Author's response to reviews

Title: Prevalence and gender patterns of mental health problems in German youth with experiences of violence: the KiGGS study.

Authors:

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Object: MS 1673831933866904 - Prevalence and gender patterns of mental health problems in German youth with experience of violence: the KiGGS study.

Thank you for considering our manuscript for publication in BMC Public Health. We also thank the reviewers for their useful recommendations. We respond point by point to their comments below.

Reviewer # 1: Christin Carotta

Reviewer's report:

Major Compulsory Revisions:

Introduction:
1. The questions posed by the authors are clearly stated. It would be helpful, however, for the authors to define internalizing and externalizing mental health problems.

A definition of internalizing and externalizing problems has been added (p. 4). It reads: “Child and adolescent mental health problems are traditionally conceptualized as internalizing and externalizing emotional and behavioral problems. Externalizing problems are characterized by dysregulated behaviors, which include problems with inhibiting unwanted behavior, controlling attention and cognitive processing. They include attention deficit hyperactivity disorder (ADHD), oppositional defiant disorder (ODD), and conduct disorder (CD). By contrast, internalizing problems are associated with an inability to control negative emotionality, such as rumination, loneliness, sadness, anxiety, and depression [11].”

It is further suggested the authors explicitly state within the introduction which mental health outcomes are considered internalizing versus externalizing. We have introduced corresponding subheadings in the introduction section: “Internalizing problems and violent behaviors” and “Externalizing problems and violent behaviors”, where we explicitly describe the respective outcomes. Furthermore, we have reorganized the tables, now presenting internalizing and externalizing indicators in a more coherent manner.

2. The authors provide important inclusion of several relevant studies. There is no clearly articulated theoretical framework, however. Elaborating on the “social and biological reasons” why internalizing disorders are more frequent in females while externalizing disorders are more prevalent in males, may provide an avenue for the inclusion of theoretical underpinnings (page 6).
We have taken up your suggestion and elaborated on the paragraph. It now includes a reference to the cognitive vulnerability-transactional stress depression model, as well as references to corresponding empirical results (see pp. 6).

Methods:
3. This study includes a nationally representative sample of impressive size. Further information, however, is needed regarding sampling and measurement to demonstrate the soundness of data (specific suggestions in #4-8).
Further information has been added to give readers a more comprehensive picture of the sampling and study procedures. Details are presented in the responses to #4-8.

4. How were the primary sample units selected?
The passage has been reformulated and additional information added. It now reads (p.9f):
“The sampling strategy of the KiGGS study has been described elsewhere in detail [49, 50]. Briefly, the sampling frame followed the principles of a stratified multistage probability sample [51]. The participants were recruited in two steps. In the first step, 167 study locations (primary sample units or PSUs) were systematically chosen from an inventory of German communities stratified according to the BIK classification [52], which measures the degree of urbanization and geographic distribution. Using the Cox procedures for community sampling [53], the number of PSUs per stratum was determined with a sampling probability proportional to population size. In the second step, an equal number of study subjects per birth cohort over the entire age range was randomly selected (simple random sample) from the local population registries.”

5. How was information about the study and the self-administered questionnaires dispersed?
Corresponding information has been added. It reads (p.9):
“Parents of eligible children and adolescents were contacted by letter and invited to participate. Information was provided about the type of investigation, ethical approvement, data processing, the voluntary nature of participation, and monetary compensation.”

6. More information regarding the weighting of the sample population would be beneficial.
We now describe the weighting procedure in more detail; it reads (p. 14f):
“In a first step, the sample weight takes account of the study design by considering both total numbers of eligible youth aged 0-17 within the PSUs and the sampling probability of the PSUs itself. The design weights are inversely proportional to the sampling probability of the study subjects, which itself is composed of the PSU’s selection probability (proportional to the number of 0- to 17-year-olds in the community) multiplied by the sampling probability of subjects within the community (i.e. the number of actual participants relative to sex and age group divided by the total number of children in the community within the respective gender and age group; the age-group classification is as follows: 0-2, 3-6, 7-10, 11-13 and 14-17). The design weighting was conducted separately for the three regions of eastern Germany, western Germany and Berlin. In a second step, the weight was adjusted for deviations from the population structure (as per December 31, 2004) regarding the cross-classification of age (in years), sex, region (eastern Germany / western Germany / Berlin), and nationality (German / not German). For further details of the weighting procedure see [49].”
7. Additional information regarding specific measures is also needed. For instance: 1) What were the response options for violence involvement; and 2) What types of questions and response options are included in the SDQ? Example items and response options of the SDQ have been added (p. 11). As to violence involvement, the passage reads (p. 11):

