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Title	Using the Edmonton Obesity Staging System in the real world: a feasibility study using cross-sectional data
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Reviewer 1	Dr. Svetlana Puzhko
Institution	McGill University, Montreal, Que.
General comments (author response in bold)	Study design 43) It is a cross-sectional study using a cohort of primary care patients. "Retrospective" assumes a cohort study. **Thank you for your comment. We concur and have made the change. (1, 3)** The Abstract. 44) In the background section of the abstract, I would change the wording to make it more clear that EOSS was already validated as a better predictor of mortality than BMI, and only a feasibility of calculating this predictor in a health database was a study goal. **Thank you for your comment; we have changed our wording to increase clarity. (3)** 45) Of note, electronic medical records and administrative health data are using interchangeably in the article despite it is not exactly same types of health data. **Thank you; we have edited the text to improve clarity. (N/A)** 46) Line 76. The abbreviation "NAPCReN" is mentioned for the first time. This abbreviation is not written after the full terminology at the beginning of the section. **Thank you, we have expanded on this acronym for the name of this network at its first usage. (4)** 47) It is better to add goal/objectives of the study in the Background/ Methods section of the abstract **Thank you. This has been addressed. (3)** 48) The assumed goal ("to describe the population") does not correspond one of the stated methods (regression); regression is not a descriptive method. **Thank you we have re-written the paper extensively. Our choice of regression is discussed in the methods. (5, 6)** 49) The Results section requires more clarity on how the authors obtained their data. I would rephrase the first sentence (lines 80-81). Since EOSS is not as commonly used as BMI, it would be better to briefly describe what is EOSS and what factors are used to categorize patients in classes. Without this description, it is difficult to understand how 97.7% patients with 11-18% missing data for comorbidities were assigned EOSS scores and whether this could affect the results. Of note, eighteen percent is a substantial (almost 1/5) proportion of missing data and normally the attempt to impute data is advisable. **Thank you we have extensively re-written the text to address these points. (3, 5)** 50) In addition, there is no description of what CPCSSN Data Presentation Tool is

and how it is related to the current study or on its validation for another dataset (lines 84-85).
Thank you. We have expanded upon this in the methods. (6)

51) The first sentence of the Interpretation section (lines 87-88) is unclear: how was this demonstrated? I suggest that the information on risks or risk stratification system in the above sections is added. The rest of the section would also gain from more detailed description
Thank you. We have rewritten this section. (7, 8)

Introduction

52) I would suggest to double check syntax and grammar throughout the manuscript. As an example, the sentence in lines 120-121 of the Intro, "Obesity is a highly prevalent disease, defined as abnormal or excess adiposity that is causing physical or metabolic harm", would have benefited from restructuring to put a definition first and than a statement that obesity is a highly prevalent disease.
Thank you. This has been done (N/A)

53) Lines 125-126. It would be helpful to know where and for what subpopulation the value of EOSS as a predictor of mortality was demonstrated (e.g., North America/worldwide/in primary care patients).
We have added additional citations where this has been demonstrated. (4, 7, 8)

54) Also, was it demonstrated in one study only? How reliable was this study?
Subsequent to Padwal's CMAJ article there have been further studies. We have expanded on the references. (4, 7, 8)

55) I would suggest to re-write the introduction for better clarity. In my view, the reader would like to have more information about what EOSS is, why is it better than BMI to predict mortality, and, briefly, what patients' characteristics participate in calculating EOSS score. This would highlight the importance of the topic.
Thank you. The introduction has been rewritten to reflect your suggestions. (4)

56) Line 131 -Same comment as for the Abstract: EMR is not exactly the same time of data as HA data.
Thank you for this comment. We have changed our wording throughout. We absolutely agree that AHD and EMR data are not the same. (N/A)

57) I would also suggest to first state that a useful tool is needed, and than move on to the description why this useful tool needs to be in a form of a dashboard, since many other tools could be possibly considered to help with the problem.
Thank you. This has been addressed. (4)

58) I would also suggest to clearly state the study goal/research question at the end of the introduction.
This has been addressed. (4)

Methods

59) Line 139 -It would be helpful to mention in the abstract that NAPCReN is one of the networks contributing to CPCSSN.
We have clarified this in the methods. Due to word limits, we have not expanded on this in the abstract. (4)

60) A description of CPCSSN/NAPCReN database is a bit vague. There are several CPCSSN studies published in CMAJ to get an idea of how it needs to be described.
Thank you. We have elaborated further based on examples from the literature. (4)

61) Line 150 The sentence "As BMI validation measures were not in place at the time of extraction" needs more clarity.
This was poorly stated. We have removed this line from the manuscript. Removing patients with a BMI >60 kg/m^2 was a clinical decision. This has been explained further in the manuscript. (4, 5)

62) Line 175. The algorithms for the diseases not having CPCSSN-validated algorithms deserve more detailed description. It should be stated whether these algorithms were previously used in published peer-reviewed studies or validated by the authors.
We have expanded upon this in the methods and noted this as a limitation. (5, 8)

63) Lines 180-183. This is a bit confusing. I believe, there is a big difference whether minimum or maximum value is used to calculate the comorbidity score. This should be clarified.
We agree that using the minimum versus maximum score for lab results would lead to different results. Due to the number of comorbidities included, we have been specific whether the minimum for maximum results were used for each comorbidity. For results where high values indicated worse health status, the maximum value was used. For results where low values indicated worse health status, the minimum value was used. These are indicated in Appendix 1. (Appendix 1)

64) In addition, missing data on individual EOSS scores for some comorbidities needs to be discussed as a limitation of the study (potential bias).
Thank you. We note that missing comorbidity EOSS scores require clinical inquiry. This has been presented in the methods and discussed further in interpretation. (4, 7, 8)

65) Lines 185-187. The classification system for obesity classes used by the authors should be references (e.g., WHO).
A citation has been added for the WHO classifications. (5)

66) Line 191. This is the first mentioning that least squares regression was used to describe relationships between the variables. Evaluating associations should be reflected in the research questions in the Introduction section of the manuscript and in the Abstract. In addition, if stepwise approach is mentioned, it deserves further details: forward, backward, based on p-values or otherwise, etc.
Thank you for pointing this out. We have now included the relevant

information in the abstract about the variables and that this is stepwise regression (although not based on p-value selection, as there are only 3 variables and the statistical method is arbitrary, but based on our conceptual frame which is more appropriate for the EOSS typology). This has also been clarified in the text. (5, 6)

67) Line 194. The abbreviation CPCSSN, accompanied by the full terminology, is mentioned several times throughout the article which is unnecessary.
This has been addressed. (N/A)

68) Line 196. Same comment about EMR. The authors go from the abbreviation to the full terminology throughout the article, the style should be consistent.
This has been addressed. (N/A)

69) Lines 195-197. The language is suboptimal, I would suggest rephrasing. More important, this description, in my view, should be moved to the description of the database at the beginning of the Methods section.
We have rewritten this section. (4-6)

70) Line 208. My apologies if I am incorrect but, in my experience, bigger proportion of BMI data than 14% are missing in CPCSSN database for national data. However, it might be different for the Alberta network.
We have added a flow diagram. In the Alberta dataset, 13.7% of patients are missing BMI data. We have added a new table of the EOSS values in those patients. (Figure 2, Table 2)

71) Also, when the authors say "58,672 patients", do they mean only obese patients?
This number includes all patients in NAPCReN >= 18. We have included a flow diagram to show how we moved from 58,672 to 22,460. (Figure 2)

72) Lines 205-206. redundant, it was already mentioned in the above section.
This line has been removed. (N/A)

73) Lines 208. I am not sure I understand how from 58,672 patients the sample was reduced to 23,460 patients. The steps of exclusion should be clear from the description: how many patients were of all weight groups, how many (or proportion) were obese, how many excluded due to missing data.
A flow diagram has been added to show how we excluded patients and how many patients were excluded at each step. (Figure 2)

74) Lines 211-212: This should be discussed in study limitations section.
This has been moved to the limitation section. (8)

75) Lines 226. "Sex and BMI all" -do you mean "both of these variables?" or you refer to some kind of synergic interactions? It is unclear.
We can see how this is confusing. It should only say "Sex and BMI", not "Sex and BMI all." We are not looking at the interactions between these variables. This has been addressed. (5, Table 3)

76) Lines 229-231. It is a bit unclear how the authors define the quality of risk

stratification tool. There should be operational definitions of "risk", "risk stratification" and "risk stratification tool" used by the authors. Without these definitions, it is difficult to understand the meaning of the sentence on lines 230-231. I would also suggest a table that compare how BMI obesity classes and EOSS categories each associated with risks for mortality, with reference to published studies.

