Author's response to reviews

Title: History of Dating Violence and the Association with Late Adolescent Health

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Version: 2 Date: 12 May 2013

Author's response to reviews:

Dear Dr. Silvestre,

Thank you for the opportunity to revise our manuscript for consideration at BMC Public Health. We are especially grateful to the peer reviewers, who provided thorough and thoughtful comments to help strengthen the manuscript and increase its relevance to the field. We wish all peer reviews were as helpful as the ones we received here at BMC Public Health; thank you again. Below is our point-by-point response to the reviewers’ concerns. Additionally, we made changes to the manuscript using yellow highlighted text.

Reviewer's report
Title: Health of Adolescents Exposed to Physical, Sexual and Non-Physical Dating Violence
Version: 1 Date: 22 March 2013
Reviewer: Alison Parkes

Reviewer's report:

Major compulsory revisions
1. A statement concerning ethical approval for the study should be included in Methods, and information on anonymity of information.

Response: Thank you for raising this question. We previously included the following as the first sentence of the Methods section: “Study procedures were approved by the institutional review board of The Ohio State University.” In response to your concern, we added a clause about confidentiality and anonymity protections.

2. Measures of health behaviors p5-6. Please supply more information on original response categories/scales used, and how these were used to create binary
measures.

Response: Thank you; we clarified the response categories in the revised manuscript.

3. P8 Please justify the grouping together of a positive response to any of the first three items in Table 1 (listed under physical/sexual) to form the measure of physical and/or sexual violence. There is a reference to poly-victimisation in the Introduction, although it appears that only a few studies have examined health effects of this. However, it seems that the authors of this paper have opted for such a measure in their own analysis although this has not been clearly stated or reasoned. Also, it would be informative to be able to compare the effects of poly-victimisation (ie physical + non-physical dating violence) with any physical, and any non-physical dating violence.

Response: We understand and appreciate this concern. We were limited in our analysis by the small sample size; this made it challenging to undertake gender-stratified analyses that partitioned violence according to the types of suggestions you are making (e.g., physical+psychological, physical+sexual, etc.). Prior literature within young adult and middle age populations—where the effects of violence have been more extensively studied than they have in adolescent populations—has considered physical/sexual violence together as a single category, and non-physical (psychological) violence only (without concomitant physical/sexual violence) as another category. The idea behind these categorizations is that physical/sexual violence and psychological violence only each have unique associations with health (see Coker et al., 2000; Coker et al., 2002; Bonomi et al., 2006; Bonomi et al., 2007; Dichter, Marcus, Bonomi, under review). We modeled the present analysis after these prior analyses, in an attempt to show that dating violence types including physical/sexual and non-physical only have unique associations with health. To additionally respond to your concern, we changed the language of “poly-victimization” in the Introduction, so as to not give readers the impression that we intended to analyze our data accordingly.

I am most concerned about Item 2 (non-physical coercion to have sex, eg verbal persuasion). As there is no information on the degree of coercion, this could be mild. I would like to know more about the response frequencies to each of the items separately, how they are correlated and what happens if the analysis of physical/sexual violence is restricted to items 1 and 3 only (representing stricter definition of physical violence). I see that the authors are having a separate paper reviewed that may contain some of this information, but nevertheless it would be highly relevant to give more details here – the statement in the Results that “Approximately 44% of subjects had been bullied” is high and we need to
see a breakdown into different categories of abuse, by gender.

Response: We appreciate this concern. In response to both reviewers’ concerns, we included a table of dating violence prevalence, by gender, including the different types of sexual violence. As indicated in the prevalence table, most of the “sexual violence” was, in fact, violence due to verbal pressure rather than physical pressure. This is consistent with what is known about sexual violence—namely, most sexual violence is verbally coerced rather than physically forced. The Centers for Disease Control and Prevention, through The National Intimate Partner and Sexual Violence Survey, set a precedent for asking about sexual coercion in the measurement of sexual violence (see Black et al., 2010). We are cautious about making an assumption that “non-physical coercion to have sex … could be mild.” The experience of verbal sexual coercion can (and often does) have serious traumatic effects for victims (see Brown et al., 2010). And, it has been demonstrated that the experience of sexual coercion increases the likelihood of future sexual coercive experiences (see Young et al., 2012). Unfortunately, within our present dating violence study, we did not have qualitative information about the nature of the violence experienced; we expanded upon this as a limitation in our Discussion section.