“Violence involvement was assessed based on two questions pertaining to the respondents’ experiences as victims of violence (“How often have you been a victim of violence in the past 12 months?”) or as a perpetrator (“How often have you been a perpetrator of violence in the past 12 months?”), which had been used before in other German studies on youth violence [54, 55]. The response options were never/once/several times. In this study the following violence typology was chosen: youth who reported having been a victim once or more often and not having been a perpetrator were classified as victims; youth who reported having been a perpetrator of violence once or more often but who reported not having been victimized were classified as perpetrators; those who reported both victimization and perpetration once or more often were classified as perpetrating victims.”

8. Parent-reported ADHD diagnosis: On page 9, the authors note that in Germany, the diagnosis of ADHD is “not legally restricted to child and adolescent psychiatrists or clinical child psychologists” but it is “likely, however, that clinical diagnoses are usually assigned by these professional groups.” What empirical support indicates this likelihood?

In Germany, the first presentation of youth with behavioral problems is usually with the pediatrician (some may also consult the family doctor or go directly to specialized centers). Once an individual presents with suspected ADHD at the pediatrician’s office, clinical guidelines recommend referral to specialized centers or clinical psychologists, where the diagnosis is made or verified. We have added the corresponding information including a reference to the respective clinical guidelines (p. 13).

Discussion:

9. The authors clearly stated several significant limitations for this work, with specific mention of the single-item violence assessment and acknowledgment that participants were not provided with a definition of violence. Further clarification and discussion, however, is needed with regards to the “limited information” that resulted in measures that “necessarily remain fuzzy” (page 17). We have now referenced the variables that were meant (p. 25). The reader is also referred to the methods section, where we provide a thorough evaluation of our measures. Furthermore, we have added additional discussion on the validity of self-reports in violence research and cited a study that investigated the understanding of the term “violence” in German students. It reads (p. 25):

“However, research suggests that German students referred to a narrow concept of physical violence in their subjective understanding of the term “violence” [86].”

Minor Essential Revisions:

10. The authors importantly acknowledge that these findings speak to associations rather than causality. However, causality is suggested in the first sentence of the implication section (page 17).
The sentence has been rephrased. It now reads (p. 26): “Our study has social-policy and clinical implications, as the results show that violence involvement is strongly associated with the mental health of youth.”

11. On page 5, the term “striking behavior” is slightly confusing when embedded within the discussion of violence. Is there empirical support for the statement that these ADHD behaviors “render individuals targets for peer aggression?” Admittedly, the term was confusing. We have changed it into “maladjusted behaviors” (p. 6).

12. Clarification is needed to indicate what general population the statistics regarding the prevalence of mental health disorders refers to (page 5). We have complemented the statement. It now reads (p.5): “Studies of mental health problems in the general child and adolescent population of western industrialized countries indicate that 9.5% to 22.2% of youth suffer from one or more mental disorders [1, 2].”
The manuscript, entitled “Prevalence and gender patterns of mental health problems in German youth with experiences of violence: The KiGGS study” presents findings from a nationally representative, cross-sectional study of mental health problems and violence involvement among German adolescents. The manuscript is an important contribution to the literature with potential clinical and social policy implications. However, several methodological, stylistic and organizational issues should be addressed to improve the quality and clarity of the manuscript.

Major Compulsory Revisions
The current sample comes from a cross-sectional and nationally representative study of German adolescents. However, the manuscript includes conclusions and interpretations that imply causality. For instance, results section of the abstract includes the following statement “However, multiple risk constellations were found for all violence-affected youth including both internalizing and externalizing mental health problems which points at violence as a potential common cause of these problem behaviors.” I believe the current study does not provide evidence to conclude that. Violence was measured for the last 12 months while mental health issues were asked for the past 3 months. Despite there are different time frames used for various questions, they were still administered at the same time point. Please review the manuscript, especially discussion section, to change referrals to causality and such misleading interpretations.

