A review of factors affecting pilot-reported bird-strike rates at Christchurch International Airport

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Introduction

The aim of the paper, "Factors affecting pilot-reported bird-strike rates at Christchurch International Airport, New Zealand" (Chilvers *at al.* 1997), was to report on analysis of pilot-reported bird incident data. The study was undertaken to investigate factors influencing reported bird-strike rates, including the effect of intensified management begun in 1987 (Introduction 4th para.).

In this report I review the scientific merit of the paper, in particular, the validity of the use of the statistical methods. I have a number of specific criticisms of the paper and these are outlined below.

1. Change in proportion of incidents reported since instigation in 1987 of the bird-management programme

The main criticism of the paper is the comparison of the pilot-reported bird strike rate before, and after, the instigation of the bird management programme in 1987. Data is not available on the actual number of incidents and therefore reported incidents are used. One study on bird strike rates at J.F.Kennedy International Airport, Long Island, New York estimated pilots report about 20% of actual strikes (Burger 1995). In this study of Christchurch International Airport (CIA) the proportion of reported incidents before and after the instigation of the management programme is likely to be confounded by a change in the proportion of actual incidents that are reported.

Part of the bird management programme was pilot awareness programmes (Discussion, Effectiveness of management practices, 1st para.). Therefore it would be reasonable to assume that after the instigation of the management programme the proportion of actual incidents that were reported was higher than in the proceeding

period. Use of the reported incident rates to infer about the true rate of actual incidents before and after 1987 is difficult. Yet, in the paper there is no specific or direct acknowledgement of the likelihood that the proportion of incidents reported may have changed since 1987. The acknowledgements by the authors that the proportion of incidents reported may have changed since 1987 and the instigation of the management programme are:

> Methods, 2nd para. "A potential bias in such bird-incident data is that pilots may be more aware of the problem, and CIA's reporting requirements, in the period immediately after publicised incidents, so that their reporting rate increases temporarily."
> The authors are discussing the potential short-term effect following an incident rather than the effect of the management programme.
> Results, 1st para. "Nevertheless, we acknowledge that changes in incident reporting rate could be a factor in the results presented below."
> This sentence immediately follows a sentence about changes in the proportion of incidents reported after bird-strike and is assumed to be referring to short term effects, rather than changes since the management programme was introduced.
> Discussion, 1st para. "CIA monitoring therefore assumes that the reporting rate remains relatively constant (H. McCarroll pers. comm.)."
> Given that part of the bird management programme was pilot awareness programmes I would have interpreted this personal communication to be in reference to the assumption that the proportion of incidents reported prior to 1987 were relatively constant and the proportion after 1987 were relatively constant. The two proportions are not necessarily equal.

Further, the authors have been rather lapse in their use of the words: "bird-strikes" and "bird-incidents" rather than "reported bird-strikes" and "reported bird-incidents".

2. Changes in proportion of incidents reported with changes in incident rate

There is no mention in the paper about the validity of the implicit assumption that the rate of increase in reported incidents will be the same as the rate of increase in the number of actual incidents. For example, if the number of incidents doubles the assumption is that the number of reported incidents also doubles. This may not be so. An alternative trend may be that when there are many incidents pilots will be more aware of bird-strike problems and more likely to report an incident than when incidents are a rare event. If the rate of increase in reported incidents was not the same as the rate of increase in actual incidents direct interpretation of changes in pilot-reported data is difficult.

3. Use of data and potential psuedoreplication

The analysis of the reported incident rate could be misleading and it would have been preferable to use the rate of "no reports of incidents". If the chance of an incident occurring is related to the number of birds near the airport, when one incident occurs, it is more likely that another incident would occur for the following aircraft flights. If this were the situation the use of the number of reported incidents, adjusted or not for

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aircraft movement, as independent data points is incorrect. This is known as psuedoreplication. Analysing individual reported incidents may overestimate incident rates and any observed effect may be exaggerated.

A more appropriate analysis would have been to use the rate of no reports of incidents within a suitable time interval, e.g., the number of days with no reported incidents.

4. Data analysis methods

In general there is too little detail to understand the method used to analyse the data. Readers are left to make their own assumptions about the methods. As a result some parts of the results may be erroneously misinterpreted. Specific examples are discussed below, e.g., point 2.

5. Other points

There are a number of other minor criticisms and they are raised in order as they appear in the paper:

1) Introduction, 2nd para. "...Christchurch International Airport (CIA) has the highest recorded strike rate nationally (Department of Civil Aviation 1991)." It is unclear from this statement in which time-period the CIA had the highest recorded strike rate, e.g., is this based on data collected over 12 years (1981 – 193), or in the period since the instigation of the bird management program (1987 – 1993)? Are the authors referring to actual strike rates, or reported strike rates? It is unclear whether the strike rate is the absolute number of reported strikes, or the number of reported strikes adjusted for differences in flight traffic. The reader could refer to the referenced article but it would be useful if the authors had provided the supporting information, including the number and size of airports that have been used in the comparative analysis by the Department of Civil Aviation.

