REVIEW 2

The purpose of the paper by Liczbińska et al. was to examine the role of temperatures and precipitation on cause-specific mortality in nineteenth century Poznan. The authors' motivation was that a body of literature on the influence of temperature and precipitation in modern settings is emerging, and the authors want to examine whether temperature and precipitation also influenced mortality in past settings. The authors rightly state that the literature on climate and cause-specific mortality in historical Europe outside of infant mortality is limited (if not nearly absent), and their paper hence offers a novel contribution.

Thank you.

The authors' source material consists of parish registers for the city of Poznan between 1850 and 1900, including the date of death, place of residence and cause of death as variables. The causes of death are divided into three broad categories: airborne diseases, waterborne diseases, and other causes of death. They test the role of temperature and precipitation via a multinominal regression model, testing both for the influence of the same month and with a one-month lag. Finally, they also conduct a spatial analysis testing for the differences between neighbourhoods in Poznan. The authors conclude that the lagged monthly temperatures are a better predictor for both airborne and waterborne diseases, and that there were differences in mortality between the five quarters of Poznan during the study period.

The paper was an interesting read, but quite a few things confused me, and I have some major concerns about the authors' methodology and interpretation of results. The paper would furthermore immensely improve, if a native-English speaker would proofread it. I will address my comments and concerns below.

Thank you very much for the report. We have tried to accommodate Your remarks, recommendations, and advice as closely as we could.

Major comments:

1. The authors have divided the causes of death into three very broad categories called "airborne", "waterborne" and "other". I find this broad distinction problematic. From an epidemiological perspective there are monumental differences between respiratory epidemic diseases like measles and smallpox, endemic respiratory diseases like influenza, and complications like pneumonia. Moreover, some of the diseases in the "waterborne"-category are not exclusively waterborne. Typhoid fever can infect from person to person via close contact or via milk, tuberculosis (in the airborne-category) can also transmit via milk, dysentery is known to transmit via bad food products, and recent historical evidence from the 1853 cholera epidemic in Copenhagen suggests that cholera might also have infected from person to person in this setting (see: 1093/infdis/jix602 and https://doi.org/10.1371/journal.pntd.0006103).
   Moreover, the authors do not make explicit what the "others"-category contains. The paper would improve greatly, if the authors made explicit why they used these rough divisions, discuss the validity of using so broad categories, and considered the role of multiple routes of transmission.
2. We agree that the category "the other" should be specified ("other causes of deaths", with examples provided). This problem has already been emphasized by the first Reviewer.
   If infectious diseases were categorized in our work more precisely (e.g., respiratory epidemic diseases, endemic respiratory diseases; or measles, smallpox, etc.), our

goal would be **Out of the scope of statistical modeling** (the time series are not so long and there is not enough information to describe climatic effects for different diseases separately).
What we do in this work was to estimate the average effect for broader disease categories (in our work: waterborne diseases, airborne diseases) for which we can estimate definite temperature/ precipitation influence.

3. In the methods-section, the authors state that they “explored various models” and found that the one-month lagged models yielded the best predictions in accordance with the AIC (l. 173-179). The way I read this, the authors’ choice of one-month lags appears to be based on better fits to the data, and not based on biological or social explanations. The authors do not fully explain why the one-month lag is important, or why the non-lag is unimportant. I would strongly advise the authors to 1) include the results of the statistical analyses without lags too, and 2) expand on why the results of the non-lagged analyses are not important.
There must be some lag between the (high/low) temperature occurring in a given place and time and development of infectious agents in this place and time. Then there is a further delay between acquiring an infectious (or other) disease and developing of a clear clinical symptom allowing for diagnosis, then another delay between a clear clinical sign and death etc. We do not attempt to estimate the exact total delay (it is not possible with the time resolution of the data we have; it would not even be practical since we model broader disease/ death categories). Our goal is not to describe the exact disease dynamics (which would be better studied on current clinical data), but to test and estimate the magnitude and direction of climatic variables upon mortality due to waterborne and airborne diseases (AND to differentiate among the climatic effects upon these two categories). To this end, we do not want to dilute the temperature effect by not allowing for the time delay. Since the monthly temperature and one-month-lagged temperature (and similarly - precipitation) are correlated, we would find some (but weaker/diluted) effects even without realistic lagging.
So, the formalized model selection is necessary. We used AIC, as acknowledged in the paper. The winner is ABSOLUTELY clear (AIC=7899 for the one-month-lagged model we present and AIC=8479 for non-lagged model). Thank you for the suggestion, we will add the AIC comparison and discussion of the lag issue into the main body of the manuscript.