It is true that the study design does not allow for causal interpretations. We have therefore put the statement into a broader context. The passage now reads (p. 23):
“Taken together, we found that internalizing and externalizing mental problems were part of the profiles of any kind of violence history in youth – victims, perpetrators and perpetrating victims – which suggests that the occurrence of these categories is not mutually exclusive [67]. Comorbidity may be rooted in a common cause [79]. Being exposed to violence may represent such a cause. For example, the co-occurrence of internalizing and externalizing problems in youth in the context of violence may well be explained within the framework of the cognitive vulnerability-transactional stress depression model [30]. The model posits that broad negative affect initiated by a strong negative interpersonal event – as violence clearly is – accounts for both elevations in depression and externalizing problems. However, a bidirectionality of relations is conceivable and, as our study is cross-sectional, we are unable to confirm causal relationships.”

The last paragraph of the introduction lists three study objectives and results section also presents findings for each research objective. However, findings regarding objective 1 have not been interpreted in the discussion section. This is particularly important given that there is scarcity of research on prevalence rates of internalizing and externalizing behaviors and violence involvement by gender among German adolescents. Please interpret and discuss findings as the space permits. For instance, one of the striking findings regarding objective 1 (prevalence rates) is that disordered eating behaviors are found to be very high among adolescents (28.9% among girls and 15.4% among boys) compared to other mental health issues (range 2.1% to 11.9%). Please discuss this finding.
A short discussion of these findings is now included on p. 19 (discussion section). As to the high prevalence of disordered eating, we have included further discussion of our instrument in methods section. It reads (p. 12): “It must be emphasized that the questionnaire is neither designed to make diagnoses according to the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) or the International Classification of Diseases (ICD-10), nor does it discriminate between different types of disordered eating (e.g. bulimia or anorexia nervosa); rather, the survey includes disordered eating on a subclinical level. This characteristic of the questionnaire may explain the far higher prevalence rates obtained in assessments with the SCOFF questionnaire compared to the rates reported by studies that strictly apply diagnostic criteria according to the ICD-10 or DSM-IV [45].”

One major methodological issue in the current study is that it does not differentiate between peer violence/bullying and intimate partner violence. It is unclear what type(s) of violence adolescents are reporting. Authors should clarify the focus of the study in the introduction more fully. We have included our understanding of youth violence in the introduction section on page 4. The passage reads: “A current definition of youth violence posits that a young person “can be a victim, an offender, or a witness to the violence” [4, p.1]. However, in bullying research, for example, it is widely acknowledged that an individual may be a victim and a perpetrator at the same time. Those who both perpetrate and are victimized have been variously termed throughout the literature (e.g. aggressive victims, bully-victims or perpetrating victims) and are regularly found with the lowest levels of psychosocial functioning [7, 8]. Recent research suggests that it might be reasonable to adopt this category in research on youth violence [9, 10].” In addition, we have cited evidence on German youth’s subjective understanding of the term “violence” in the limitations section (p.25). It reads: “Another limitation is that no definition of violence was presented to the participants. Therefore, the participants might have differed in their cognitive concepts. However, research suggests that German students referred to a narrow concept of physical violence in their subjective understanding of the term “violence” [86].”

Another major issue is internalizing/externalizing ‘behaviors,’ ‘problems,’ and ‘clinical diagnoses’ are used interchangeably in the manuscript. It is mentioned in methods section that the measures assess ‘symptoms’ and also ‘behaviors,’ so they do not necessarily indicate diagnosis. However, clinical cut off scores were used to examine adolescents who report clinically significant mental health issues. Authors address this issue towards the end of the manuscript in discussion section. I think manuscript would be more clear if they keep the language consistent (only use one term). In addition, introduction should clarify whether the authors’ interest on clinical mental health issues vs. problematic behavior. We have indeed used different terms to address youth’s mental problems. In order to keep the language more constant, we now use the term “problem” wherever possible. However, the use of different terms is in part due to the fact that various measures of child mental health were used in the KiGGS study. A more dimensional approach to psychological health and violence in this sample has been attempted in a prior study (Schlack et al., 2013). Therefore, in this study, we were interested in clinically relevant symptomatology. In order
to enhance clarity for the readers, we have added a paragraph in the measures subsection. It reads (p. 10f):

“The KiGGS study provides a basis for a variety of mental health measures. Due to constraints on time and resources, written questionnaires were used rather than diagnostic interviews. However, they included internationally recognized and validated screening tools for child mental health, an assessment of psychiatric diagnoses by clinicians (as reported by the parents), and an assessment of symptoms and behaviors self-reported by the participating youth. We intended to approximate clinically relevant symptomatology in this study. Thus, where available, clinical cut-offs of our measures were used. Below, we describe our measures by thoroughly evaluating their strengths and limitations.”

