Author’s response to reviews

Title: Illness and Economic Costs Associated with Recreational Waterborne Illness on United States Surface Waters

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Author’s response to reviews:

Many thanks for your very constructive comments. As a result of the feedback provided, the manuscript has been revised, and, in our opinion, substantially improved. Responses to individual comments appear in bold font following each comment. The line numbers noted below refer to the revised clean copy of the manuscript.

Reviewer #1: The authors provide an extensive overview of the health and economic burden due to recreational water activities in the USA. The manuscript is very well written and flows in a logical and comprehensive way.

General comments:

* It is unfortunate that the underlying data refers to a large time span, with the main economic burden referring to 2007. Was it not possible to provide more up-to-date results?

RESPONSE: We chose to represent costs in 2007 dollars since it was the year in which the two epidemiology studies were conducted. In the discussion (page 16, lines 309-314) we have converted to cost to 2017 dollars for comparison.

* Why was the estimation of the cost of severe illness restricted to GI illnesses only?

RESPONSE: Data summaries from the CDC’s Waterborne Disease Surveillance System indicate that, most severe illnesses associated with water recreation are GI related. However, severe illnesses were not only restricted to GI illnesses, we included Vibrio (including Vibrio vulnificus...
which can cause severe skin infections on open wounds) as well as Naegleria fowleri which can lead to primary amoebic meningoencephalitis (PAM) (page 7, lines 106-108, and lines 113-115.

* Supplementary material, tables with parameters (e.g. Table S4 and S5). Did the authors make sure that the sum of mild/moderate/severe illness equals and does not exceed the total number of infected? How did they ensure this?

RESPONSE: In our analysis, we were focused on quantifying the number of water recreators, and subsequently the number of water recreators that become ill following water recreation. We were not able to differentiate individuals whose symptoms were due to infection vs. other causes; likewise, our reliance on symptom reporting means that we were not able to estimate the number of asymptomatic infections (pages 14-15, lines 271-274).

We included footnotes in table S4 to provide more detail about how the parameters were used in the analysis. (Supplement table S4).

* Supplementary material, tables with attributable fractions by illness: did the model foresee the possible state of co-morbidity (e.g. eye&ear infection)? If yes, how was it dealt with? If not, how did the authors ensure that the AF did not exceed 1?

RESPONSE: For the purpose of our analysis, we assumed that each illness was independent, but we do agree that it is possible for a recreator to have more than one illness following recreation. While this likely would not impact the attributable fraction (calculated based on the difference in the predicted probability of illness in the exposed compared to the unexposed), it could have an impact on cost. For example an individual would be most likely be charged for one physician office visit even if they were being seen by a doctor for GI symptoms in addition to respiratory symptoms. We have added this to the discussion (pages 14, lines 265-271).

* How is it possible that the total cost per severe case (0.30 million USD, Table S12) is less than the mild and moderate costs per case?

RESPONSE: Thank you for highlighting this opportunity to improve the clarity of the manuscript. While as demonstrated in Table 4, the total costs of severe illness (mean cost per case x number of cases) was less than the total costs of mild illness, the per-cost cases were far greater. The re-numbered Supplemental Tables S11-S13 differentiate total cost and cost per case.
* Methods, lines 98-105. If I understood correctly, the authors have applied a multiplication factor (MF) of 25.5 to the number of notified severe GI case, in order to account for under-estimation of cases. This MF was based on a study aiming at evaluating the under-estimation of all cases (mild to severe) during an outbreak, not only the severe ones. If this is the case, it seems that this is an important assumption, with the potential to grossly over-estimate the health and economic burden. Moreover, intuitively the MF (25.5) seems high for cases that visit the hospital and that should be captured by the surveillance system. If indeed the authors think that this high MF is justified, this should be better explained.

RESPONSE: The number of outbreak-associated cases of GI illness that are recognized by the passive surveillance systems in the US, is a small fraction of all outbreaks that occur. This is because for an outbreak to be recognized and a case to be linked to an outbreak, 1) the patient must see a clinician, 2) the clinician must order a stool sample analysis, 3) the sample must be tested for the relevant pathogen, and 4) positive culture results must be reported to public health authorities. Steps 1-3 are elements that lead to under diagnosis; step 4 is under reporting. The CDC FoodNet program has characterized rates of under-diagnosis and under-reporting by comparing by implementing local active surveillance systems (reaching out to clinicians to identify cases), and by comparing active and passive surveillances systems, developed multipliers for under-diagnosis and under-reporting, specific to a variety of pathogens. The under-diagnosis multipliers for a variety of bacterial pathogens is about 25 (each diagnosed case represents 25 cases in the community) (Scallan 2011 – Reference 14). (page 7, lines 98-99 and lines 110-113-).

* Methods, lines 111-112. To which GIs is this statement referred to? Why doubling, what is the rationale? How is doubling justified?