4. The authors are right that overcrowding likely played a key role in the transmission of airborne diseases (l. 302-310). However, couldn't one also argue that crowding and general poverty played a key role for the so-called "waterborne" diseases? Many diarrhoeal diseases are known as "oral-fecal" diseases within epidemiology due to the importance of poor hygiene.
In the case of the poor epidemiological situation that took place in Poznań at that time, overcrowding (limited access to the already limited infrastructure, e.g., accesses to clean water, medical service, etc.) and poverty (worse standard of living, nutrition, lack of access to medical care, etc.) may additionally have contributed to the increase of mortality. The Market Square was the only place in the city where clean water through wooden pipes from the small Bogdanka River was supplied to public wells located there.
As we have emphasized in the text, the rest inhabitants drew water from shallow wells which were often polluted with harmful sewage discharged directly to the

rivers and the city moats. There were also primitive street gutters with ineffective drainage full of still, contaminated water. In many cases the Warta River was a source of drinking water.

5. The paper starts with a climate-related angle, arguing that we need to examine whether temperatures and precipitation also influenced mortality patterns in past populations (l. 61-63). After that, the paper focuses exclusively on the historical demography and the context of nineteenth century Poland. As a historian, I do not find this problematic, but I would encourage the authors to either change the focus of the paper to be more climate-oriented, or to return to the modern relevance in the discussion and/or conclusion.
   Thank you for this suggestion. It has also been emphasized by the 1st Reviewer. We will propose a new title (**Climate and Disease in urban space (the evidence from 19th c. Poznań, Poland)** and the Introductory chapter will be re-edited.

6. l. 280: “At the end of the 19th century there were almost 8,000 inhabitants per 1 km2 enclosed within the walls”. Was the population density so high in all four quarters behind the city walls? If so, why are there so distinct differences between them? The authors mention that the poorest population lived in quarter 5, which was outside the city, whereas the city was populated by artisans. I am not an economic historian, but can it really be the case that the poorest exclusively lived outside the city walls? It seems a bit simplified to me.
   We cannot provide the exact population density in subsequent quarters of the city, because we do not have accurate data on their exact area. Historians have provided the area within the city walls which accounted of 943.4 ha. In (appx. 9.5 km2) and did not change till 1900. In the light of Prussian Statistics, in the 1860s, 1870s and 1890s the city was inhabited by over 53, 60 and 73 thousand people, respectively. It gives over 5,500, 6,300 and 7,700 people per km2, respectively. In 1900 the fortress was demolished, and suburban quarters were incorporated into the city. The urban area expanded to 3,300 ha and the population density decreased to 3,500 per km2.
   We have data on the number of inhabitants in each of 1-5 quarters, collected by the police heads during the cholera epidemic in 1866. Quarters 1, 2, 3 and 4 were inhabited by 8513, 8631, 11095 and 9194 people, respectively, plus troops (constituted of 12-15% of the total city's population). Meanwhile, Quarter 5 was inhabited by 7,706 people. The area outside the fortress did not have limited space, so theoretically it should not have been as crowded as the one within the fortress. In practice, people lived in one-room dwellings, often cramped, damp and unheated, one room being sometimes shared by 5 to 12 people. In 1866 Cholera Specific Mortality Rates in quarters 1, 2, 3, 4 and 5 were 20, 22, 28, 19 and 55 per 1,000 people, respectively. So, despite theoretically smaller population size in Q5, cholera death rates were the highest there. In quarter 5 people were deprived of medical care. The clinical symptoms of cholera were known to doctors working in 1866 since being described by Doctor Kaczkowski in 1830. Poznań inhabitants had already experienced cholera epidemics in 1831, 1837, 1848, 1852 and 1855, so that in 1866 it was not an unknown phenomenon for doctors and inhabitants of Q1-Q4 compared to migrants from Greater Poland villages, living in Q5. For the latter cholera could have been a new experience the did not know how to cope with.
   As for the social status of the inhabitants living in the city quarters, the professional status in the fortress and outside it was reconstructed earlier based on data from

parish books. Craftsmen and white-collar workers predominated in the fortress, while unskilled workers outside it. Unskilled workers earned the least. The workers earned an average of 500 to 600 marks a year, most of which they spent on rent: a man earned 1.6 marks a day, a woman 1.0 million marks, boys under the age of sixteen 0.75 marks, and girls - 0.50 marks (Łuczak 1965). While the salaries of skilled workers were sufficient to cover the cost of a very modest living, the earnings of unskilled workers did not provide them with a minimum subsistence. Even at the beginning of the 20th century, the rent for one room was 40 to 56% of the salary of the lowest earners. After paying it, a worker earning 300 mk a year had less than 200 mk left, so less than 1 mk per day was enough to support the whole family (Łuczak 1965). The intelligentsia (doctors, teachers, officials) lived within the walls of the fortress. The literature shows that the financial situation of this group was quite diverse: doctors and lawyers earned the best. There was a great polarization among officials: in 1847, the chief president of the office received 6,000 a year. thalers, while others - the so-called senior officials – from 800 to 1500 thalers. Rank-and-file officials, especially part-time employees, fared much worse (Makowski 1992). On the other hand, the group of good earners (about 1,000 tal./year) included middle school principals, teachers, and newspaper editors. The salaries of secondary school teachers ranged from 400 to 800 thalers per year.