4. Discussion – authors should discuss the following (1) Mechanisms explaining poorer sexual and mental health outcomes for victims of abuse, (2) possibility of reverse causation (eg mental health to depression) or third variable causation (eg from childhood problems to young adult risk involvement and depression), (3) particular difficulty over sexual health measures, in not taking account of the likelihood that young adults who have sex more often in casual relationships will likely have more chance of meeting a violent partner.

Response: We are cautious about extrapolating beyond what we are able to do in the present analysis. In the present analysis, we were able to examine the association between dating violence and health, but we were not able to determine temporality or to determine mediating/moderating factors. In response to your concerns, we included language in the Discussion highlighting the limitations of our cross-sectional data, and a very brief section speculating about possible mechanisms between violence exposure and health.

Minor essential revisions
5. Reword last sentence on p3 (“However, the experience of non-physical victimization….”) to insert the word “also” before “associated with adverse mental health….”

Response: We made this revision.

6. P5 Numbers in the 2 studies as shown add up to 559 rather than 585. Please clarify. (Looks as though the first study should be 297 rather than 271 from
figures supplied earlier, at the top of the page – but again there seems to be a mistake adding up 190 and 107 that is puzzling).

Response: Thank you for noting this. We made the revision.

7. P5 Please clarify whether the two questions used from the 9-item Patient Health Questionnaire had the sensitivity to detect depression given, or whether it was in fact the whole 9-item scale.

Response: Thank you; in response to both reviewers’ concerns, we clarified information about the two items from the Patient Health Questionnaire.

8. P6 Change “At less to lose weight” to “Ate less…”

Response: In response to Reviewer 2’s concerns, we deleted the items addressing “ate less to lose weight” and “exercised to lose weight.” Thus, the editing suggestion became obsolete.

9. P6 Clarify what is meant by “based on frequency distributions….” in the paragraph on sexual behaviour measures.

Response: We clarified this in the revised manuscript. For most of the sexual behavior types (e.g., vaginal, oral), five or more partners was the natural cut-point for number of sexual partners based on the frequency distributions. However, anal sex occurred much less frequently and with fewer partners (usually one partner), so the cut-point for anal sex was “had anal sex” versus “did not have anal sex.”

10. P6 last paragraph. Change “to retrospectively assess subjects’ dating violence histories” to “to assess subjects’ dating violence histories retrospectively” (avoids split infinitive)

Response: Thank you; we made this revision.

11. P7 Insert a space before the dash at the end of the fourth line.

Response: We changed this to a comma, rather than a dash, after our editing of the manuscript.

12. P7 The reference to Table 1 in parentheses should come after the citation number referring to Centers for Disease Control and Prevention.

Response: Thank you; we made this revision.

13. P9 Analysis section. Suggest remove the phrase “For the multivariable models,” from the beginning of the third sentence. Likewise remove the phrase “For the generalized linear models,” from the start of the next sentence. The use of these opening phrases is potentially confusing, eg. almost implying that they
are alternative models, which is not the case.
Response: We made these revisions.

14. P10 Multivariable analysis. First sentence: suggest insert a full stop after “adjusted for childhood abuse” and start a new sentence with “We report the results...”
Response: Thank you; we made this revision.

15. Discussion. Please mention lack of information on severity of violence as a limitation – this applies especially to use of non-physical forms of violence such as verbal persuasion.
Response: Excellent point; thank you. We made this revision.

Discretionary revisions
16. I would like to see further analysis of the mental health outcomes, adjusting for number of sexual partners, to see whether relationship violence remains associated with the health outcomes.
Response: We appreciate this concern. However, adjusting for the number of sexual partners would represent an over-adjustment, as it could potentially fall in the causal pathway.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests

Reviewer's report
Title: Health of Adolescents Exposed to Physical, Sexual and Non-Physical Dating Violence
Version: 1 Date: 25 March 2013
Reviewer: Deinera Exner-Cortens
Reviewer's report:
Reviewer’s report:
Review of “Health of Adolescents Exposed to Physical, Sexual and Non-Physical Dating Violence”
This article presents data from a sample of 585 students at Ohio State University (mean age=19.8), who retrospectively provided data on exposure to dating violence. Using these retrospective reports, the authors demonstrate that females who were exposed to dating violence when they were aged 13 to 19 were more likely to report smoking, depression, disordered eating and sexual risk taking, compared to their non-exposed peers. Males who were exposed to dating violence when they were aged 13 to 19 were more likely to report smoking and disordered eating, compared to their non-exposed peers.

This article addresses an issue of major public health concern (dating violence), and adds to a growing body of literature demonstrating that dating violence exposure is associated with adverse health and well-being. However, there are several major limitations that need to be addressed, in order to maximize the impact this paper can make to the field.

Major compulsory revisions:

(1) The authors assessed exposure to dating violence that occurred from age 13 to 19, but did not specify why they chose this age range (spans three developmental periods—early, middle and late adolescence—but is not inclusive of the entire adolescent period). Primarily, it is not clear why the authors did not ask about dating violence that occurred during high school (i.e., middle adolescence), and separately about dating violence that occurred during college (i.e., late adolescence), since by design, their sample made the transition from high school to college during the age period 13 to 19—developmental differences between these two age groups are important to acknowledge, including potential access to the victim, which is likely to be greater in college (for example, regarding the item about always showing up in person and waiting for them). From their description of the survey, it seems that the authors have data that would allow them to make this division.

Response: Thank you for this concern. We assessed dating violence spanning age 13 to 19 in response to the NIH’s call for proposals that assess dating violence spanning that age range. The survey was developed in response to the NIH’s call and piloted in late adolescent samples (ages 18 to 21) at The Ohio State University (the present study) and at Group Health Cooperative in Seattle (our original pilot site for the NIH proposal). The purpose of our study was to examine health experienced in association with “ever” dating violence, rather than health in association with the timing of dating violence. In our studies of adults, where we had a much larger sample (n=4,000 women and men), we were statistically able to examine health in relation to domestic violence timing across...
developmental periods (see Bonomi et al., 2006; Bonomi et al., 2007; Reid et al., 2008; Rivara et al., 2007). Within the present study of adolescents, where our sample is much smaller, we do not have the statistical power to partition abuse types AND timing in relation to health. We wish we could, as yours is a great idea! Our survey did ask about dating violence that occurred in high school and college, in that the survey covered up to ages 18 and 19. We did not, however, ask specifically about the “high school” and “college” periods, due to the nature of our study’s overarching objectives and the corresponding survey. The survey did include memory prompts that asked adolescents to “think about what year you were in school if you have trouble remembering when a relationship began …”

We are somewhat skeptical about your comment that “access to the victim being likely greater in college;” namely, we know from our extensive field work with middle- and high-school students that “showing up and waiting for the victim” occurs during these developmental periods in a variety of settings, including schools. Additionally, our survey published last year showed that a substantial proportion of females and males had their first experience of dating violence, which included these stalking-type behaviors, between the ages of 13 and 15 (see Bonomi et al., 2012). In response to your concern about developmental periods, we expanded upon our description and rationale in the Methods section.

Related to this, at the beginning of the Results section, the authors state that participants were aged 18 – 21 when they were surveyed. Thus, for some of these individuals, the assessment of dating violence and outcomes was concurrent (those aged 18 and 19), while for others, it was not (those aged 20 and 21). This is a point of confusion, and should be addressed. Also, since dating violence data are for the three most recent partners, it would be useful if the authors provided an estimate that allowed the reader to assess when most of these relationships occurred (e.g., average age at relationship beginning), since it seems possible that most of these relationships were college relationships, especially for the older individuals in the sample.

Response: This is an important point. However, to avoid confusion for readers about the objective of our paper (examination of dating violence types and health, rather than dating violence timing and health), we did not make this change. We added a notation in the Limitations section of our manuscript that future studies should examine both the qualitative nature of dating violence (Reviewer 1) and the timing of dating violence (your concern here). Related to your point here and a later point about the timing of our assessment, we also changed the title of the paper and attempted to clarify throughout the manuscript that we assessed health in late adolescence.