Methods section refers to Kurth et al., 2008 publication for details on study design. Therefore, it is understandable authors provide short description of the study in the manuscript. However, I believe there are several important details missing. For instance, sampling strategy is unclear (method section, under subtitle ‘sample’). Are adolescents recruited from schools or neighborhoods? Parents reported on ADHD diagnosis of their child – then, are adolescents and their parents were recruited as dyads? How many parents were there in the sample who agreed to provide that data? One option is you can include a diagram showing the sampling strategy and study procedure in the manuscript. Another option is to include 2-3 more sentences on some essential issues regarding study procedures. I think interested readers who need more information can use the methods paper (Kurth et al., 2008), but some fundamental information still needs to be mentioned in the current manuscript.

Following reviewer #1’s suggestions, we have added substantial additional information on the study design, the study procedures and the weighting of our sample (pp.9; see also above).

Socio-demographic characteristics of the sample have not been mentioned in the manuscript. Please include a demographic table in the end of the paper if space permits. Another option is to have a short paragraph in methods section under the subtitle ‘sample.’ If you choose to have a table, please report sample and subsample characteristics. Given that this is a sample from nationally representative study, it is especially important to have descriptive information on the sample and the extent to which it represents German adolescents.

We have now included a paragraph in the results section (subheading: “Sample characteristics”) with a basic sociodemographic description of our sample. The use of the term “subsample” was probably misleading and caused by word confusion. In this study we did not examine a subsample in the sense of a random sample from the total sample; rather, we examine all the subjects of our sample who were 11 and older at the time of the survey. To clarify matters, we have eliminated the term “subsample”. The passage now reads (p. 9):

“A total of 17,641 children and adolescents between the ages of 0 and 17 and their parents participated in the baseline assessment that took place between May 2003 and May 2006. The net response rate was 66.6%. The study is unique in Europe in terms of its sample size, its age range and its response rate, as shown by the EU Health Surveys Information Database (http://www.euhsid.org/). The participating children and adolescents were given a physical examination; the parents – from age 11 on, also the children and adolescents themselves – completed extensive self-administered questionnaires on their physical, social and mental
health. The present study was conducted on 6,813 adolescents (3,492 boys and 3,321 girls; unweighted totals) aged 11 to 17 who took part in a violence assessment.”

There is no justification for why ADHD diagnosis was assessed via parent report. Please explain (in introduction or in methods)

The assessment of ADHD as parent-reported clinical diagnosis refers to constraints on time and resources in our survey. The ethics committee did not permit minor study subjects to be examined for more than two hours in total. Because the survey included a host of further health issues (physical, social, behavioral), it was not possible to include full diagnostic interviews (see also the measurements cited above).

In methods – statistical analysis section, authors report using logistic regression. Authors need to justify why they chose to utilize logistic regression, rather than any other test. For instance, the group comparisons could be done using cluster analysis. Or more sophisticated analysis could be done using multilevel modeling (i.e., HLM) because subjects were nested in schools (or neighborhoods) or in dyads (parent-adolescent). One strategy could be analyzing the data as nested within violence involvement groups. Given the study design, multilevel modeling seems more appropriate.

The purpose of this study was not to group the subjects according to their individual characteristics, as would have been achieved with cluster analysis or other person-oriented analysis strategies. Because we expect clear relations between the variables, the use of logistic regression analyses appears adequate to us. Because there is now additional information on our sampling frame (see above), it should by now be clear that the study subjects were not nested in schools. Likewise, analyzing the data as nested within the violence-involvement groups does not appear to be mandatory because there is no good reason to assume that the various (theoretically derived) violence-involvement groups would be clustered. However, it is correct that hierarchical linear modeling could have been employed to account for the multistage cluster sampling of our study. However, HLM is only one possibility to account for the sample clusters. Another possibility is the use of the Taylor-series (linearization) method which we employed in this study. The use of the Taylor-series method is advantageous because it corrects for deviations of the standard errors and leaves the point estimates unaffected.

