS is that it allows relationships between alcohol and emotions to be explored within a large, international sample which includes a high proportion of younger age respondents who can be difficult to capture via telephone or face-to-face interviews. This age group corresponds with age groups often studied within this field of research, for example students and adolescents.[5, 15, 28] Using a unique range of questions, the survey data allowed for novel analysis on how groups within the survey population associate emotions with different types of alcohol in different settings. More specific surveys which are perhaps limited for instance to only one country (e.g. the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) in America [29]) can examine these issues in more details within a more tightly defined respondent group."

Pg20:
"The analyses undertaken do not measure the proportions of any population other than the study sample."

2. One way the authors could deal with this conundrum is to compare GDS results to surveys and

other instruments with greater internal validity. That they make no attempt at doing so raises important concerns about the reliability of these findings. For example, if on key metrics (such as comorbidity, age and sex prevalence of drinking, or other endpoints) the GDS respondents showed similar patterns to more reliable surveys, readers concerns about the reliability of the present findings might be assuaged. At times, the authors present their findings when they compare favorably to other data types, for example, the effect of AUDIT score on stimulation achieved after drinking. But they don't discuss other outcomes that do not accord with more reliable instruments. One of the most common findings across laboratory tasks and cultures is that those with a heavy drinking history become tolerant to the sedative effects of alcohol. Yet consulting Table 2 shows clearly that self-reported AUDIT score has no effect on feelings of tiredness after drinking. Since the authors don't cite other's work sufficiently, readers may not understand how unexpected such a finding is, or that it contradicts much other evidence. The fact that such findings show up in these data, indeed, starts to claw at the entire foundation of the purpose of this type of survey.

We have not attempted to compare GDS results to surveys and other instruments with greater reliability. With regards to internal validity, the GDS uses a standard methodology annually, with previous publications referring to validity of the results, for example:

Bellis MA, Quigg Z, Hughes, et al. Harms from other people's drinking: an international survey of their occurrence, impacts on feeling safe and legislation relating to their control. BMJ Open 2015:5(12):e010112.

The reviewer suggest that heavy drinkers become tolerant to alcohol from laboratory tasks, but the GDS captures people's feelings in a range of settings where there are multiple stimuli, and the effects of alcohol are not in isolation which we include reference to in the discussion. The reviewer rightly states that Table 2 shows no relationship between heavier drinking and tiredness. However, this is consistent with the literature. Tolerance would represent consumption of a constant amount of alcohol producing a lesser effect or increasing amounts of alcohol are necessary to produce the same effect. Here, despite dependent drinkers being much higher consumers of alcohol they are showing a negligible increase in tiredness but show very marked increase in all other emotions. This effect is consistent throughout our analyses with the smallest increases being seen in tiredness with increased consumption and in the case of spirits for instance we see lower levels of tiredness reported in those with AUDIT scores of 8 or more than those with AUDIT below 8. We have now edited the text to make this important point and added a reference:

pg8:
"This relationship was especially strong for the emotions of aggression, whereas the increase in tiredness was negligible"
"Odds of reporting all emotions except tiredness increased with AUDIT score category, in particular feelings of aggression (Table 3)."

pg18:
"Conversely, relationships between tiredness and drinking pattern were negligible and for some drink types (spirits, white wine) heavier drinkers were less likely to report feelings of tiredness. These results are consistent with existing evidence on heavy drinking and alcohol dependence, including the development of tolerance to the sedative effects of alcohol.[22-23]."

3. The findings here aren't anchored in any theoretical constructs about the causes of differences in emotional responses to different alcoholic beverages. Of course, all beverages contain alcohol, the primary pharmacological cause of these drinks' effects on the CNS. They may also contain other compounds, but I don't think the authors are arguing that these are mediating the differences between beverages (for example, reliable lab data exist indicating differences in hangover severity between whiskey and vodka related to differences in trace toxins between these drinks). Thus, the differences in emotional effects must primarily relate to expectancies, a contention I think the authors would agree with. But they make no attempt at understanding what, in turn, drives these expectancies, and why there might be nation of origin differences in the pattern of outcomes. There are a number of odd patterns in the data, to wit:

a. Distilled spirits are said to make folks feel sexy, more so than other alcoholic drinks. These feelings are highest in South America. They are very low in Scandinavian countries, where (especially in Norway) distilled spirits are associated with aggression. The authors don't even cite the fact that Scandinavian countries have strict controls on advertising, while those in South America are much looser. Doesn't it seem probable that the consistent use of sexual themes in liquor advertisements drives respondents' expectancies in South America as compared to Norway? If so, what are we actually measuring in these surveys that is useful, other than as marketing tools? Is that explicitly what the authors desire to obtain with these data, and if so, to what end? What, really, is the purpose of these data?

b. Other oddities stand out that aren't discussed here. Two very similar beverages, white wine and red wine, are said to cause very different effects in the drinker. Sixty percent of red wine drinkers say it causes them to be tired, while only 18% of white wine drinkers say this is the case. Such findings are unexpected in the extreme. Might they have to do with the fact that white wine is consumed earlier in the day than red wine, which might be more likely to be consumed with dinner? Without ANY theoretical basis for understanding these data, the reader is rudderless, and tables of findings become difficult to relate to anything scientifically or sociologically relevant.

