Reviewer’s report

Title: A descriptive study of reportable gastrointestinal illnesses in Ontario, Canada, from 2007 to 2009

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Reviewer: Gordon Nichols

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A descriptive study of reportable gastrointestinal illnesses in Ontario, Canada, from 2007 to 2009

Comments on the revised manuscript

The authors have addressed most of the referee’s comments.
Under Point #5 I was unable to access case definitions from the website given.
Under Point #7 the under-ascertainment is pathogen dependent and is not a single figure for all pathogens.

Comments on the original manuscript

1. This is an interesting study that documents the occurrence of diagnosed and reported gastrointestinal infections in Ontario. It includes some data attributing the percentage of infections to primary sources and settings.

Discretionary Revisions

2. The authors have outlined the study purpose on page 4 line 11-15, identifying the publication of incidence, demographics, outcomes, seasonality and overall epidemiology with the goal of informing the development and evaluation of control policies and programmes. However there is no information on current and past control policies and programmes or their effectiveness. I understand that presenting current data is likely to inform future interventions but it is unclear in what way this current data changes the understanding of the situation in Ontario. The authors are advised to address this in the manuscript.

Major Compulsory Revisions

3. The paper relates to data from 2007 to 2009. However, there is only limited data on whether the individual pathogens have increased or decreased since the previous reports which document infections between 1997 and 2003 (Refs 5-7). This would be useful for those designing interventions to prioritise these. The data on page 11 line 8 provides insufficient temporal and organism specific data to easily understand long term trends and what might be contributing to these and this data should be strengthened.

4. The way the data is presented suggests that there could be changes in the
surveillance process that may have an impact on disease reporting. On page 15 line 8 it says that “a full discussion of these factors is beyond the scope of this paper.”. The authors need to provide a paragraph that identifies the changes in surveillance that are most likely to have influenced case reporting over recent years.

5. The diagnoses are assumed to be through laboratory diagnosis of the microbiological agent in clinical material. The diseases are reported rather than the pathogen. It is therefore slightly difficult to know how to interpret some of the diagnoses. For example, amoebiasis could be any laboratory diagnosed cases where Entamoeba histolytica is detected in the faeces. This would require specific testing to separate E. histolytica and E. dispar which are morphologically similar. In my experience few diagnostic laboratories will speciate these so such strains would be either E. histolytica or E. dispar. It could also refer to any amoeba that is discovered in the faeces (e.g. Ent. coli, Endolimax nana, Iodamoeba butschlii). For yersiniosis is the data for all Yersinia species and strains or restricted to Y. pseudotuberculosis and pathogenic types of Y. enterocolitica? Is the VTEC predominantly E. coli O157? The authors need to give case definitions of what these diagnoses refer to.

6. The commonest enteric infections are viruses (norovirus, sapovirus and rotavirus). None of these are included in this paper. I assume this is because there is either limited testing in laboratories or more likely poor or no overall surveillance reporting. The authors need to address this in the report.

7. The surveillance data represents differential reporting as exemplified by the ‘tip of the iceberg’ reference on page 15. However there are big differences in reporting between pathogens (Tam et al, 2012) which can affect overall disease burden and importance. The authors need to address this in identifying priorities that regulators and others can use for designing interventions.

8. The authors have provided five tables that examine the sources and settings of the selection of pathogens. There is some concern about the way the data is presented. I will take as an example campylobacteriosis. Of the 10,916 cases presented in Table 1 only 60% were followed up (this is actually quite a high rate for Campylobacter). In table 3 there were 10 outbreaks (without any numbers of cases involved), and in Table 4 the endemic sporadic cases were 4,982. For table 5 the sporadic endemic cases with a primary source is down to 1,272 cases and the primary sources (%) relate to this. This suggests that 63% of campylobacter infection is due to food, 26% is due to animals, 6% to person and 2.8% to water. However it is notoriously difficult to determine the source of sporadic Campylobacter infection by interview. If the cases are related to food (which I estimate to be 801) all are derived from a food source then this is 7.3% of all Campylobacter cases. I suspect the true level may be below this. My point here is not to rubbish the data, which is interesting to read. It is that the way this is presented may be misinterpreted and the discussion needs to highlight these problems and provide further description of what the results mean.
**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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