Finally, information on smoking, disordered eating, and sexual behaviors are lifetime prevalence rates, and so it is possible that these health risk behaviors occurred prior to dating violence, and thus are not a result of dating violence
exposure. Although this is an issue common to any cross-sectional analysis, it deserves discussion in the Limitations section.

Response: Thank you. We added a notation in the Discussion section that our study could not determine a temporal relationship between dating violence exposure and health.

(2) The data were collected retrospectively from a sample of university students. In general, the authors’ need to be more careful with their language, given their methodology. For example, the title should be revised, since the paper is not about the health of all adolescents, but the health of late adolescents (per Steinberg, 2008 – late adolescence is the period 18 to 21). Similar language revisions are required in the Objective section of the abstract. In the first sentence of the introduction (which refers to the “adolescent/young adult period”), the authors combine statistics across college, adult and early/middle adolescent populations. Since this paper is about dating violence experienced across the adolescent period, it seems that these statistics should be restricted to adolescent populations only (upper age limit of ~21). It would also be helpful if the authors clarified for what age group the outcome was found (e.g., the polyvictimization citation [15] is from a college, or late adolescent, sample, and therefore may not apply to early or middle adolescents).


Response: We appreciate this concern. Accordingly, we changed our title to: “History of Dating Violence and the Association with Late Adolescent Health.” We clarified the samples in the Introduction. We deleted references to the studies of “poly-victimization,” as both reviewers had concerns about it and due to our small sample size we were not able to examine such “poly-victimization” (e.g., physical+sexual dating violence).

(3) This comment refers to the stated purpose. Firstly, it seems the authors could make the novel features of their study more apparent (e.g., data on sexual, physical and psychological aggression; psychological aggression measure that includes an item on cyber-bullying, a new but prevalent form of aggression in teens). Secondly, the authors should more clearly state what their study adds to the field. Finally (related to comments (1) and (2)), please revise language to accurately reflect sample (i.e., it is the health of late adolescents).

Response: Thank you for these suggestions; we sincerely appreciate them. We included a much expanded section in the Introduction highlighting the novel features of our study.
(4) The authors state they used a log link with a Poisson error distribution, and report prevalence ratios. (a) Typically, Poisson distributions are appropriate for count data. The authors do not have count data, so it is not immediately clear why they are using a Poisson distribution. I am gathering they are using a robust Poisson model, where they assume that their binomial data are approximately Poisson. Is this a reasonable assumption in their data? Deddens and Petersen (2008) discuss that the Poisson method can be used to estimate prevalence ratios if the log-binomial model does not converge, so perhaps this is why the authors used this particular model. In any case, the use of this model needs to be justified. (b) The authors report prevalence ratios, which are useful when studying prevalent outcomes. Please provide information on why prevalence ratios were chosen (over odds ratios) in the Analysis section. Please also provide information on what statistical program was used.


Response: Thank you for these comments. We added a sentence at the end of the analysis section indicating that STATA statistical software was used for all analyses. We chose to use a modified Poisson regression approach to estimate prevalence ratios (relative risks) instead of odds ratios because relative risks are generally more interpretable than odds ratios. The outcomes in our study are not rare, and therefore odds ratios estimated from logistic regression models will not necessarily be good estimates for relative risks. Therefore, we chose to directly estimate the relative risks using the modified Poisson model approach. We added a sentence to the analysis section to reflect this choice, and added a reference for this (see Zou, 2004).

(5) The authors controlled for bullying victimization, and physical and sexual abuse experienced before the age of 18, in their multivariate analysis. They mention they did not control for other factors, such as age and education, because of the nature of their sample. The latter statement would be stronger if the authors could state that age and education weren’t empirically related to any of their outcomes and/or the exposure, and therefore aren’t confounders (as opposed to a theoretical statement). Also, there is mounting evidence that individuals who experience dating violence are also at risk for a number of other problem behaviors, potentially making them quite different than their non-exposed peers on a number of indicators. Since, given the authors’ data, it does not appear possible to control for these myriad confounders (or to control
for prior levels of the particular health behavior), the authors should discuss this in their Limitations section.

