Article details	
Title	The consumption of alcohol mixed with energy drinks: prevalence and key correlates among Canadian high school students
Authors	Sunday Azagba PhD, Don Langille MD MHSc, and Mark Asbridge PhD
Reviewer 1	**Mary Claire O'Biren**
Institution	Wake Forest School of Medicine, Winston-Salem, NC
General comments	Important topic and a national prevalence study; results are worthy of dissemination/ publication. However there are problems in this manuscript with the use of cited references (see comments). Page 4 Line 42-47: The cited reference (Ferreira 2006) does not support that statement at all. Page 4 Line 47-49: The cited reference (Miller 2012) does not support AmED and cardiac arrest. Page 4 Line 49-50: The sentence as worded suggests that AmED has a protective effect ("… found to reduce risk-taking…") but the referenced study involves self-report retrospective behavior; causality cannot be inferred. Page 7 Line 55: "AmED use was higher for those in higher grades and in those who were older." This appears to contradict the statement on page 9 line 13: "AmED use was increased for students who were younger…" This disparity is clarified on page 10 lines 23-33. Earlier statements should be clarified to reflect prevalence and adjustment for risk factors. Page 10 Line 6-15: The citation (#18) should appear after the words "…but not varsity athletes…", not at the end of sentence. The supposition regarding 2 types of athletes does not appear as a conclusion of the original (cited) authors and so should not be construed as such. Page 10 Line 37-40: For the non-Canadian reader, it would be helpful to clarify the drinking age in the provinces studied, there being no federally defined drinking age. Page 10 Line 40: Should read, "…where it has remained for the last 20 years." (not: "next 20 years") Page 11 Line 44-51: ("Given that individuals….") The cited reference (Ferreira 2006) does NOT support that statement. Page 12 Line 5: The FDA citation regarding the ban of pre-mixed drinks is should be original (fda.gov) and should not cite a newspaper (reference #34) Instead, use: http://www.fda.gov/food/foodingredientspackaging/ucm190366.htm). In addition, the manuscript's wording is not strictly correct: the FDA did not "ban" the sale of these beverages; that implies the sales were legal in the first place. The FDA warned manufacturers that caffeine was an "unsafe food additive" to alcoholic malt beverages, and said that seizure of the products was possible under federal law. To clarify, caffeine was never "permitted" under US law as an additive to alcohol. Page 16 Line 29: table 1: section of "risk taking behaviours" is more properly, "risk behaviours". (seems confusing to list "nonsmoking" under "risk taking behaviours") Page 18 Table 2: It is not clear to look at the Table how the Models apply or what they stand for; even after reading the text, this is unclear.
Reviewer 2	**Cecile A. Marczinski**
Institution	Psychological Science, Northern Kentucky University, Highland Heights, Ky.
General comments	This research reports that rates of alcohol mixed with energy drinks (AmED) were approximately 20% for Canadian high school students in the past year. The odds of AmED use were higher with younger adolescents. AmED use was associated with other risky behaviors, such as other substance use and school truancy. The authors conclude that AmED consumption is an emerging public health concern that should receive more attention. This research addresses a topic of importance. I agree with the authors that there is a paucity of research examining AmED use in adolescents. Researchers have argued that these beverages would appeal to underage drinkers, yet empirical data is lacking. However, there are some problems with this manuscript in its current form. 1. The major problem with this paper concerns the literature review. The authors have made significant errors in citing the literature. In many cases, they authors make such significant errors that one wonders if any of the authors actually read any of the papers that were cited. Here are some examples:

	Page 4, lines 38-42: “due to increased feelings of alertness produced by caffeine, subjective estimates of alcohol impairment are typically underestimated, while perceived rewarding aspects of drinking are enhanced.” This information comes from laboratory research that is reported in papers by Marczinski & Fillmore (2006) and Marczinski et al. (2011, 2012), not citations 7 - Arria & O’Brien (2011) and 8 – Weldy (2010) which in both cases are just commentaries and not original research. If the authors had actually read the FDA statement which is cited in citation 6, these errors would not have been made. Page 4, lines 43-47: “AmED has been associated with greater risk-taking, impaired driving, higher volumes of alcohol consumption per sitting, increased injury…”. The authors cited 9 – Ferreira. Again none of this information comes from the Ferreira study. Studies by Arria, Brache, Berger, Cheng, Price, Thombs etc. have all carefully documented these relationships. Ferreira was a laboratory study. Page 4, line 49 – citation 10 Miller does not investigate risk of cardiac arrest 2. It is odd to put the Peacock study at the end of a paragraph which discusses risks of AmED. This requires some explanation or comment regarding how this study is inconsistent with the other reports (since Peacock found that AmED was associated with reduced risks). 3. In the Methods or Results, there is no explanation regarding what Models 1 and 2 are referring to. What is going on with these analyses? 4. In the Discussion section, the authors discuss sports participation and how jock identity may differentiate different athletes. There is a highly cited paper by Miller on ‘jock identity and energy drink consumption’ that needs to be cited since this is not the authors’ own ideas (page 10, 1st paragraph). 5. In the Policy implications section, there is again a citation for Ferreira that incorrectly described reports of greater risk taking or being less able to identify symptoms of intoxication. Ferreira only reported decreased dry mouth, headache, weakness and impairment of motor coordination. Other studies have specifically asked subjects about intoxication. Moreover, Ferreira did not collect epidemiological data on injury risk, such as papers written by O’Brien or Arria. 6. The authors should incorporate the recent literature review written by Verster. This review is critical of the approach used by these authors in determining risks associated with AmED and should be discussed somewhere in the paper. 7. The retrieved from link is missing from citation 6 for the FDA news release.
Reviewer 3	**Frederic Nault-Briere**
Institution	University of Montréal, Montréal, Que.
General comments	The reviewed manuscript presents data on the prevalence and correlates of alcohol mixed with energy drink (AmED) consumption among Canadian high school students. This study is relevant and timely: there is currently a paucity of scientific information regarding AmED in adolescents, despite the fact that this substance use behavior is raising growing health concerns among researchers, clinicians, and policymakers. The key strength of the study is the use of the Youth Smoking Survey (YSS), a large nationally representative sample of students from all Canadian provinces and territories (n = 36155). Despite these strong elements (clear relevance, sample strength), I have some concerns about the manuscript in its current form: - I have several comments/questions regarding the models used in the study. First, the two models should be introduced and described in the text. Second, it is unclear to me how reference categories were chosen for nominal variables (e.g. race, province of residence); there may be more interesting ways to obtain pairwise comparisons between all categories than to use one reference category. I think this is especially true for province of residence, which could be explored in a separate analysis. Third, there is a certain redundancy between alcohol use and AmED use (by definition, AmED users have to be alcohol users in the past year) and thus, alcohol use has in different status than other correlates in the model. I think it could be helpful to present model 1 without alcohol use, and then model 2 with alcohol use and discuss the differences (and again, possibly without province of residence which could be explored separately or controlled for but not shown in these analyses and explored in details elsewhere). - The analyses are mostly based on dichotomous measures, which may not be the most informative approach. Correlates are often used with cutoffs that are somewhat arbitrary (e.g. grades) and statistically speaking, continuous scales may represent a better option when available. - I suggest systematically examining gender differences. Research shows that the determinants and outcomes of substance use behaviors often differ by gender and the