2) Methods 3rd para. "Changes in the strike rate were tested using polynomial regression, weighted by the numbers of schedule flights each hour." The use of the term "weighting" is misleading and implies weighted regression, a technique commonly used to deal with heterogeneous variances. There is no discussion of variances and the authors may have used standard polynomial regression on the number of reported strikes divided by the number of scheduled flights. However, my interpretation may not be correct because in the previous sentence they refer to the relative strike rates and that these relative rates are corrected for number of the number of aircraft movements. It is not clear what the relative strike rate is. This paragraph needs more detail for the reader to understand the analysis of diurnal patterns.

3) Methods, 3rd para. "Seasonal patterns were assessed by calculating the mean number of strikes each month: between-month differences in the number of flights were assumed to be minimal." This assumption may be valid, but it should have been tested. It would be a fairly easy task to check for between-month differences in the number of flights. If there were these differences then the number of reported strikes per month should have been divided by the number of scheduled flights. This would also remove any differences among years in the number of reported strikes.

Such an analysis may have improved the validity of the ANOVA. One of the assumptions of ANOVA is that the variances are equal. In this example, the variances of each month's data should be (roughly) equal, but in Fig 3., it appears that there are differences in the standard error, and hence variances of the monthly data.

4) Methods, 3rd para. "The effect of aircraft type and bird species on strike:miss ratios was tested using χ^2 tests of association...". There is insufficient detail for the reader to understand what the strike:miss ratio is, in particular, what data is being used.

5) Results, 1st para. "The number of near misses reported daily was not significantly influenced by how recently a bird strike had been reported (Fig 1),...". As mentioned above, this analysis is confounded by differences in the expected reporting rate prior to, and following, the instigation of the bird management programme. The pre-1987 and post-1987 data should have been analysed separately.

There is no mention of the total number of reported near miss incidents following the day since the last strike. It would have been helpful to include the sample size (*n*) for each bar in Fig 1. Given the data in Table 1, if there were 202 reported near-miss incidents over 4 years (August 1989 to August 1993) the expected number of reported near misses is 50.5 per year, or about one a week. Without seeing the relevant sample sizes, a reader would be suspicious of the reliability of interpreting the "robustness" of pilot reporting rate beyond about 2 weeks because there would be too little data.

The final three bars in Fig 1. are from data pooled over 6 day intervals while the first seven bars each represent 1 day. Since temporal change is what is being investigated it is somewhat odd to change the size of the interval used to record steps in time.

6) Results, 2nd para. "Over the past 13 years...362 bird strikes have been reported to CIA." However, Table 1 only has data for the 92 reported strikes between August 1989 and August 1993. It would have been helpful to include data from all 13 years, or, at least explain why only these 4 years were displayed.

7) Results 3rd para. "Most strikes (91%) occurred between 0700 and 2200 h..". This sentence should include what time period for the data was used in this analysis – was it the 1981 – 1993 or the 1989 – 1993 dataset?

Some explanation of the reported strikes outside this hours would be useful (9% is approximately 32 of the 362 strikes in the 13 year period, or 8 of the 92 strikes in the 4 year period). The authors have chosen 0700 and 2200 h and I would expect that there was either very few, or no aircraft movement outside these hours. These 9% may represent data that had no time period recorded, and if so this should have been noted. As it reads, the reader is left to wonder about the remaining 9%.

8) Results, Fig 2., This graph needs more explanation. The vertical axis is labelled "Bird strikes per scheduled flight" and the values of the curve ranges from 0.2 to 0.75. These bird-strike rates seem very high. The total number of reported strikes for the 4 year period (August 1989 to August 1993) was 92. Therefore, for the period 1987 to 1993 138 reported strikes would be expected in total. The average bird strike per scheduled flight from Fig 2 is about 0.3 which suggest there were only 460 flights in the 13 year period. This is clearly incorrect. The rate of (reported) bird strikes per scheduled flight is far too high and either there has been a mistake in the analysis, or the graph is labelled incorrectly.

9) Results 5th para. "There was significant monthly variation in the number of strikes..". As mentioned in point 3 it would have been more appropriate to have adjusted the number of reported strikes in a month by the number of scheduled flights for that month.

10) Results, 6th para. "This suggest that, on a per-flight basis, 737's are less likely to be involved in strikes...". It would have been useful to have investigated whether there were diurnal patterns in the types of scheduled aircraft flights. If there were diurnal differences the analysis should have been adjusted to take this into account. For example, it may be reasonable to expect that the larger planes are used in the morning and later afternoon and smaller planes at midday.

