Reviewer's report

Title: Lessons from a one-year Hospital-based Surveillance of Acute Respiratory Infections in Berlin- Options to optimize the SARI case definition

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Reviewer: James Fielding

Reviewer's report:

Summary

This study describes results from syndromic surveillance for respiratory infection in nine Berlin hospitals, and compares risk factors for different RI case definitions and SARI and for confirmed pandemic H1N1 influenza infection.

In general, the manuscript is well written but I think the clarity and focus could be improved by removing elements that attempt to evaluate the system. The data appear to be sound and the results are well presented. The study's limitations have been appropriately discussed, but I am also a little unclear as to the value of the findings from this paper and perhaps the authors could highlight their significance in a better way. A further concern I had was the absence of some statistics related to those who weren't tested and possible bias that this may introduce bias to the results. Specific revisions and comments regarding these concerns are listed below.

Discretionary revisions

1. The second last paragraph of the Methods section refers to a "Hospital Controlling Unit" that provided denominator data, although it is not clear what this unit is. Is it a central repository of all hospitalisations in Berlin?

2. I think Table 3 would be better shown as a chart showing percentage of all admissions to internal medicine/ICU by case definition over time.

Minor essential revisions

1. Acronyms such as 'SARI' should not be used in the article title.

2. I would suggest removing the first section of the results that qualitatively assesses the feasibility of SARI surveillance. It does not add much value to the paper with respect to the main objectives of the study as described in the last paragraph of the introduction. Rather, this paragraph would be better suited to a separate paper that formally evaluates the surveillance system.

3. In the case definitions subsection of the Methods, the criteria for the case definitions do not read well and gets a bit confusing; suggest to just refer to Figure 1.
4. In the Methods the authors should specify which statistical package was used.

5. The first sentence and parts of the last paragraph of the discussion should be removed for the reasons as described in point 2 above.

Major essential revisions

1. I found the overall objective and formulation of the study slightly confusing. Whilst a specific SARI surveillance system may have been established during the pandemic, this study uses data from a broader RI surveillance system (that includes SARI) and so I think it would be useful to reword the title to reflect this. I suspect this has arisen because part of the paper looks at assessing the feasibility of establishing the surveillance system (which I've suggested above be removed).

2. The low testing rate of RI patients for pandemic influenza (35% overall) is concerning, despite the authors reporting that there was no statistically significant differences in age or gender distribution between those tested and those not tested. Given that much of the paper is devoted to presentation and discussion of results based on this low testing rate, more detail needs to be provided. The authors have indicated the exclusion criteria for swabbing in the Methods section but even accounting for these the testing rate still seems very low. Were there other reasons for why the rate was low? For example, because it comprised mostly those that were swabbed as part of the vaccine effectiveness study? A breakdown of reasons for refusal could provide some useful perspective.

3. A likely source of bias with respect to the laboratory confirmed influenza results is that 23% of nose/throat swabs were taken more than 7 days after symptoms onset, and further highlighted by the statistically significant difference between those that were pH1N1 positive and those that were pH1N1 negative. The authors have appropriately identified this limitation and have justified it by referencing several papers that indicate influenza virus shedding for 6-9 days. However, a meta-analysis by Carrat et al (Am J Epidemiol 2008) found that average shedding of influenza virus lasted less than five days. I would suggest that the authors either reanalyse the data or undertake a sensitivity analysis that examines the effect when only patients for whom the swab was taken within 7 (or 5) days of symptom onset.

4. PCR is used as the 'gold standard' for influenza infection, but it would be helpful for the authors to quote the specificity and sensitivity of the test as reported by the manufacturer, and any potential impact this may have on the results.

5. The authors have not really articulated the significance of the findings of the study, and I think the discussion needs some reworking to highlight them. Many of the findings are not novel insights, such as: seasonality associated with RI, RI caused by a spectrum of pathogens, longer hospital stay and ICU admission associated with SARI. Given the lack of statistically significant associations for pH1N1, I wondered whether the authors might have commented about this
perhaps reflecting the wide spectrum of clinical presentations observed with pH1N1 infection from other surveillance studies.

6. I was also confused by the discussion regarding sex as a risk factor for ICU. Two papers were referenced to support the study finding of an increased proportion of males requiring ICU admission but is contrasted with another study that found females at higher risk for pH1N1 (perhaps not surprising given the known elevated risk for pH1N1 infection among pregnant women in particular). It is then suggested that confirmation of such risk factors might be useful in predicting the risk of further deterioration among patients hospitalised with respiratory disease, but are the authors suggesting that this would be based only on sex? And applied to SARI and/or pH1N1? The argument that authors are trying to make in this paragraph needs to be further honed and clarified. There are also numerous other risk factors such as obesity and concurrent conditions, as well as the exclusion of those at each end of the age spectrum (that have important differences in influenza presentation compared to working age adults included in this study) that have not been included in the analysis which raises questions about the generalisability of the findings.

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests