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Title	Hypertension screening and follow up in children and adolescents in a Canadian primary care population sample: a cross sectional study
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Reviewer 1	Götz Gelbrich
Institution	University of Leipzig, Germany
General comments (author response in bold)	As this is a descriptive work, it is probably too brave to draw any conclusion. This part, and so the discussion, could be kept at a minimum. Instead, more data should be presented. 1. I would appreciate seeing the relationships between measured BP values and the probability that BP had been measured at another visit of the same patient. You only reported Prob[BP measured again in a subsequent visit|BP abnormal]. In a GEE model with a visit as statistical unit, you might examine Prob[BP measured|BP high in same patient at any time]. I feel this is important when speaking about under-recognition. High BP might indicate problematic patients, and it might be under-recognition if BP was not measured even in a preceding visit. **We respectfully disagree. This study was intended to provide information on follow up of abnormal blood pressures. We therefore provide information on whether an additional blood pressure was measured after an initial abnormal blood pressure. We also provide data on the prevalence of any abnormal blood pressures.** 2. Include effect of family history, BMI and the fact whether BMI was recorded, on BP values. (You may use categorised BMI and add a category "not recorded", so the whole stuff can be done in one model.) **The main study outcome is BP screening and rate of BP measurement; relationship between actual BP values and other factors is outside of the scope of this study. We are not sure if the space limitation would allow us to do additional analysis and add other materials to the paper in form of text and table to discuss findings that could be important or interesting but in our view are not directly related to study objectives.** **The current Poisson GEE model presented in Table 2 incorporates BMI centiles in the model as a continuous variable. We can include a missing value indicator variable in the regression model, but this would then require categorizing BMI centiles (normal, overweight, obese, missing). Categorization may entail loss of information compared to the use of BMI centiles as a continuous variable; as such we are more comfortable proceeding with the current analytic approach.** 3. I also wonder if physicians only measured BP or also looked up the percentile in the charts. The sole measurement would not be HT screening if it is not assessed whether the measured value represents HT. If you cannot say whether physicians were aware of the centiles, list this as a limitation (and explain to the reader why). **We cannot say whether the physicians were aware of the centiles. This was not measured as part of the study. Most EMRs do not provide information on BP centiles automatically. A note mentioning this and discussing the fact that it could be part of further studies was added to the paper.** 4. Did you record the (categorised) reasons for the visits? When BP was measured prior to a vaccination, I would not be surprised about an elevated value. In case of illness, BP might be checked to exclude too a high abnormality, but a small abnormality might be considered still normal in the given condition without triggering HT surveillance. **We did not record reasons for visits. The paper focuses on rates of screening. We would expect that, as previously published, a majority of abnormal BP readings will be found to be normal in subsequent visits. Most abnormal readings should simply be repeated in a timely manner.** 5. In the revision, please provide readable tables. Please understand that I am reluctant to print and cut them out and put the pieces together on my desk. Take care that all parts of one table are on one sheet, or better, use landscape orientation; each table should then fit on one page with appropriate column widths. **We hope CMAJ's editors can help us with this**
Reviewer 2	Maureen Mayhew
Institution	University of British Columbia, Canada
General comments (author response in bold)	The paper is well-written but substance is lacking. Findings are not compelling. 1. Given the paucity of evidence regarding the value and meaning of routine BP measurement in children, whether it is being done seems an inappropriate research question. **We respectfully disagree; the need for routine screening of BP in children is a controversial subject, and this is reflected in differences in guideline recommendations or lack of recommendations (please see this commentary as an example: Brady TM, Redwine KM, Flynn JT. Screening blood pressure measurement in children: Are we saving lives? Paediatric Nephrology. 2014;29:947 as an example). We wanted to know, given the controversy and differences in guidelines, what proportion of children receive BP measurement as part of their regular care. In addition, despite controversy on the value and effectiveness of regular screening for paediatric hypertension, there is no controversy on the need for follow-up blood pressure measurements once a higher than normal BP reading occurs. Considering the fact that determining higher than normal BP in children is more difficult than adults, we wanted to know what proportion of these children are being**

	identified and receive proper follow up and management. 2. The modeling is inadequately explained. **We agree that it is complex. We are limited by the word count. We can provide more details in an appendix if requested.** 3. The framework behind the explanatory variables was not adequately explained. In fact, there is no mention of the framework used - whether lit review etc. **We used the recent work by the US Preventive Services Task Force on paediatric hypertension screening and US. As mentioned in the text, these were variables used in their report.** 4. Were the variables used ones that were easily obtained from EMR? **We were limited by what was available in the EMR. CPCSSN is increasingly sophisticated, and is less limited by what is easily obtainable in the EMR. We can use more difficult data; however, EMR data quality continues to be suboptimal and CPCSSN data processing include extensive data cleaning and coding that makes data ready for analysis.**
Reviewer 3	Darren Lau
Institution	University of Alberta, Canada
