Article details: 2015-0049	
Title	An analytic approach for describing and prioritizing health inequalities at the local level in Canada: a descriptive study
Authors	Cory Neudorf MD MHSc, Daniel Fuller PhD, Jennifer Cushon PhD, Riley Glew MPH MSc, Hollie Turner MA, Cristina Ugolini MPA
Reviewer 1	Patricia O'Campo
Institution	St. Michael's Hospital, Centre for Research on Inner City Health
General comments (author response in bold)	In this paper, the authors present details about an analytic approach they took to document health inequalities in Saskatchewan. What seems to be missing from the paper, however, is more information about the rationale for the specific measures they chose to examine. In particular, how do those specific variables inform our understanding of health inequalities and how to intervene based on their findings? **Response:** We understand the question to mean why we chose the Index of Deprivation developed by Pampalon et al., rather than other measures of socioeconomic status. The primary reason we chose the Pampalon Index is availability and common use in Canada. While we agree there are many measures of socioeconomic status with various implications for understanding our concern for creating a replicable index outweighed more theoretical **concerns, as stated "As well, all data are available to local health authorities across Canada making the analysis replicable."** As discussed in R1C4 this approach provides information on where to intervene (the assessment area of Urban HEART), it is not our intent with this method to provide specific guidance on how to intervene (the implementation are of Urban HEART). Our approach measures health inequalities, not inequities. This implicitly assumes that the most prevalent and unequal health outcomes between SES groups are unjust and should be acted upon. However, as we discuss, exceptions to this assumption should include low prevalence, high inequality outcomes. Local health authorities and groups using this approach must be aware of the implicit assumptions in order to apply value judgments about what is unjust in order to justify action. Additionally, the IPM applies equal weights to all inequality measures, which could be altered based on the value users may place on appropriateness of the different measures. We used equal weights for all inequality measures because we did not decide a priori that some inequality measures were inherently superior to others. A major gap that seems to be missing is the notion of social determinants of health as health inequalities are generated by social inequalities more so that health care inequalities. **Response:** We have included a brief discussion of this point in the limitations section: Addressing inequalities in health care utilization does not directly address the fundamental causes or social determinants of health that structure inequalities. However, we believe reducing inequalities in health service utilization is an important objective. It would have been helpful to have the authors articulate their overarching framework informing this work as that too would be useful in considering how to design interventions to address growing inequities. **Response:** See R1C4 below. We have linked our approach with the WHO Urban HEART. Our approach considers only one small aspect of Urban HEART, rather than a fully articulated conceptual framework related to intervention. It is curious that the authors did not compare their work to existing frameworks and analytic approaches for measuring, documenting and acting upon health inequalities such as the World Health Organizations Urban HEART tool developed by Sir Professor Michael Marmot and colleagues. While that tool is designed for urban settings, the overall framework identifies the specific domains and pathways (and associated metrics and interventions) by which inequalities are generated and can be addressed. This framework has been applied in dozens of countries and while it was originally applied in low to middle income countries, it has been adapted to high income countries and applied in Canada as well. How does the approach proposed by the authors compare to similar existing approaches (e.g., Urban HEART) and what are the advantages of their approach over these existing frameworks? **Response:** We have included some discussion in the introduction of how our approach relates to the WHO Urban HEART: The conceptualization of the approach we have developed is based on the World Health Organization Urban HEART (Health Equity Assessment and Response Tool). The Urban HEART tool conceptualizes a cyclical tool with four areas; Defining the Problem (Assessment), Setting the Agenda (Response), Developing Policy (Policy), and the Implementing the Program (Program). The analytical

approach we have developed is a part of the assessment area of the Urban HEART conceptualization, adapted to the Canadian context and available Canadian data, while also providing a broader range of inequality measures than those suggested by the Urban HEART tool.

The province is very broad in terms of geographic setting. What are the advantages or limitations of generating data for the province as a whole versus more local areas where services and health planning are likely to be designed?

Response: This analysis is not at the provincial level, rather we analyzed data for the Saskatoon Health Region. We are current preparing a provincial report with different indicators and inequalities measures that are more relevant to the province.

"Data" is plural and this should be corrected throughout the manuscript.

Response: Correct throughout the manuscript.

In the method, page 8, first paragraph, the authors assume the reader will know what several of the terms mean such as medical services data base, shadow billings, or most responsible diagnosis to name a few. The authors might use plain language to explain these ideas.

Response: We have included descriptions.

The authors might provide some rationale for choice of variables. Were these outcomes chosen due to availability of data or was there some framework informing their choice of variables? Also, provide justification for the use of 2006 Census data for their deprivation index since later data are available.

