Reviewer's report

Title: Cyclical changes in seroprevalence of leptospirosis in California sea lions: endemic and epidemic disease in one host species?

Version: 2 Date: 14 May 2007

Reviewer: Andrew Roddam

Reviewer's report:

General

This is an interesting and well prepared manuscript reporting the results of a study investigating the prevalence of leptospirosis in Californian sea lions. I comment here on the statistical aspects of the manuscript for which I have only a small number of comments.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. My main comment concerns the use of the logistic regression model – the dataset has as its outcome a three factor variable (uninfected, low titre, high titre). Therefore the most appropriate model to fit would be a multinomial logistic regression model rather than 2 individual models. This would then allow the authors to present tests of whether or not the effect of age, sex etc are different between high and low titres.
2. My other main comment concerns the analysis presented in Fig 2(b) and how stable the analyses are. One of the assumptions in the regression line is that the residual errors are normally distributed. However there does appear, as the authors note, to be an outlier for the year 2000 and possibly 2004. I wonder how much influence these points have on the estimate of the intercept and whether there are any suggestions of departure from normality. Also as the authors propose a reasonable explanation for the year 2000 being anomalous there could be a strong case for omitting this data point – what impact would this have on the results?

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. It would be helpful to the reader if, in Table 3, the numbers of infected/disease free sea lions are included in each of the groups.
2. In the labels for Fig 1 it would be helpful to describe the differences between panels a b and c.
3. Could the authors check Fig 3 as I think they have panel b and c the wrong way round.
4. There may be a good reason for this (Im not an expert in sea lions!) but why use different age classifications in Table 1 for males and females – and more importantly to me, how does this translate into the analysis.
5. I think that 2 paragraph in the analysis section would benefit from a little more explanatory text to explain some of the terms/equations in English as I found that I had to read it a few times as well as look at the results to understand what the authors were doing.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

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