General comments (author response in bold)	The authors examined the correlates of blood pressure measurement as a process of care among paediatric patients, using EMR data from 79 family practices in Toronto. The rate of BP recording was 62% (>= 1 measurement). Of the entire cohort, 8% met criteria for hypertension, of whom very few (5%) had a follow-up BP measured within 6 months. 1. The authors point out that there is little evidence for treating paediatric hypertension (introduction). Few physicians may check paediatric blood pressures because it does not change management – obese children would already be recommended lifestyle changes, do paediatricians actually treat hypertension per se? It may be helpful to include some data on the natural history of paediatric hypertension, especially whether it is independently associated with outcomes. There may be unintended consequences of labelling children as hypertensive, especially e.g.: for insurance coverage later in life. **Children could have secondary causes of hypertension. In pre-adolescents, the finding of hypertension (without obesity) might suggest the need for further investigation for secondary causes. There are published studies that suggest the rate of primary hypertension in children may be increasing along with rise in incidence of obesity. As well, children and adolescents are treated for hypertension using medications. If a child has an established hypertension, proper investigation is needed to determine the nature of hypertension (primary versus secondary), followed by management through lifestyle changes or medication.** 2. In methods, I am unclear about the extraction period of the study. When was the study start date? Please provide more data about the composition of the sample including the number of included providers and practices. I agree with the decision to include children and adolescents with minimum of two office visits at least six months apart – I presume this was intended to screen out patients seeing a family physician for ad hoc specific complaints, for whom the family physician may not have a particular role in preventive care. **We thank the reviewer for these comments. We specifically included children with visits six months apart to exclude transient patients with inactive charts. 79 providers were included. The duration of observation is provided in table 2.** 3. Are the included family practices all on the same EMR? **No, practices from three different EMRs are included (Nightingale, Practice Solutions, and Bell EMR)** 4. I agree with the use of GEE to account for provider-level clustering, and over-dispersion corrections in the provider-level analysis. In the patient-level analysis, I wonder if there has been enough account for patient-level clustering. E.g.: Could a few older patients in each practice with a large number of visits and a large number of blood pressure measurements create associations between age and BP measurement rate? Would the p-values then be under-estimated? It is probably true that the effect on results is small (but difficult to gues-timate), but logistic regression using "one or more BP measurements" or some other similar outcome would side-step this issue; the time under follow-up could be introduced into the logistic regression model as a covariate. **This was a complex analysis and we have removed significant parts of draft manuscript to meet word-count limitations. Adding a logistic regression would force us to remove other parts, but we are prepared to include this in an appendix if editors feel that could be helpful. We included the time under follow up in table 2 (table 1 was renamed table 2) as the duration of observation in years (denominator). We provide rates of blood pressure screening.** 5. Regarding table 1, I'm guessing the rate (number of BP measures / years of observation) for each patient was used to divide patients into the columns shown. This could be made more clear. Virtually none of the data shown in Table 1 is referenced in the results or discussion. The mean / median rate of BP measurements should be mentioned in text. Could Table 1 be condensed down into fewer columns – e.g.: above median vs below median? **This was extensively discussed with our statisticians during the study analysis. We feel that providing rates as shown (table 1 has been renamed table 2) is the most correct interpretation of data. We have provided additional details in the body of the paper.** 6. The significant RRs shown in Table 2 are mentioned in the abstract but not in the results section. Some highlighting of the key findings from Table 2 should be mentioned in results. The associations found in Table 2 look very miniscule, but if multiplied over, say, 5 years (the standard deviation in age range), become a little more substantial (e.g.: IRR = 1.34 more likely to have BP measurement for a 5-year increment in age at first encounter age). Some interpretation of these findings is warranted,

	either in results or in discussion. **We have added RRs in results. We agree that larger associations will be found if a wider age range or a greater number of years of observation is used. We have not focused on this as it was not a primary research question, but did use the results for adjustment.** 7. The intra-class correlation coefficient mentioned in Table 2 should also be mentioned as an important result. Are we able to get ICCs as well for the patient-level variance? The ICC is important, because it highlights a lot of idiosyncracy in physician practices that are not accounted for by the measured variables, and unlikely to be accounted for by measured patient factors since none of them were substantial (despite being statistically significant) predictors of BP measurement. The interpretation brings home the point that, as family physicians, we are somewhat okay at measuring paediatric BPs (62%), but when we find an abnormal results, we rarely follow it up – less so than our US counterparts. These are certainly interesting results! **We agree with the reviewer that the high ICC at the provider level is an interesting finding indicating a moderate degree of variability in the response (i.e. number of BP measures on a given paediatric patient) across providers. In other words, some providers take/record many BP measures on their paediatric patients whereas others perform/record few BP measures – and these provider care patterns seem to be fairly correlated within their practice. That said, in the analysis in Table 3 (formerly 2), the outcome is number of BP measures. As such, there is no patient level ICC. The patient is the unit of analysis, and each patient has a single measure denoting the number of BP's observed. It is possible to conceptualize the problem in a more multi-level framework as the reviewer has suggested: for example, consider patient-clinician encounter with a BP measured (a binary event) as unit of analysis rather than a count summed over all of the encounters. Under this approach these BP measures would be nested within patients (level-2), who are themselves nested within providers (level-3). As such a provider level ICC and a patient level ICC would be calculable. Under the current conceptualization of the problem there is only one ICC as patient and their (aggregated/summed) BP counts are nested within providers; we view this suggestion out of scope for the manuscript (i.e. it answers a different question, using a different primary response variable).** Minor 9. Page 5 "We used descriptive statistics to characterize the sample" seems needless. **This has been removed.** Additional changes: **The footnote of Table 3 (second and third line) was changed; the revised parts are in red.**