7. My last major comment relates to the authors' explanations of the variability of the "waterborne" mortality between the quarters. The authors state that "Inhabitants of quarters: 1, 2 and 5 had access to the Warta River, being a potential source of drinking water for many of them. In those sectors the highest mortality due to waterborne diseases was observed" (l. 189-190). Figure 10 however shows that the probability of death was highest in quarters 1, 3 and 5 with a low probability of death in quarter 2. All three quarters had access to the river water, but with very different rates in mortality.
   The surplus of deaths from waterborne diseases in quarter 1 (privileged in terms of the access to clean water) and quarter 3 (with no direct access to the rivers) was related to the presence of two hospitals in those quarters. Cholera patients were treated there and some of them died there, increasing the death statistics. We have mentioned this fact in the paper (lines 291-294).

Minor comments:

1. The authors lead the paper by stating that "Humans inhabiting a given climate zone for generations have developed particular characteristic traits that make them better suited to the environment. Due to genetic changes, individuals show adaptations in the structure and/or functioning of their organisms allowing them to live successfully in various environments" (l. 25-27). The authors are correct that some populations have adapted to specific diseases; people of African descent have better resistance to yellow fever and malaria, and Inuit populations of the Arctic regions are very vulnerable to influenza. However, genetics do not appear to be an issue in the paper, and it is not addressed further. If the authors believe that genetics was relevant for the paper, they should explicitly state so and include this in their discussion. Otherwise, it appears a bit redundant and could be omitted from the introduction.
   We agree with the Reviewer's comment. This part is redundant and will be removed from the Introduction.

2. The authors are correct that the body of literature on the relationship between diseases, temperatures, precipitation and diseases in Europe is small (l. 79-80), it is not entirely absent. In recent years, a body of literature on the relationship between temperature, precipitation and malaria in Scandinavia has emerged. See: https://doi.org/10.1186/s12879-022-07422-2, doi: 1186/s12936-021-03744-9 and doi: 10.1186/1475-2875-8-94.
   Thank you for the hint. We will implement this paper into our work!

3. On the topic of infant mortality and temperatures (l. 64-65), the authors might want to reference the work by Johan Junkka in Sweden (doi: 1097/EE9.0000000000000176 and doi: 10.1016/j.envres.2020.110400).
   Thank you. The suggestion will be implemented.

4. On lines 77-78, the authors state that warm temperatures facilitate diarrhoeal diseases. This is true, and it is also worth noting that warm temperatures facilitated malaria (see above-cited malaria-papers) and plague (doi: 10.1098/rspb.2020.2725) in Europe historically.
   We agree with the Reviewer's comment. However, the indicated diseases did not occur in Poznań at all. This suggestion will be included in the introduction of the manuscript.

5. l. 94: change “till” to “until”.
   Manuscript will be re-edited.

6. l. 100-101: “With time, the city started to suffer from the lack of free space and at the end of the 19th century it was virtually suffocating within the surrounding walls.” Use a different phrasing than “virtually suffocating”.
   Thank you for this suggestion. Manuscript will be re-edited.

7. l. 115-116: the authors need to revisit these two lines. First, they state that the urban ecology was “really bad”. I would appreciate a different phrasing. Secondly, they state that “This translated into the health status of Poznań inhabitants, who often suffered from outbreaks of epidemics, i.e., cholera (Piankowski, 1988; Liczbińska, 2021)”. The terms “epidemics” and “outbreaks” are synonymous for the same thing. It is also unclear to me, what the authors mean by “i.e., cholera”. “i.e.,” is an abbreviation for “id est”, in English “that is”.
   Yes, we agree with this hint. In stead of “This translated into the health status of Poznań inhabitants, who often suffered from outbreaks of epidemics, i.e., cholera (Piankowski, 1988; Liczbińska, 2021)” it should be “This translated into the health status of Poznań inhabitants, who often suffered from outbreaks of epidemics, e.x., cholera (Piankowski, 1988; Liczbińska, 2021).”