Response: There are two schools of thought about how to manage confounding. One suggests that factors empirically associated with outcomes be considered/included as “confounders.” Another suggests that factors theoretically associated with outcomes be considered/included as “confounders;” this approach is more targeted and is based on theoretical associations and associations documented in prior research, rather than considering “the mass of potential confounders” determined through empirical testing. Our analysis approach considered theoretically-based potential confounding factors (e.g., bullying). As ours is a college sample, all were similarly educated, therefore adjustment for education is not warranted. The age range of our sample is limited by design, and we did not adjust for age in our original model due to the lack of variability (ages ranged from 18-21). However, given the potential for confounding by age, we have refit multivariable models now adjusting for age. The confidence intervals (which you noted were wide for some estimates, particularly for males) became narrower after we adjusted for age.

(6) The authors are correct that collection of data via retrospective methods is the current standard in the field, and the use of memory prompts is a strength of the study. However, the use of retrospective data in any study is a limitation, and seemingly more so as the length of the recall period increases. Since all findings in the paper hinge on accurate assessment of dating violence, more discussion of effects of retrospective recall on the present findings in the Limitations section seems justified. The authors are encouraged to see Jouriles, McDonald, Garrido, Rosenfield and Brown (2005), who investigated this issue for teen dating violence specifically.


Response: We augmented the Discussion section to reflect potential problems with our collection of retrospective teen dating violence data, and made reference to the Jouriles’ study. Thank you for providing relevant references. The difference between our assessment approach and that done by Jouriles is that 1) we collected a detailed partner history across age 13 to 19 before we assessed dating violence histories and 2) we assessed multiple dating violence types (rather than physical aggression only). Our detailed assessment approach helped link subjects to the particular time frame we were interested in, by asking specific questions about their dating relationships first. We then included an assessment of ever dating violence history from age 13 to 19 and then partner by partner during that age period. In contrast, Jouriles compared a 2-month retrospective assessment of physical aggression among high school students to
a “cumulative method” that used four 2-week retrospective assessments; the study found that a higher proportion of subjects reported physical aggression in the “cumulative method” than in the 2-month retrospective approach. To compare our study directly to that of Jouriles is not advised, since our assessment method was more comprehensive (we asked about dating partner history first, and then about abuse history; we also assessed multiple types of dating violence, rather than physical aggression only.)

(7) The items “exercised to lose weight” (particularly) and “ate less to lose weight” (to a lesser extent) seem like they could be healthy behaviors. For example, the percentage of participants who report that they exercised to lose weight (58.3-89.3%) makes it seem like this could be a normative behavior. So, ask that the authors either provide justification that these items are assessing disordered eating, or remove these items from the paper.

Response: Excellent point. We removed “exercised to lose weight” and “ate less to lose weight” from the analysis.

(8) Is there a reason the authors chose to look at individual items, rather than composite scales, for depression and disordered eating? For example, for the Depression scale, it seems that the screen was intended to be both questions, and not individual items, and so it is not clear why the authors chose to use individual items, as opposed to the composite scale.

Response: There is no composite scale for the two depression items. They are single items and are scored as such. In response to both reviewers’ concerns, we clarified information about the depression items in the revised manuscript.

(9) Given the authors statement in the Limitations section that males were under-represented, which may have led to reduced statistical power, the first sentence of the Discussion (that dating violence had more pronounced health effects for females than males) seems potentially misleading. Sample size does seem to have impacted the analysis; for example, the confidence interval for the male smoking prevalence ratio (1.08, 14.2), is very wide, reflecting the small sample size and lack of precision in the estimate. So, the lack of associations may reflect power issues, and not a difference in true associations. Given the reduced power in the male sub-sample, the authors should be careful with statements that compare males with females. In light of this, would suggest the authors remove the first sentence of the Introduction, and begin the introduction with the second sentence (“Compared to non-exposed females…”).

Response: Another excellent point; thank you. We removed the first sentence of
the Discussion, and removed other comparisons of females and males. As a side note, when we re-ran the analyses adjusted for age (in response to reviewer request), the confidence intervals became narrower.

