Author's response to reviews

**Title:** Is there a relationship between weather conditions and aortic dissection?

**Authors:**

Costa Repanos (costa@repanos.com)
Neil K Chadha (neil.chadha@entsouthwest.co.uk)

**Version:** 2  **Date:** 11 February 2005

**Author's response to reviews:** see over
Response to reviewers’ comments

February 11, 2005

Dear Editor,

Re: Do changes in atmospheric pressure and temperature relate to the incidence of aortic dissection? Repanos C, Chadha NK

Thank you for providing us with the peer-review comments. We have carefully addressed all of the points that have been raised, and provided a detailed explanation to the appropriate reviewers below. We hope the revised version of the paper may now be considered suitable for publication.

Review 1 (Mehmet Kurtoglu)

Thank you very much for your comments on our submission. We are also grateful to you for bringing to our attention your previous work on the subject, and we have now cited and made reference to this article in our manuscript.

Review 2 (Matthew Bown)

Thank you very much for your comments on our submission. We have addressed all of the points you have raised, and a detailed explanation is below:

1) The data concerning how many patients were admitted to this tertiary referral centre with aortic dissections, including those referred to medical teams and managed conservatively is not available to us. However it was not the intention of this paper to accurately calculate the incidence, but to correlate those cases that we definitely know were aortic dissections to the weather conditions. We have however changed the title appropriately to “Is there a relationship between weather conditions and aortic dissection?”

2) The reason that we have used mean monthly data centred on the time of the operation is that the pattern of the weather will obviously change through the progression of the seasons. We wanted to find out whether the weather conditions for that season had deviated from the norm. By just looking at the 30 days preceding the event, a weakness may have been that this represented the natural seasonal progression of the weather.

3) We have taken advice from the Meteorological Office and a local statistician in designing this study and from further reading we have found no evidence that this definition is in wide use. The term "climate variability" is often used to denote
deviations of climate statistics over a given period of time (such as a specific month, season or year) from the long-term climate statistics relating to the corresponding calendar period. In this sense, climate variability is measured by those deviations, which are usually termed anomalies.

4) We agree with the reviewer’s assertion as to the reasons for using the 24 and 48 hour periods for comparison – we have now made this reasoning more clear in the manuscript.

5) We agree that Table 1 can be inferred easily from Tables 2 and 3 so is redundant, and it has been removed. We have now added the numbers of observations to Tables 2 and 3 (now tables 1 and 2) as suggested. We have also removed the redundant Wilcoxon column from Table 2 as suggested.

6) We have deliberately not checked for statistical variation in monthly incidence in Figure 1, as it was not our objective to look for the seasonal variation in this condition. The small numbers in each month also preclude any meaningful conclusions. We feel it would be misleading performing post-hoc analysis that was not intended at the outset.

7) We have amended the abstract and discussion so as not to imply significance of analysis where statistically significant differences did not exist.

8) We have now tabulated the numbered list in the methods as suggested.

9) We have amended the quoted P-values to consistently 3 decimal places.

10) We have removed the markers for each ½ of a dissection on Fig 1 as we agree these are meaningless.

Thank you for your further consideration of our manuscript.

Yours sincerely,

Mr C Repanos

Mr NK Chadha