	sample size allows exploring these differences. I think such an exploration would better exploit the potential of the YSS data and improve the paper. - One counter-intuitive finding of the study is that the prevalence of AmED use is higher in older adolescents than younger ones, but that young age is associated with increased risk of AmED use than older age in logistic models. The authors suggest that this could be due to the ratio of AmED v. alcohol only users in younger and older adolescents. This is a reasonable explanation, but it would be helpful to provide data more systematically to support this assertion. I suggest present this information in the descriptive part (maybe as additional information in Figure 1). - The implications of missing data and non response to the survey for representativeness should be discussed. - I believe that the second parag. of the discussion (page 9) should focus on explaining why school variables matter for AmED use rather than suggesting strategies for intervention. - Relatedly, as the focus of the paper is on describing AmED use and identifying the risks associated with this use in Canadian students, I think that "policy implications and future research" should focus on drawing specific conclusions from this exercise rather than suggesting general strategies for intervention. - The study would benefit from a linguistic review and more careful editing. Terms should be used in a consistent manner (e.g. alcohol mixed with energy drinks, and not energy drinks mixed with alcohol), there are some grammatical mistakes and incorrect verb tenses in the text, and referencing is not always proper. Minor comments: - At the end of parag 2 in the introduction, the authors mention that one study found AmED to reduce risk-taking relative to the use of alcohol alone. This finding is opposite to the results presented the previous sentences, but is not discussed. It would be helpful to mention a few words to make sense of this discrepancy. - In the last paragraph of the introduction, a more compelling evidence-based argument could be made as to why adolescents are potentially at greater risk of AmED use than adults. Furthermore, although it may seem obvious, explaining why prevalence and correlate data matter could be a valuable addition for less familiar readers. - On page 9 in the discussion, the authors mention that "It is not apparent, however, whether there are systematic variations in terms of energy drink availability, product price, or provincial taxes." It is unclear to me what supports this conclusion. Was this explored in the YSS? - Self-reporting of all data should be added as one of the key limitations of the study. - The term "Prevalence" should be removed from the title of Table 2.
Author response	Editor comments: The response rate appears to have been only 50% for schools. Is that right and perhaps you could comment on this in the discussion. *The response rate for schools was 50% but for students it was 73%. The survey weights account for non-response bias, which we have explained in the Statistical Analysis section.* The measure Yes/No for AMED is a little crude. Do you have more graded responses? *The YSS did not include a graded response for AMED use, only a dichotomous measure.* The analysis should be weighted to reflect the complex sampling. *All analyses were weighted. This was described on page 6, second sentence under the subheading "Statistical analysis"* You should justify categorizing continuous variables as this loses some information. *Our manuscript included only one continuous measure, school connectedness, which we left as continuous in the original submission. There were no other continuous measures in the survey and we did not convert any continuous measures to categorical.* Comments to the Author Important topic and a national prevalence study; results are worthy of dissemination/ publication. However there are problems in this manuscript with the use of cited references (see comments). Page 4 Line 42-47: The cited reference (Ferreira 2006) does not support that statement at all. Page 4 Line 47-49: The cited reference (Miller 2012) does not support AmED and cardiac arrest. Page 4 Line 49-50: The sentence as worded suggests that AmED has a protective effect ("… found to reduce risk-taking…") but the referenced study involves self-report retrospective behavior; causality cannot be inferred. *Reviewers noted concerns about appropriate referencing of arguments. This was an unfortunate error in the original submission where our final submission unintentionally failed to include all of our references. As such, a number of our arguments were under- or poorly*

referenced.
We apologize for this clerical error and have corrected all of these issues.

Page 7 Line 55: "AmED use was higher for those in higher grades and in those who were older." This appears to contradict the statement on page 9 line 13: "AmED use was increased for students who were younger…" This disparity is clarified on page 10 lines 23-33. Earlier statements should be clarified to reflect prevalence and adjustment for risk factors.
The second statement has been clarified to include mention that this finding is based on adjusted model results.

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Corrected.

Page 10 Line 37-40: For the non-Canadian reader, it would be helpful to clarify the drinking age in the provinces studied, there being no federally defined drinking age.
We have included details on legal drinking age.

Page 10 Line 40: Should read, "…where it has remained for the last 20 years." (not: "next 20 years")
Corrected

Page 11 Line 44-51: ("Given that individuals….") The cited reference (Ferreira 2006) does NOT support that statement.
Corrected.

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Thank you for pointing out this clarification. We have edited the manuscript to reflect this point.

Page 16 Line 29: table 1: section of "risk taking behaviours" is more properly, "risk behaviours". (seems confusing to list "nonsmoking" under "risk taking behaviours")
Corrected.

Page 18 Table 2: It is not clear to look at the Table how the Models apply or what they stand for; even after reading the text, this is unclear.
The tables and text have been edited to add further clarity.