Minor Essential Revisions:
(1) With the exception of citation [5], the articles the authors cite in the first paragraph of the introduction are cross-sectional, and so are limited in terms of their ability to show temporal associations. There are now several longitudinal studies that demonstrate outcomes of teen dating violence, and the authors are encouraged to focus on these stronger studies in their introduction (and/or clarify that most of the studies they are citing are cross-sectional):

Response: Thank you for these suggestions. We revised our Introduction to note that the health-dating violence association studies we cited are largely cross sectional. Your recent study (Exner-Cortens et al., 2013) is the most relevant to our Introduction (and to our Discussion), as you focused on a wide range of health outcomes (rather than simply abuse re-victimization as the other studies
did). We included a detailed review of your study in the Introduction, and noted that your findings support those of other studies showing cumulative violence victimization risk. We also compared our study findings to yours in the Discussion section, but cautioned readers about making direct comparisons due to differences in study design (yours is longitudinal, ours is cross-sectional) and to differences in the way violence was assessed across studies. We are grateful to have such an important study to refer to in the literature!

(2) The reasoning for the inclusion of the second paragraph of the Introduction was not clear. The authors state that “studies of adults have more extensively parsed health effects…”, but given their overview in the first paragraph, the adolescent literature seems fairly extensive. If the authors feel there is something in the adult literature that adds to the arguments in their paper (that is not currently addressed in the adolescent literature), please make this clearer.

Response: We revised this section, including noting that studies in the adult literature examined the full range of violence types (physical, sexual, and non-physical) recommended for assessment by the U.S. Centers for Disease Control and Prevention. Moreover, the studies in the adult literature parsed these specific violence types and examined their association to health. Prior studies among adolescents had not done this; for example, while the Exner-Cortens study examined psychological abuse only and physical and psychological abuse categories in relation to health outcome, sexual violence was left completely out.

(3) Do the authors think there are any potential limitations from combining the two samples? (one which was randomly selected, the other involving volunteering as a part of a class, and which had differential response rates for males).

Response: We agree that there could be potential limitations by combining samples. We expanded upon this in the Methods section. The over-distribution of females in Study 2 (recruited from undergraduate classes) compared to Study 1 (recruited through the university registrar) represented the main difference between the two samples. Otherwise similar characteristics existed between the two samples due to our narrow eligibility criteria (e.g., subjects had to be between the ages of 18 and 21).

(4) Clarify how asking about health before dating violence reduces response bias (Methods, Survey-Health and Health Behaviors).

Response: Asking subjects details about dating violence first, which could be a traumatic experience, could potentially cause bias in their responses to the health items; specifically, subjects might judge their health as “worse” if the experience of completing the dating violence questions was traumatic (see Bonomi et al., 2006). We clarified this in the revised manuscript.
(5) The scale used to assess dating violence is not a validated scale. Please mention this in Limitations.

Response: The Retrospective Teen Dating Violence Assessment, which has been presented at the Women’s Health Congress in March of 2013, is now under review in manuscript form. To address your concern, we further discuss limitations of our retrospective assessment approach in the Discussion, including drawing upon some of the literature you provided and other literature we consulted.

(6) In Methods section, when describing the dating violence measure, missing a citation for “Coker’s dating violence survey.”

Response: Coker’s dating violence survey has not been published yet. We corresponded directly with Dr. Coker about this.

(7) Meaning of the sentence in the Methods section beginning “As we were attempting to collect detailed information…” is not clear.

Response: We deleted the sentence and expanded upon the merits of our dating violence assessment coverage (physical, sexual, and non-physical abuse types, including those relevant to today’s teens such as cyber-bullying).

(8) What questions were used to assess bullying victimization? Please list in the Methods section.

Response: We added clarification about the bullying question (from the CDC’s Youth Risk Behavior Survey) to the Methods section. One question was used to ask about whether subjects had been bullied between age 13 and 19.

(9) In the last paragraph of the Methods, the authors state that they asked about abuse experienced by “someone other than a dating partner…” But, in the Results, Abstract and some tables, they also refer to this as “childhood abuse” and “child abuse”, which gives the impression that it was abuse perpetrated by a parent and/or family member. For clarity, please give term and definition in the Methods section for how this variable will be referred to throughout the paper, or consistently refer to as physical and sexual abuse, specifying that this was abuse perpetrated by someone other than a dating partner.

Response: Thank you for pointing this out. We clarified the items in the text and tables to be consistent throughout the manuscript. We asked two questions about abuse before age 18, including being punched, kicked, choked, or receiving a more serious physical punishment from a parent or other adult guardian before age 18 (1 question) and being touched in a sexual place or being forced to touch another person when they did not want to before age 18 (1 question).
(10) Please list the % female in the Results section (Characteristics of Study Sample).