RESPONSE: Hospitalization data and death data (distinct from counts of the number of cases in communities) can also underestimate the number of GI cases caused by specific pathogens. For example, if patient hospitalized for (or died from) severe gastroenteritis due to norovirus, but a stool sample was not tested for norovirus, the hospital discharge or death data would not reflect the specific pathogen responsible for that hospitalization (or death). For that reason, FoodNet epidemiologists have used a multiplier, their best estimate, of the rate of under-diagnosis and reporting of pathogen-specific deaths and hospitalizations. (References 14, 17). We followed that approach, recognizing that this multiplier is subject to more uncertainty than the 25.5 multiplier for the number of cases of illness that occur in communities for each cases diagnosed and reported. This has been clarified in pages 7-8, lines 115-118).
* Methods, line 120. Sequelae are mentioned here, but no information is available as to the relative risk of developing these. Sequelae can be an extremely costly short and long-term consequence, so this should be better described, at least in the Supplementary material.

RESPONSE: Total excess costs per case due to the development of sequelae were obtained from studies of foodborne illness Studies by Scharff et al (Scharff, 2011; Scharff et al., 2009) developed costs estimates for gastrointestinal infections that included costs associated with sequella, such as Guillain-Barré syndrome (GBS) (Campylobacter), hemolytic-uremic syndrome (HUS) with or without end stage renal disease (ESRD) (E. coli), and reactive arthritis (ReA) (Campylobacter, Shigella). We applied Scharff’s costs to the severe illnesses for which these specific pathogens were identified. This has been updated in the methods (page 8, lines 134-135).

* Table 3: the concept of moderate severity is new and not explained in the text.

RESPONSE: For clarity, we changed “moderate severity” in the table to read as “moderate illness”. (Table 3)

* The total number of severe cases and total number of deaths are missing from the text/table of the main manuscript - readers will surely be interested in having this presented.

RESPONSE: We have incorporated these estimated numbers into the main text of the manuscript

Reviewer #2: This is an interesting paper and the authors attempt to provide a quantification of the economic costs associated with recreational waterborne illness on United States surface waters. The categorisation of illnesses into three groups is interesting and enables to tackle some of the challenges of the quantification of the health impacts in monetary unit.

This paper could be more complete if:

1) age groups were distinguished for health impacts as well. I understand that age groups were identified to determine the millions of person days and then the health impacts is determined based on the overall person days. However, the health impacts (and most likely the associated severity) won't be the same between a 3 year old child, an adult of say 30 years and an elderly of say 65 years old, as they don't have the same immune. I would expect different attributable risks based on the age groups. Not only, will the attributable risks be
different, I would also expect different economic costs associated with the illness based on the age groups, (loss in productivity for instance).

RESPONSE: We agree, we would not expect the risk of illness or the distribution of health impacts of illness to be equal across all different ages. The observed health impacts (direct medical costs, lost time, etc) for mild-and moderate illness were based on observational data at the individual level (in the NEEAR and CHEERS studies). For that reason we did not apply assumptions of equivalence in health impacts to our estimates of cost. We estimated the approximate proportion of mild and moderate illnesses by age group (0-10, 11-19, 20-54, and 55+) and have added this table (Table S9) to the supplemental material. In the results (page 10, lines 168-173), we highlight that moderate illnesses are more likely to affect the youngest age category (0-10) as well as the oldest age category (55+). Likewise, the severe illness category was based on observational data from CDC, and not assumptions of uniformity of health impacts by age were made.

We know less about the age distribution among those who develop severe (hospitalizations/death) illnesses following water recreation because these are rare events (no deaths and few hospitalizations were observed in the cohort studies which combined enrolled more than 30,000 participants). We have added these points to the discussion (page 15, lines 275-286).

2) the time frame considered was presented. I acknowledge that the incubation period of the illness is very short but what about how long does the disease last? (1-2 days for mild illness? 3-5 for moderate? XXX for severe?)

RESPONSE: In our calculation of cost, we considered the mean (and the 5th and 95th percentile of the distribution) number of days of work missed by illness category. Days of work missed are indicated for mild illnesses in Table S6 and for moderate illnesses in Table S7 (median of 1.04 and 1.28 days, respectively for GI illness). Time missed from work varied by pathogen for severe illnesses, and has been added to Table S8.

3) the authors strengthen the discussion regarding the added value (so to speak) of the overall message of the paper. I'm always puzzled to see estimates of health impacts irrespect of water quality monitoring data, however according to the authors, the attributable risks were drawn from NEEAR and CHEERS projects where the recreational surface water generally meet the local water quality criteria. Therefore, I'm not sure to have understood the message of the paper. The annual economic burden of illness due to surface-water recreation ranges from $2.2-3.7 billion but given that it seems that local water quality criteria are met, the authorities
need to reevaluate the water quality criteria? What would be the economic gains if the the water quality criteria is improve by say 10%? etc.

RESPONSE: The reviewer raises an excellent point. The data from CHEERS was almost entirely in exceedance of EPA criteria for swimming (though the CHEERS study focused on limited contact recreational activities) as has been detailed in a previous study (Dorevitch et al. 2015). In the NEEAR study, water quality generally was acceptable. NEEAR indicated that the risk of illnesses increases on days that bacteria concentrations are higher (Wade et al. 2010). It is reasonable to assume that as water quality improves, the economic burden would decrease, however an intervention study would be necessary in order to prove this. We have added this point to the discussion (pages 15-16, lines 287-300). Furthermore, we hope that future investigations build upon our work by estimating the health care costs saved as a function of improvements in water quality.

Overall, it's an interesting paper but the overall message would benefit from being enhanced.

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