Comments to the Author
This research reports that rates of alcohol mixed with energy drinks (AmED) were approximately 20% for Canadian high school students in the past year. The odds of AmED use were higher with younger adolescents. AmED use was associated with other risky behaviors, such as other substance use and school truancy. The authors conclude that AmED consumption is an emerging public health concern that should receive more attention.

This research addresses a topic of importance. I agree with the authors that there is a paucity of research examining AmED use in adolescents. Researchers have argued that these beverages would appeal to underage drinkers, yet empirical data is lacking. However, there are some problems with this manuscript in its current form.

1. The major problem with this paper concerns the literature review. The authors have made significant errors in citing the literature. In many cases, they authors make such significant errors that one wonders if any of the authors actually read any of the papers that were cited. Here are some examples:
Page 4, lines 38-42: "due to increased feelings of alertness produced by caffeine, subjective estimates of alcohol impairment are typically underestimated, while perceived rewarding aspects of drinking are enhanced."
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relationships. Ferreira was a laboratory study.
Page 4, line 49 – citation 10 Miller does not investigate risk of cardiac arrest
See initial comments to Reviewer #1 that explains the citation issue and our corection.

2. It is odd to put the Peacock study at the end of a paragraph which discusses risks of AmED. This requires some explanation or comment regarding how this study is inconsistent with the other reports (since Peacock found that AmED was associated with reduced risks).
This paragraph was removed.

3. In the Methods or Results, there is no explanation regarding what Models 1 and 2 are referring to. What is going on with these analyses?
A third model has been added and an explanation has been provided in the methods and results sections, and in Table 2, to clarify each model.

4. In the Discussion section, the authors discuss sports participation and how jock identity may differentiate different athletes. There is a highly cited paper by Miller on 'jock identity and energy drink consumption' that needs to be cited since this is not the authors' own ideas (page 10, 1st paragraph).
Fixed

5. In the Policy implications section, there is again a citation for Ferreira that incorrectly described reports of greater risk taking or being less able to identify symptoms of intoxication. Ferreira only reported decreased dry mouth, headache, weakness and impairment of motor coordination. Other studies have specifically asked subjects about intoxication. Moreover, Ferreira did not collect epidemiological data on injury risk, such as papers written by O'Brien or Arria.
See comment to reviewer #1 above.

6. The authors should incorporate the recent literature review written by Verster. This review is critical of the approach used by these authors in determining risks associated with AmED and should be discussed somewhere in the paper.

7. The retrieved from link is missing from citation 6 for the FDA news release.
Fixed.

Comments to the Author
The reviewed manuscript presents data on the prevalence and correlates of alcohol mixed with energy drink (AmED) consumption among Canadian high school students. This study is relevant and timely: there is currently a paucity of scientific information regarding AmED in adolescents, despite the fact that this substance use behavior is raising growing health concerns among researchers, clinicians, and policymakers. The key strength of the study is the use of the Youth Smoking Survey (YSS), a large nationally representative sample of students from all Canadian provinces and territories (n = 36155).

Despite these strong elements (clear relevance, sample strength), I have some concerns about the manuscript in its current form:

- I have several comments/questions regarding the models used in the study. First, the two models should be introduced and described in the text. Second, it is unclear to me how reference categories were chosen for nominal variables (e.g. race, province of residence); there may be more interesting ways to obtain pairwise comparisons between all categories than to use one reference category. I think this is especially true for province of residence, which could be explored in a separate analysis. Third, there is a certain redundancy between alcohol use and AmED use (by definition, AmED users have to be alcohol users in the past year) and thus, alcohol use has in different status than other correlates in the model. I think it could be helpful to present model 1 without alcohol use, and then model 2 with alcohol use and discuss the differences (and again, possibly without province of residence which could be explored separately or controlled for but not shown in these analyses and explored in details elsewhere).
As noted to Reviewer #2, we have added more details about the models in the methods and results sections, and in table 2.
With respect to reference categories, we typically selected reference categories of interest (NS for province), and for race we selected the largest group (White). Ultimately, the choice of reference category does not matter as they represent pairwise comparisons between each category and the referent.
We included a new Model 1 that includes all covariates except heavy drinking and province. Model 2 adds in heavy drinking and Model 3 adds in province. Heavy drinking is included because we are interested to see whether it, and not just alcohol use per se, is associated with mixing with alcohol with energy drinks, and whether it shapes associations between respondent age and AmED use.
- The analyses are mostly based on dichotomous measures, which may not be the most informative approach. Correlates are often used with cutoffs that are somewhat arbitrary (e.g. grades) and statistically speaking, continuous scales may represent a better option when available.
As noted above, the survey included only one continuous measure, school connectedness,