Thank you. We have rewritten the section and endeavored to clarify that we are talking about EOSS as a clinical tool not as risk calculator. We have provided more citations around EOSS and clinical outcomes. (4, Figure 4, 7,8)

77) Lines 233-234. Redundant, this has been mentioned in above sections.
This has been addressed. (N/A)

Interpretation
78) Lines 231-242. This is the first time the ALT and triglycerides data are mentioned. If those are important characteristics, they need to be mentioned earlier in the manuscript.
Thank you. We have rewritten the section to make it more clear why we are mentioning these measures. ALT and triglycerides are included in Appendix 1. (Appendix 1, 7,8)

In general, traditionally, the first paragraph should re-state the problem and the summary of findings. Then there should be a description of findings in details and how they get aligned with the previous findings/literature. I suggest restructuring the Interpretation in this way.
Thank you. We have rewritten the section. (7, 8)

79) Line 250. I suggest rephrasing this sentence.
This sentence has been reworked. (7)

80) Lines 251-253. I think there might be a syntax error in these sentences, please double check. In addition, the end-organ damage is mentioned for the first time in the article. There should be some connection between EOSS score, mortality risks, and the end-organ damage (e.g., what comorbidities are meant and corresponding references).
Thank you. We have rewritten the section. Figure 1 has been added to show all stages of the EOSS staging system, including end-organ damage. (Figure 1)

81) Lines 254. I respectfully disagree that it has been demonstrated. In my view, more convincing description and interpretation should be provided. I suggest revising the whole paragraph (lines 254-260) to make sure that there are connections between the sentences and all the statements are referenced.
We have clarified that we mean a more informative clinical assessment. This sentence has been removed. (N/A)

82) Lines 261-262. It is a bit unclear what the authors mean by "clinical and administrative health data" in this paragraph.
We understand the confusion with this. We have replaced both these terms with electronic medical record data. (7)

83) Lines 263-267. I suggest changing to shorter sentences and to rephrase for better clarity.
This sentence has been split into two. (7)

84) Lines 269-270. Again, please double check what databases you refer to as "clinical" and "administrative".
This has been addressed. (7)

85) When limitations are discussed, it is better to mention in what direction these limitations can bias your results/not likely to bias your results.
Thank you. We appreciate that our intent of describing the creation of clinical tools to support the use of EOSS as a clinical staging system was not clear in the previous version. We have rewritten the paper to make this explicit. As such, we are not claiming "results" in the sense of a study, rather how to have more useful clinical information to inform patient care. (8)

86) Lines 278-281. Please double check for syntax and typos. For example, as far as I know, the data on income and education level are completely unavailable in CPCSSN database and not "reliably available"- I apologize if this is different for the Alberta network data. In my view, some other limitations should be discussed. For example, BMI has one advantage over EOSS score: it can be easily measured in 5 minutes in the family physician's office. Since authors state that their goal is to evaluate a feasibility, it is advised to discuss and compare how feasible it is to calculate EOSS score for a physician during the routine appointment, in comparison with BMI score. I suggest to also add a brief discussion on a feasibility of using EOSS score for risk stratification and decision making in practice and how the discussed dashboard can be helpful. If this dashboard is a new tool (as I understand from the article) then there may be no evidence on this topic and, as with any new risk stratification tool, a qualitative feasibility study is needed, that would include family physicians and other health care providers who are expected to use it.
Thank you for your interest in the dashboard. We can understand how previous wording made it seem as though we were assessing the feasibility of the dashboard itself. We have changed our terminology to state that we are not assessing the uptake and acceptability of the dashboard itself. What we did do was determine that a dashboard could be developed using data that is already routinely available. A great next step would be to complete a qualitative study that assesses the uptake and use of the dashboard within clinics. This is beyond this paper. (8)

Conclusion
87) Line 285. As I understand, this sentence may be the topic sentence of the article. If so, it should be stated as the research question in the introduction and the abstract.
Thank you. Variations of this sentence have been added as research questions in the abstract and introduction. (3, 4)

88) In my opinion, the sentence on lines 285-287 cannot be inferred from the present study and needs to be referenced by more than one reference, at least in the Introduction section.