8. I don’t presume that the authors meant that cholera was the only epidemic disease in Poznan, as they later mention scarlet fever, typhoid fever, and measles as other epidemic diseases.
   Cholera epidemics were not the only epidemics in Poznań but compared to the epidemics of smallpox or scarlet fever, for instance, they took the greatest mortality toll. They were the largest epidemics in the 19th-century Poznań causing massive and rapid changes in population numbers within a short period of time.

9. Finally, on line 116, the authors use the term “contagious diseases” rather than “infectious diseases”, and “mortality toll” instead of “death toll”.
   Thank you for this suggestion. The suggestion will be implemented.

10. On lines 119-120, the authors state that “Infant mortality in Poznań was estimated as >250 deaths per 1,000 live births, and during the intervals of the epidemics, infant mortality was >300 per 1,000 live births”. As I am sure the authors are aware, there was a massive drop in infant mortality rates during the second half of the nineteenth century. I would appreciate it, if the authors be precise and clarify which period they are referring to.
    For example:
    1850-1874 =315.03 per 1,000 live births*
    1875-1884=280.4 per 1,000 live births**
    1885-1894=261.8 per 1,000 live births**
    1895-1904=199.9 per 1,000 live births**
    1905-1913=171.05 per 1,000 live births**
    *Calculated based of data derived from birth and death books for Poznań parishes
    **Calculated based of data derived from Prussian Statistical Yearbooks

11. Lines 160-162: this sentence seems a bit redundant to me. The authors have already described the nineteenth century history of Poznan and can easily state that the city was divided into four quarters by the Prussian authorities. Also, it is unclear why the authors use the German name “Posen” in this sentence.
    Posen was the official name of the city introduced by German authorities. This name was in force until 1918 (Poland gained independence from the partitions). Therefore, next to the name Poznań, Posen is often given in the brackets (German: Posen).

12. On line 165 the authors state that the five quarters of Poznan differed in ecological conditions. Please expand on this: how were they different?
    Overall, the situation in Q1-5 was not black and white. In general, quarters 1-5 were inhabited mainly by craftsmen and white-collar workers, while quarter 5 – unskilled laborers. The ecological conditions in Poznań differed among the city quarters, which influenced the number of deaths from infectious diseases. The analyses of the epidemic in 1831 showed that on the right bank of Warta River (Q5) deaths from cholera accounted for 32.7% of all deaths in the city in 1831. In the light of the same data, deaths from cholera in the suburbs belonging to the parish of St. Martin (quarter 3) accounted for 25.2%, the parish of St. Mary Magdalene, encompassing the streets around the market square (quarter 1), accounted for 19.2% of all deaths. In 1866 Cholera Specific Mortality Rates in quarters 1, 2, 3, 4 and 5 were 20, 22, 28, 19 and 55 per 1,000 people, respectively. The poorest conditions were in the quarter 5, where people used shallow wells, often tainted with harmful sewage from cesspits, gutters, and rubbish sites. There were also primitive street gutters with ineffective drainage, which were full of stagnant contaminated water during summer heat periods. They did their laundry in the nearby rivers which was a source of drinking water. Inhabitants of quarters 2-4, although represented a wealthier part of Poznań society, had no access to clean water. Unfortunately, they used shallow wells located in courtyards. The further away from the market square, the worse was

the access to clean water intake. The quarter 1 had wells providing a supply of clean spring water but was not free of danger: the area around the Market Square was a place of a large rotation of people, and this was where travellers usually stayed. For example, cholera was probably brought to one of the taverns in the Old Market Square in June 1866 by rafters from Szczecin.
In the quarters 1 and 3 there were located hospitals. Their presence was helpful for the sick but on the other hand it could additionally have increased the number of deaths from cholera, or in general – the number of deaths from other causes in Q1 and Q3.

13. l. 229-231: “The incubation period of *Vibrio cholerae* is consistent with the duration of 1–5 days (Azman et al., 2013; Eisenberg et al., 2013). For diarrhoea-causing pathogens, mainly rotavirus and pathogenic *Escherichia coli*, the incubation period is usually around 1week (Eisenberg et al., 2003)…” I would suggest that the authors write “For **other** diarrhoea-causing pathogens”, since cholera also produces diarrhoeal symptoms.
Thank you for this suggestion. The suggestion will be implemented.

14. l. 269-270: “This model suggests that there are substantial differences in the incidence of deaths due to waterborne and airborne diseases in different quarters.” Incidence is an epidemiological term for the number of notified cases (not deaths) per capita. I would suggest that the authors go with “mortality rates” instead.
Thank you for this suggestion. The suggestion will be implemented.

15. l. 302: “The biological standard of living could…” You can just say “the standard of living”.
This will be re-edited.