Response: We made this revision.

(11) Results, Paragraph 1: “consistent with the Ohio State University student population” – do the authors have a citation for the data used for this comparison?

Response: We added a citation for The Ohio State University student population.

(12) In the Results, the authors state that 67.4% of females and 57.1% of males reported dating violence victimization, which thus makes dating violence seem like a fairly normative occurrence (since it is being reported by a majority). I suspect this number is being driven by items such as “called you names…”, and that the prevalence would be much lower for the physical and sexual items. It would be helpful if the authors listed the prevalence for each item in Table 1, and then in the Results section, gave a) the overall prevalence, b) the prevalence of non-physical only, and c) the prevalence of physical/sexual, stratified by gender.

Response: Thank you for this very important suggestion. We added a table showing the prevalence of abuse types, stratified by gender.

(13) In the Limitations section, it is not clear how their prior study of adult women (demonstrating that the probability of participation did not differ based on age or health care variables) supports ignorable non-response in this different sample of late adolescent males and females.

Response: We deleted reference to this study in our reworking of the Discussion.

(14) In the References section, Reference 4 is listed twice (as Reference 4 and Reference 10).

Response: We have had a problem with our reference manager in our submission of the manuscript revision. For example, you will note the duplication of references #4 and #10. Rather than hold up the submission process (the revised manuscript is due May 11), we will make this very important correction in our next round of revisions.

(15) There are a number of grammatical errors throughout (in manuscript text, tables and references). Also, please make sure the number of decimal places is consistent throughout (e.g., Table 4, 1.23 vs. 10.7).

Response: We carefully reviewed our manuscript prior to submission and again
prior to submitting the revision. We are meticulous in our manuscript submission processes, so we are concerned that there were “a number of grammatical errors throughout.” We certainly hope we captured and addressed the grammatical errors, and expect that the copyedited version of the manuscript will serve as an additional check on the grammar. Typically, in confidence interval estimation, the “0” is dropped from the second decimal place, which is why some numbers in Table 5 (formerly Table 4) are specified to once decimal place.

Discretionary Revisions:

(1) Please give the SD for age in the Methods section of the abstract.
Response: We added the SD for age in the Methods section of the abstract.

(2) While the response rates to the internet survey are low, they do not seem out of the ordinary for this type of research. In order to make this clear to the reader, the authors are encouraged to cite literature on typical internet survey response rates, e.g.,


Response: We very much appreciate these reference suggestions.

(3) The authors’ findings regarding gender differences in associations between victimization and outcomes reflect findings reported by Exner-Cortens, Eckenrode and Rothman (2013), and so they may wish to cite this paper in the Discussion. Also, the two papers they currently cite (Silverman et al., 2001; Holt & Espelage, 2005) both use high school samples, which may explain some differences in findings; the Exner-Cortens et al., 2013 paper also uses a late adolescent population, which may be a more appropriate comparison.

Response: We incorporated the study by Exner-Cortens, Eckenrode and Rothman (2013). We are grateful to have such an important study to refer to in the literature!

(4) The last sentence of Paragraph 2 of the Discussion could be more clearly
written.

Response: In the manuscript revision, we decided that the sentence was not needed. We deleted it.

(5) In Table 2, it would be clearer if missing data were put in a footnote.

Response: We appreciate this suggestion. However, because there were different numbers of missing data for each item, representing this information in a footnote would be confusing to readers. If you have a suggestion for how this change could be made to make it transparent for readers, we would be happy to make the change.

(6) In Table 3, would be helpful if the authors listed the sample size for males and females, and also the sample size in each category (No TDV vs. Any TDV; similar to how sample size is shown in Table 4).

Response: Thank you; in response, these revisions were made.

(7) The inclusion of confidence intervals in Table 4 is appreciated. While confidence intervals allow the reader to determine whether the association is significant, it might also be helpful to include asterisks for p-values (e.g., *p<.05, **p<.01, ***p<.001), so that readers can quickly assess which associations were significant.

Response: The confidence intervals provide more information about the precision of the estimates than the p-values. We did not make this change.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare I have no competing interests