The reviewer raises a number of interesting areas to consider. We agree that whilst these need to be explored, to be comprehensive much greater between and within country information is needed to explore these relationships, cultural differences in attitudes and expectations from alcohol, legislation and marketing.

The purpose of this study was an initial exploration of the relationships between perceived emotions and alcohol consumption. We have amended the discussion to acknowledge that this raises a number of questions, and that further research is needed to understand the additional affects of differences in alcohol volume between drinks, when drinks are consumed, the effect of alcohol advertising, other compounds with different beverages and the mixers used when consuming different types of drinks:

pg19:
" This study is an initial exploration to understand the relationships between perceived emotions and alcohol consumption. Further research is required into why people choose to consume specific drink types in different settings, their mood prior to drinking, drinking patterns including combination of drinks consumed on individual occasions, differences in alcohol volume, mixers consumed with drinks and the effect of alcohol advertising on the perceived mood of drinkers."

Response to point a. - We have amended the report to make it clear as to why comparisons between countries are not relevant due to the sample, but have included one sentence of interest which compares the country samples. The text explicitly states that caution should be used when interpreting these results due to the limitations of our sampling methods:

pg8:
" Differences in emotions were also reported by respondents from different countries with the highest association with the positive emotions of feeling energised, relaxed and sexy being the South American sample of Colombia and Brazil. For negative emotions, the country sample with the strongest association with aggression when drinking alcohol was Norway and feeling restless was France (online supplementary table B). However, caution must be taken when interpreting these results due to the limitations of the sample for each country."

Response to point b. The terms red and white wine were used in the survey and because of the differences in results between these groups we considered it important to report these results separately rather than collectively for wine. We were also surprised by this result and as the reviewer rightly points to these results reflect individuals beliefs about the effect of alcohol. We have added a sentence to reflect this.
Page 19 . "The reported emotions for wine differed, with red wine drinkers more likely to report tiredness than white wine drinkers. Within the limits of the GDS it is not possible to explore this further, whether due to drinking at specific times of day or the expected effect specific alcoholic drinks, influenced by culture or marketing. "

4. The authors repeatedly use causal language not justified by the methods. For example, the first sentence of the discussion reads: "our study found that different types of alcohol make people feel differently." This statement is unsupportable, because the alcohol wasn't administered by an experimenter in a controlled and blinded setting. The MOST the authors could say is that peoples' BELIEFS about the effects of alcohol are PREDICTED by the type of beverage, in this self-selected sample. Language needs some cleaning up here.

Report has been amended to reflect this comment.

Reviewer: 3
Reviewer Name
Anthony Mwinilanaa Tampah-Naah

Please leave your comments for the authors below
Abstract
Line 20 - "... red and white wine ...". Apart from the colour of wine, are there any difference in alcoholic volume? If no difference, the authors should just state 'wine' instead of 'red and white wine'. If different, then the statement should be revised to reflect that. See line 40 of Results section, and Table 1

We have used the term red and white wine as this is what was asked about in the survey. We did not ask about Rose wine so did not want to use the overarching term of wine. We did not account for any differences in alcoholic volume of wine and have noted this as a limitation in the discussion with the need for further exploration in future research:

pg19:
" This study is an initial exploration to understand the relationships between perceived emotions and alcohol consumption.

Further research is required into why people choose to consume specific drink types in different settings, their mood prior to drinking, drinking patterns including combination of drinks consumed on individual occasions, differences in alcohol volume, mixers consumed with drinks and the effect of alcohol advertising on the perceived mood of drinkers."

Line 28 - '... sexy and confident ...' I believe these are separate issues. Authors should revise this. In

the analysis, they are analysed separately.

Report amended to reflect this comment:

Abstract:
"Positive and negative emotions associated with consumption of different alcoholic beverages (energised, relaxed, sexy, confident, tired, aggressive, ill, restless and tearful) over the past 12 months in different settings."

Introduction
Authors should be consistent with the usage of 'e.g.' or 'for 'for example'. See line 9 and line 20. The the usage of 'for example' is preferred

Report amended to reflect this comment (Pg 4).

VERSION 2 – REVIEW

REVIEWER	Prof. Avijit Ha Department of Pharmacology Institute of Postgraduate Medical Education & Research Kolkata India zra
REVIEW RETURNED	22-Jul-2017

GENERAL COMMENTS	The authors have addressed the concerns of this reviewer satisfactorily.

REVIEWER	Nicholas Grahame IUPUI, Indianapolis, IN USA
REVIEW RETURNED	03-Jul-2017

GENERAL COMMENTS	The new text helps readers contextualize this survey data better.

REVIEWER	Anthony Mwinilanaa Tampah-Naah University for Development Studies Ghana
REVIEW RETURNED	18-Jul-2017

GENERAL COMMENTS	The manuscript requires proofreading before it can be published. Authors should avoid abbreviation of words