We have rewritten this section and added references. (4)

Lines 288-289. I suggest changing to "may support" as the authors did not perform any feasibility studies to proof this.
This has been addressed. (8)

Other comments

89) 1.There are surprisingly few references. Unless it is a requirement of the journal, I would suggest adding important references, especially regarding the validation and use of EOSS tool, and CPCSSN database description.
Thank you. More references have been provided. (4, 7, 8)

90) 2. I am just curious why Python was used? The analysis described in the article is simple and could be done with the use of SPSS (or SAS/R).
Python is the preferred language of our team, even though--for this paper-- we are using only its basic functions. Of course, our most computationally intensive work benefits tremendously from the flexibility of Python. We double checked our regression modelling and results via Stata. If you and the editorial team believe we would remove this from the paper, we are more than happy to do so. We wanted to be transparent about the technology but understand it may not be relevant. (N/A)

Tables

91) Table 1. The table looks a bit unusual; however, it is clear what authors want to state.
I would change "Characteristic" to "Patients' characteristics".
This has been addressed. (Table 1)

92) Table 2
This table looks a bit unusual for a regression table. It is unclear from the title, if the data on 528 of 22,932 patients are missing or whether the data on 22,932 patients are complete. I am just curious why the authors want to only present R2 values and not OR data, as it is traditionally presented for regression analysis results -please explain, if possible.
528 of the entire study population (N=23460) did not have an EOSS score available and thus only 22932 were included in the model. We have clarified this in the table note. Our choice of OLS regression have been explained further in the manuscript. (5,6, Table 3)

It is a bit unclear to me why the authors have chosen to perform this kind of analysis. I suggest that the explanation is added for the justification of why the outcome of interest is % variance to be explained by Age, Sex and BMI and how does it help to prove a feasibility to calculate EOSS score in EMR database, which is the aim of the study. If the authors planned to compare feasibility of calculating EOSS versus calculating BMI from EMR, this analysis does not add anything to the conclusions. This should be explained.
Thank you for this comment. Justification of our methods have been added. We did not want to compare feasibility of calculating EOSS versus BMI from routinely collected data sources. (5,6, Table 3)

93) Figure 1: If I understand correctly, this is just % of missing data on the comorbidities? Will it be clearer to just present this graph as a % of missing data?
Thank you for this comment. We have relabeled the bars to "unavailable" which should help clarify the confusion around missingness of data. We have also combined Figure 1 and 2. These are now both Figure 3. (Figure 3)

94) Figure 2.
IN my view, there are unclarities in this graph. For example, how have been left-to right order chosen? It does not seem to be from low to high %. Also, how the colors were chosen? Usually, green represents close to normal and red is an alert. The authors may want to double check on their choice of colors.
Thank you for this comment. We have changed our colour choices based on your recommendations - we absolutely agree. We have also chose to combine Figure 1 and 2 in a way that shows what proportion we are describing. The left to right order was chosen from lowest to highest EOSS stages (0 on the left, 3 on the right). (Figure 3)

95) Figure 3.
Same comment regarding the colors.
We have modified our colour choice. (Figure 4)

Overall, the figure is a bit confusing. It is unclear to me what the authors want to say by the discrepancy between BMI classes and EOSS category. Was low class obesity by BMI expected to be similar to the low class EOSS, and study results showed otherwise? What is the rational for this, and how does this matter in terms of measuring EOSS feasibility (which is a study goal)? Please, explain your rational for this comparison with more clarity.
We had clarified our study goals in the introduction - we understand how the EOSS feasibility did not align with what we presented in our manuscript. This figure helps to show the relationship between EOSS and BMI. If higher BMI was correlated with greater risk of disease, we would expect the proportion of patients in higher BMI categories to proportionally increase with higher EOSS. Although there are slight increases, the proportions are very similar between each BMI class. (Figure 4)

96) Appendix 1.
I would move this table to the main tables; alternatively, I would put a brief description in the text. It is important for the reader to understand how the EOSS scores are calculated.
Thank you. We have added this detail. (5, Appendix 1)

97) Appendix 2.