There is a mismatch between statistical analysis section and the results section. Last paragraph of the methods reads ‘adjustments were also made for age, socio-economic status and family status.’ Findings and tables do not report those adjustments. Also, it would help greatly if you could provide the equation for logistics regressions in the text.

The adjustment variables are now indicated in a footnote underneath Tables 3 and 4. We do not advocate including the equation for the logistic regressions in the text because the basic model is not too complicated. However, for your convenience, we provide the general equation for the models here:

$$\text{Logit (Y} = \alpha + \beta_1(\text{age}) + \beta_2(\text{family structure}) + \beta_3(\text{SES}) + \beta_4(\text{sex}) + \beta_5(\text{violence involvement}) + \beta_6(\text{sex*violence involvement})$$

Variables and codings:
Y = mental health outcome (0=absent; 1= present)
One of the most striking findings of the study is how perpetrating victims were found to be at higher risk of mental health issues than other violence-involved youth. I think authors should discuss this finding more fully. In addition, authors suggested that this result revealed how some youth had ‘anxious-aggressive behavioral style.’ There is no data to draw that conclusion because assessment of violence involvement does not specify dynamics of the incident (i.e., nature of the event, peer vs. IPV, no assessment of self-defense.)

We have now added more discussion on the findings on perpetrating victims, beginning on page 21. We have further revised the passage on anxious-aggressive behavioral style. The passage now reads (p. 22):

“Translational science posits that traumatic experiences – such as being victimized – may account for individual differences in attributional style, which, in turn, explain differences in aggression [74]. Empirically, individuals who are both anxious and aggressive are found to be likely to attribute hostile intent to the behavior and actions of others (predominantly in ambiguous situations) and are thus more likely both to behave aggressively and to be victimized [75]. Emotional and behavioral dysregulation have been described as causal on the road to peer victimization in aggressive primary school children [76]. In our study, perpetrating victims displayed the highest rates and highest risks for clinical ranges of both emotional and conduct problems. This may indicate that problems with affect regulation, which put younger children at higher risks for simultaneous perpetration and victimization, extend into teen age. However, we were not able to objectify this in our study.

We hope you’re happy with this.

Another striking finding is gender-cross over effects and authors are recommended to talk more about existing theories to explain and discuss that phenomenon. In addition, authors should be especially cautious to avoid ‘gendered’ language. For instance, the paragraph before ‘limitations section’ includes a sentence ‘possibly, the lack of conformity with specific role expectations exacerbates maladjusted behaviors in vulnerable girls and boys in the respective direction.’ This sentence implies that once adolescents do not fit in gender roles, there is something wrong with them. However, there is research suggesting how gender roles can create distress on the young adults and (gender) stereotype threat can have negative consequences. I also wonder if gender-cross over effects may occur through different socialization practices among boys and girls. It is common that girls socialize to internalize while boys are encouraged to externalize their emotions. For those who show gender cross-over effects, it would be interesting to look at their family background, institutional involvement, and potential traumatic experiences during their childhood. It is possible that for an adolescent girl it was functional to ‘externalize’ the behaviors if there was constant threat for personal safety.
It was not our intention to imply that there may be something wrong with an individual once it does not fit a gender role. We have, therefore, rephrased the passage. It now reads (p. 24f.):

“Motivated by observations in earlier non-normative studies [5, 43, 44, 46], one research question of this study was to investigate whether gender cross-over effects would occur in a representative sample. Indeed, we found such reverse associations in the form of five-fold higher risks for conduct problems and up to nine-fold higher risks for illicit drug use in violence-affected girls; there were also increased risks for somatic pain in violent boys, but lower overall prevalence rates for males in our sample. The fact that gender cross-over effects for both boys and girls emerged in a representative sample suggests that this phenomenon is not an artifact of sampling [23]. Nonetheless, the underlying reasons why such effects occur in violence-affected youth are still unknown. Gender research distinguishes between the concepts of gender stereotypes (i.e. people’s beliefs about how the sexes differ or should differ) and gender identity, which can be described as a representation of self in relation to gender categories – including comfort with one’s gender and internalized social pressure to conform to gender stereotypes [81]. Research indicates that pressure to conform to gender norms predicts internalizing problems, more strongly for girls than for boys [82]. It is certainly conceivable that a mismatch between gender stereotypes and gender identity exacerbates maladjusted behaviors in vulnerable girls and boys. This hypothesis is supported by the observation that gender-dysphoric girls are perceived as more aggressive, more disruptive and antisocial by their peers than gender-content girls [83]. On the other hand, highly adversarial interpersonal relationships are one of the gender-specific risk factors for female offending [84]. It is conceivable that it may be functional for some girls to externalize behaviors in the face of a constant threat to their personal safety. Nonetheless, there is a clear need for in-depth research into the specific determinants of reverse gender stereotypes in the context of violence in youth.”