11) Results, 7th para., ".. the number of bird strikes was increasing annually by an average of 5.4±1.2 SE strikes per 100 000 aircraft movements." As mentioned above it may have been more appropriate to look at the number of time units where there were no reported incidents to avoid any problems with pseudoreplication.

12) Results, 7th para., "However, the variation in quarterly bird-strike rates has increased significantly.....", <since the dump closed and the bird management practices were implemented>. The variation appears (Fig 4) to have increased since 1987. I have a number of concerns with this graph, and interpretation.

a) There is not enough detail in the methods to understand how the difference in variation before and after 1987 was statistically tested but I infer a variance ratio test was used. Such a test may have been incorrectly used if the serial correlation in the data were not accounted for, and secondly, if the assumption of normality were not valid.

One way to deal with serial correlation would have been to redraw the graph with yearly intervals rather than quarterly intervals and to have compared the coefficient of variation (*cv*) for the pre-1987 period with the post-1987 period. With yearly intervals the *cv* would be approximately equal for the pre and post 1987 period with no evidence of an increase in variation in bird-strike rates.

b) The data displayed in Fig 4 are somewhat odd and I am sceptical of them. The results for 1987 to 1993 displayed in Fig 3 for mean monthly reported bird strike rates (and SE) have little variation among months. The only trend is that April has a higher than expected mean number of reported bird strikes. However, in Fig. 4 there appears to be a lot of variation among quarterly

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reported bird strike rates for the data in the post 1987 period. This variation is not due to the high April rate because the peak quarter differs among years.

Unless there is considerable differences in aircraft traffic among quarters it is difficult to understand this apparent anomaly. One explanation is that there is considerable monthly variation within each year, and this monthly variation differs among years. When the data is pooled across years the monthly variation is not evident. If this were the case then displaying pooled monthly data in Fig. 3 is incorrect because the interaction between year and month is not accounted for. The data should have been more carefully analysed.

c) The authors, in the last sentence of the paragraph say, "This variation was not due to any statistically significant variation between seasons, so its cause remains obscure." One obvious possible explanation that could have been addressed in the discussion is that the fluctuations represents variation in the proportion of actual incidents that pilot reports since the management plan were introduced. Part of the management plan is to increase reporting, and it is possible that after periods of low numbers of reported strikes, pilots are reminded (officially, or indirectly) to report strikes. Their response to low reported rates is to increase the rate of reporting. Over time the pilots report fewer of their bird incidents, until few reports are delivered. Pilots are then reminded again and so on.

13) Discussion, Effectiveness of Management practices, 1st para., "Surprisingly management practices appear to have destabilised the strike rate, not decreased it....". The authors do not present statistically sound evidence to support this statement in relation to destabilising the reported strike rate (point 12ab).

The reference to the reported strike rate not being decreased is also unsubstantiated. The expected post 1987 reported strike rate from Fig 4. is 0.43 per 1000 aircraft movements. The expected reported strike rate for the same period if the pre 1987 trend were to continue in the same linear rate, is about 0.55 per 1000. This simple analysis suggests that since the management programme was instigated the increase in reported incidents has been stopped.

Summary

The paper reviews factors that affect the rate of pilot-reported bird strikes, but the details of the analysis methods that were used are sparse. The study used data that was the reported bird-strike rates but in places, the wording of the paper suggests the data were the actual bird-strike rates. There is likely to be differences between the actual and reported bird-strike rates. Without further study it is a spurious assumption that the difference between the actual and reported bird-strike rates is constant over time, or constant with higher, or lower strike rates.

Further, it is highly spurious to assume the difference between the actual and reported bird-strike rates did not vary before and after the instigation in 1987 of the management programme. Given that part of the bird management programme was pilot awareness programmes direct comparison of pre- and post-1987 reported bird-strike rates are likely to be confounded by differences in the proportion of incidences

that are reported. If in the post-1987 period the proportion of actual incidents that are reported is higher than in the proceeding period, the actual incident rate after 1987 will be lower than it appears to be.

The data used in study is the rate of reported incidences. If the chance of an aircraft being involved in an incident is related to whether the proceeding aircraft were involved in incidents any reported effects may be exaggerated. It would have been more appropriate to use data on the rate of "no reported incidences" rather than "reported incidences."

The claim that the variation in quarterly bird-strike rates has increased significantly (Results, 7th para.) is unsubstantiated, and I suspect, erroneous. The data displayed in Fig 3. for the 1987 to 1993 period suggests there is little variation among the average monthly reported bird strike rates other than the peak in April. The data displayed in Fig 4. suggests there is a lot of variation in quarterly reported bird strike rates for the same period. If there were no variation in monthly rates there should be no variation in quarterly rates unless there were large monthly differences among years. There is no explanation given for the anomaly between the two graphs. I find no reliable evidence in the paper to support the claim, "Surprisingly management practices appear to have destabilised the strike rate, not decreased it....", (Discussion, Effectiveness of Management practices, 1st para.).

References

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