I would absolutely advice to move this figure to the main figures. This is the first figure where the reader sees what is meant by the "EOSS Dashboard" which is the main topic of the article.
Thank you we have reformatted the paper. As much, this has been left as an appendix. (Appendix 2)

In terms of representation of the dashboard, I find this figure a bit confusing. The

	font is very small. There are no explanations for the different parts of the graph. What is very important, I have not found an explanation in the text of what the dashboard is and how it is supposed to be used. From there text, I understood that the dashboard should be useful for clinical practice. But there is a proportion of male/female on the graph. Therefore, it does not seem to be an instrument for calculation EOSS scores of a patient during doctors' appointment. If it is expected to be used by policy makers to have an idea what are the scores of a population/group of populations, it should be clear from the article. **We have revised the figures and the description to provide clarity. We have expanded our description in the text. (6, Appendix 2)**
Reviewer 2	Dr. Catherine Birken
Institution	Hospital for Sick Children, Toronto, Ont.
General comments (author response in bold)	Background 98) The authors frequently cite the relationship between use of EOSS and association with morbidity from previous work, although it is not clear if the version of EOSS used in this study was associated with this health outcome. Please clarify. **We have expanded upon the literature around EOSS which has consistently shown similar outcomes and we have added to the limitations section. (4, 7, 8)** 99) There are very limited references throughout the background = please cite references to obesity classes, as well as the EOSS version used in this study. **We have added additional information in regarding EOSS and have provided appropriate references. The WHO BMI classification reference has been provided along with Padwal et al, which we based our methodology off of. (4, 5)** 100) Please clarify the purpose of the dashboard –is it so clinicians can see the characteristics of their practices? Or is the dashboard available for each patient's results and displayed in their EMR? Please clarify the goals of the dashboard. **We have elaborated on the dashboard and explained how it can be used in practice to manage patient panels. (4, 6-8)** 101) Please articulate the primary and secondary objectives of this study. **Thank you, we have reworked the paper and the objectives of this study have been more clearly stated. (4)** Methods 102) Please state the analysis plan aligned to each study objectives. **This has been added. (5, 6)** The least squares regression – I am uncertain of the purpose of this analysis. **Thank you for this comment. We have provided further clarification in the text as to why we chose OLS regression. (5)** Results 103) Please provide a flowchart for patient inclusion in this study. It is unclear how

the sample size went from 58,672 to 23,460.
A flow diagram has been added. (Figure 2)

104) Are the authors able to describe the characteristics of the patients who did not have BMI data?
We have done this and added it in as an appendix. (Table 2)

105) Appropriately, the main analysis of this study are descriptive. Several times the authors make statements about the relative comparisons without suitable analysis. For example line 231 - The analysis used does not determine that EOSS was a better stratification tool - these results are simply descriptive and I would interpret them in this way. There was no formal statistical method used.
Thank you for this comment. We can understand how the previous phrasing was misleading. We have changed the sentence to say that it provided a more comprehensive assessment of patient comorbidities as this is inherent in the calculation of EOSS stages. (8)

Discussion

106) Line 239 – the authors summarize that 97.7% of patients with were able to be assigned a EOSS score. However, this appears to be 97.7% of those included in the study – this does not take into consideration those patients who didn't have BMI data available or were excluded. Please clarify.
We have added in a flow diagram which will help to show who was included in analysis and how was excluded due to missing measurements. (Figure 2)

Conclusion

107) Line 293 - whereas the use of the dashboard to improve identification and management of obesity related comorbidities is a laudable goal of implementation of EOSS in clinical systems - this remains to be studied - I suggest the authors recommend that this study is the first step.
Thank you. We have made the change. (4, 8)

108) The authors did not really assess the feasibility of acceptability of the dashboard in this study. I suggest removing this from the study objective
Thank you we have reformulated the paper to increase clarity. (4, 8)

109) Figure 2 requires better title describing the dashboard
Thank you. Our Figure 2 has been updated and a new title provided. (Figure 3)