which we left as continuous in the original submission. There were no other continuous measures in the survey and we did not convert any continuous measures to categorical. The cutoffs employed generally represent recognized cut-points associated with adolescent risk behaviours elsewhere.

- I suggest systematically examining gender differences. Research shows that the determinants and outcomes of substance use behaviors often differ by gender and the sample size allows exploring these differences. I think such an exploration would better exploit the potential of the YSS data and improve the paper.
We carried out additional stratified analyses based on gender and found little difference in associations between risk behaviours and AmED. We note this in the results section and can include the tables if needed.
- One counter-intuitive finding of the study is that the prevalence of AmED use is higher in older adolescents than younger ones, but that young age is associated with increased risk of AmED use than older age in logistic models. The authors suggest that this could be due to the ratio of AmED v. alcohol only users in younger and older adolescents. This is a reasonable explanation, but it would be helpful to provide data more systematically to support this assertion. I suggest present this information in the descriptive part (maybe as additional information in Figure 1).
The addition of a new Model 1 (without heavy drinking) helps to clarify this effect.
- The implications of missing data and non response to the survey for representativeness should be discussed.
The YSS data weights were derived from a two-stage process, which included adjusting for student non-response.
- I believe that the second parag. of the discussion (page 9) should focus on explaining why school variables matter for AmED use rather than suggesting strategies for intervention.

- Relatedly, as the focus of the paper is on describing AmED use and identifying the risks associated with this use in Canadian students, I think that “policy implications and future research” should focus on drawing specific conclusions from this exercise rather than suggesting general strategies for intervention.
We have tried to both drawn conclusion from the exercise but also provide some sense of policy direction.

- The study would benefit from a linguistic review and more careful editing. Terms should be used in a consistent manner (e.g. alcohol mixed with energy drinks, and not energy drinks mixed with alcohol), there are some grammatical mistakes and incorrect verb tenses in the text, and referencing is not always proper.
We have addressed these issues.

Minor comments:
- At the end of parag 2 in the introduction, the authors mention that one study found AmED to reduce risk-taking relative to the use of alcohol alone. This finding is opposite to the results presented the previous sentences, but is not discussed. It would be helpful to mention a few words to make sense of this discrepancy.
This sentence has been removed.
- In the last paragraph of the introduction, a more compelling evidence-based argument could be made as to why adolescents are potentially at greater risk of AmED use than adults. Furthermore, although it may seem obvious, explaining why prevalence and correlate data matter could be a valuable addition for less familiar readers.
We feel that there is sufficient common knowledge about the adverse effects of alcohol and caffeine alone, on kids, that an explicit point is not needed.
- On page 9 in the discussion, the authors mention that “It is not apparent, however, whether there are systematic variations in terms of energy drink availability, product price, or provincial taxes.” It is unclear to me what supports this conclusion. Was this explored in the YSS?
We have clarified this point to reflect our suggestion that differences in observed AmED use may be reflective of provincial differences in the prices of each product.

- Self-reporting of all data should be added as one of the key limitations of the study.
We have added mention of self-report bias.
- The term “Prevalence” should be removed from the title of Table 2.
Corrected.

Comments to the Author
Well written manuscript on a very important topic, adolescent AmED use. Major methodological strength is the national, representative sample.

In the measures section, in terms of the dependent variable, does "pre-mixed" refer to AmED sold in a can? This was the only question I had. Premixed AmED is banned in the US as of 2010, but not certain if this is the case in Canada.
Pre-mixed refers to mixed and sold in a can. We have clarified this point.