Response: We have included justification: We used the 2006 census because of data quality concerns for the 2011 National Household Survey.

Reviewer 2	Leslie Roos
Institution	Manitoba Centre for Health Policy, John Buhler Res Ctre/Univ of Manitoba
General comments (author response in bold)	I don't believe the manuscript is suitable for a journal oriented towards physicians. The measures used are not 'attuned' to anything physicians are normally sensitive to. As currently written, the gap in language is too great. A health policy journal might prove a better choice. I am sympathetic to health planners efforts to make inferences from administrative data! **Response:** We respectfully disagree. The CMAJ is a broad medial journal which states "CMAJ has had substantial impact on health care and the practice of medicine in Canada and around the world." Physicians must know that large gaps exist in service utilization and that some of solutions to address these gaps are at the individual physician level. Also, medical health officers can apply this approach in their areas to better understand the extent of the problem. The tables seem too complicated and the writing style a bit turgid. **Response:** We have converted Table 2 to a figure and edited sections of the manuscript for clarity. I am concerned about between 30 and 40 percent of the physician claims being based on shadow billing. There are ways to partially check these claims. A formal audit process has not been done. Even without an audit, diagnoses from 'shadow billing' could be compared with those on subsequent hospitalizations. Diagnoses from 'shadow billing' could be aggregated from each physician to indicate which physicians might be goofing around. **Response:** We agree that shadow billing is a problem, in particular for Saskatchewan, because the quality of validation for shadow billing varies by province.1 We discuss the limitations of the physician billing in detail in the limitations section. There is no formal method to check shadow billing in Saskatchewan and it is not our objective to conduct this type of analysis. 1 Because our objectives are first to present the approach, and second to make comparisons between socioeconomic groups the most important concern is differential shadow billing by the socioeconomic status of the patient. To our knowledge, there is not research examining differential shadow billing by socioeconomic status. We have updated the limitations section around shadow billing: Shadow billing differences between SES could bias the results. It is not known whether SES differences in shadow billing exist. 1. Cunningham CT, Cai P, Topps D, Svenson LW, Jetté N, Quan H. Mining rich health data from Canadian physician claims: features and face validity. BMC Research Notes.

	2014;7(1):682. doi:10.1186/1756-0500-7-682. Physician turnover and change in practice style over time might well affect the diagnoses. With the number of years involved, turnover is almost certain to be a major issue. Turnover may differ among the different 'practice areas' in Saskatoon. Such differences among providers in diagnostic practice are well established in American data. JE Wennberg is coauthor of several studies highlighting this problem. See, for example, Song et al., Regional variations in diagnostic practices, N ENGL J MED, 2010 Jul 1; 363(1), 45-53. **Response:** Thank you. We have included this in the limitations section: Given the long period of study it is possible that physician turnover and changes in diagnostic practices over time could affect the results. Regardless of the publication outlet, I think the paper needs some consideration of the substantive issues which may be affected by physician coding. **Response:** We have included considerable discussion of the physician billing limitations in the limitations section.
Reviewer 3	Irfan Dhalla
Institution	University of Toronto, Department of Medicine
General comments (author response in bold)	The authors state clearly in the second paragraph of the paper that inequalities become inequities when the "differences are deemed unnecessary, avoidable, unjust and unfair." The authors also state that the IPM "can be used to assign value judgments about the equitable distribution of health outcomes by deprivation quintiles." However, the IPM is a mechanical formula that, at least as far as I can see, does not include explicit value judgments necessary to determine whether the differences are avoidable or unjust. A good example is TB, as noted below - is this an inequity, or simply an inequality? It might be helpful for the authors to more clearly state the value judgments that are implicit in the IPM, and when the formula might not work as well as is hoped. **Response:** We have addressed this comment (see R1C1): Our approach measures health inequalities, not inequities. This implicitly assumes that the most prevalent and unequal health outcomes between SES groups are unjust and should be acted upon. However, as we discuss, exceptions to this assumption should include low prevalence, high inequality outcomes. Local health authorities and groups using this approach must be aware of the implicit assumptions in order to apply value judgments about what is unjust in order to justify action. Additionally, the IPM applies equal weights to all inequality measures, which could be altered based on the value users may place on appropriateness of the different measures. We used equal weights for all inequality measures because we did not decide a priori that some inequality measures were inherently superior to others. The authors note that the IPM method gives different priorities depending on the data source. It would be helpful to many readers to more thoroughly explain why this is. **Response:** We have provided examples: The IPM analysis was conducted separately for each data source. This reflects the fact the data sources have different meanings (e.g., a hospitalization for diabetes in very different from visiting your family physician for diabetes), limitations, which make comparisons across data sources inappropriate (e.g., limitations related to physician shadow billing), and physicians, hospitals and provincial health departments having different priorities both in terms of addressing the most inequitable outcomes and potential interventions. Although hepatitis C can be transmitted through sexual activity, this is relatively rare. Suggesting that hepatitis C is the highest priority among STIs has the potential to be misleading. **Response: We have changed the description of hepatitis C to "communicable disease"** throughout the manuscript. Stating that all cause mortality is one of the highest priorities is somewhat confusing because all cause mortality, by definition, cannot be a priority. **Response:** The statement above is a common confusion and critique of using mortality **data for social inequalities research… everyone dies. The key is quantifying and** intervening to reduce differences in all cause mortality if they are present. Interventions that address that social determinants could reduce or eliminate differences in all cause mortality between socioeconomic groups (see R1C2): Differences in teen pregnancy and all cause mortality are the most inequitable in the vital statistics data. For communicable diseases, Hepatitis C is the highest priority. Figure 1 might be more easily understood if it used real data from real diseases rather than hypothetical curves.