Discussion – limitation section should include the low response rate and how it potentially limits the generalizability of the findings to the general population. However, if 66.6% is an acceptable response rate as seen in similar studies in Germany, authors should include that information to justify external validity of the study.

A response rate of 66.6% in a population-based, representative, large-scale health interview and examination survey is quite good. We have included a corresponding reference in the sample description, beginning on page 9. The relevant passage reads (p.9):

“The net response rate was 66.6%. The study is unique in Europe in terms of its sample size, its age range and its response rate, as shown by the EU Health Surveys Information Database (http://www.euhsid.org/).”

Minor Essential Revisions

Methods section of the abstract includes information on the sample size and age range. More information is needed to inform the reader about sampling and design of the study.

Corresponding information has been added (pp. 9). See also above.

Results paragraph of the abstract should include p values, odd ratios, and coefficients. Please report major findings in the abstract with relevant statistics in the parentheses.
We do not advocate including p-values and other statistics in the abstract. Abstract space is limited and, because we are analyzing many different groups, there would be a too many statistics to report.

Second paragraph of the introduction does not flow and it is hard to follow. Please revise the paragraph and organize accordingly.
We have completely reorganized the introduction section (see also above).

Measures section in methods, paragraph regarding ‘violence involvement’ should include how items were obtained and the original source (i.e., pilot study, previous prevalence study, and citation etc.)
The corresponding references have been added.

Under subtitle ‘emotional and conduct problems’ of methods section, please indicate how many item each scale included and what type of Likert scale and response categories were used (i.e., 5 point Likert scale, 7 point Likert Scale vs. dichotomous yes/no.). Also, change “a” to “#” (alpha). It looks like there is a typo. Finally indicate German cut off scores in parenthesis.
The requested information has been added. The typo must have occurred in the course of the conversion from a .docx to a .pdf document while submitting the manuscript in the upload center. In our original copy, the letter appears the Greek letter “alpha”.

Methods section, subtitle ‘frequent drinking and illicit drug use’ should include information on time frame. Please clarify what ‘current’ alcohol use refers to (a week? a month? Etc.) Also, provide citation for 5 standard drink rule.
The question was asked in this way, without offering a time frame. This clearly represents a limitation. As regards the classification of frequently drinking youth, there is no such thing as a recommendation on hazardous regular drinking in youth in order not to promote risk-free alcohol consumption. The cut-off that we chose in our study represents alcohol consumption levels approximately at the 90th percentile in our sample. We have, therefore, added a corresponding statement (p. 14):
“Unlike for adults, there are no recommendations as to hazardous regular alcohol consumption in adolescents in order not to promote a risk-free drinking. The cut-off of 5 glasses and more was therefore arbitrarily chosen. It represents, however, alcohol consumption levels approximately at the 90th percentile in our sample.”

Results section, second paragraph includes some findings for research objective 1, documenting prevalence of mental health issue. Please report actual percentages here, rather than ‘3 times more’ because it may be misleading. We do not know about the missing data due to non-response in each measure. Valid cases may not match.
Because we have used multiple imputation methods to replace missing data in our data set, all cases do match (for details on multiple imputation, please refer to methods section, subsection ‘Statistical analyses’, pp. 14).

Similarly, please report both frequencies and percentages in the text (results section) as well as in the tables. This will allow the reader see valid cases.
Results section, paragraph 3 and 4, please report odd ratios and p values.
We have opted not to change the description of the results. First, as indicated above, all cases can be considered as valid cases due to the use of multiple imputation. Second, besides problems of redundancy, including percentages, odds ratios and p-values would render the text barely legible, given the variety of groups that were examined. For example, reporting the percentages of Table 2 alone would involve 8 (mental health indicators) x 4 (violence involvement groups) x 2 (sexes) = 64 numbers to report plus 16 p-ranges.

References