Response: We have changed the figure and included diabetes (ALCC=0.39), Stroke (ALCC=0.17), and Cancer (ALCC=0.05).

Table 2 is very interesting. I wonder if the paper would be more interesting if the authors focused on this as their main result? I found it particularly interesting that inequality seems to have declined substantially between 1995 and 2011 for most of the causes of hospitalization that were examined, as well as for many of the vital statistics. It is also interesting that STIs and physician visits appear to be much more unequally distributed than hospitalizations and vital statistics outcomes. It may be interesting to explore this further.

Response: We agree the table is interesting. The challenge, and why we developed the IPM is summarizing the data in some way. Changes depend on the measure used to quantify inequality (Rate ratio, rate difference, and ALCC in this paper), and while we agree with the reviewer that inequalities in hospitalizations appear to have declined overall**, we don't think a visual inspection of the table goes far enough to help prioritize** outcomes for intervention.

The seven-step procedure used in the IPM seems fairly arbitrary. (For example, why are the weights equal? Why not just focus on the most recent ALCC data?) If it is indeed arbitrary, the authors might say so. If there is a solid rationale for the procedure, I would suggest the authors explain it more clearly. The authors note that they developed multiple iterations of the IPM. Did the other IPMs yield different rankings? Why did they choose this version over the others?

Response: We have included text related to these comments in the limitations section: Multiple iterations of the IPM were developed over the course of this project including using only rate ratios and rate differences, accounting for changes in area level concentration coefficient over time, and excluding the overall rate from the calculation. We believe the IPM provides sufficient nuance to prioritize conditions, while being replicable. However, we also encourage other provinces or health regions to test different specifications of the IPM and publish their findings.

Additionally, the IPM applies equal weights to all inequality measures, which could be altered based on the value users may place on appropriateness of the different measures. We used equal weights for all inequality measures because we did not decide a priori that some inequality measures were inherently superior to others.

The vital statistics section is confusing, for several reasons. First, it includes all cause mortality, and it is not clear how this can be prioritized. Second, it includes both teen pregnancy and teen abortion.

Response: We have clarified all cause mortality based on R3C4. Teen pregnancy and teen abortion are distinct outcomes. We do not feel there is confusion including both in the analysis.

Some of the rankings do not seem to me to relate to inequities (as opposed to inequalities). For example, why are TB rates higher among the poor than the rich? Is this in part because immigrants are more likely to be poor? If so, many observers would say that this is not necessarily unjust.

Response: We agree. The IPM prioritizes inequalities not inequities. It is up to the users of the tool to judge whether inequities are present and what should be done to act to reduce the inequality. The approach assumes that people will agree that the highest priority areas based on the IPM should be acted on. However, this may be not the case. See R1C1.

I don't understand why Hep C and TB should be of "particular concern" because they have the lowest prevalence. Shouldn't it be the other way around? That is, if all other things are equal, shouldn't we focus more on diseases with high prevalence?

Response: We have clarified this. There was some confusion between the prevalence and the degree of inequality: Despite the focus on highly unequal conditions with a high prevalence in the population, users of the analytic approach should pay special attention to those conditions with a degree of inequality based on the ALCC of greater than 0.5, regardless of prevalence. These extremely unequal outcomes may not respond well to population-level interventions and will likely require interventions based on the notion of vulnerable populations.(36,37) In Saskatoon, Hepatitis C and Tuberculosis are of particular concern because they have extremely high inequality, in particular among IV drug users and First Nations populations