Letters

Still shooting in the dark

PJ Baker* and S Harris

School of Biological Sciences, University of Bristol, Woodland Road, Bristol BS8 1UG, UK

* Contact for correspondence and requests for reprints: P.J.Baker@bristol.ac.uk

In response to our criticisms (Baker & Harris 2005) of their study 'Wounding rates in shooting foxes (*Vulpes vulpes*)' (Fox *et al* 2005a), Fox *et al* (2005b) state that their aim was "not to test multivariate relationships", but to "assess the probabilities of outcomes for a number of regimes based on limited randomised shots fired by large groups of shooters"; although we note that only small to moderate numbers of shooters were actually used in the majority of regimes assessed (71% of the 51 regimes utilised less than 15 shooters, and some used less than 10 shooters). This statement of their aims is evidently not true. The results outlined in Table 2 (Fox *et al* 2005a) do outline the probabilities referred to by Fox *et al* (2005b), but the subsequent results presented in Tables 3–9 then attempt to quantify the impact of a range of factors underlying the probability that individual shots resulted in a 'kill', 'serious wound', 'light wound' or a 'miss'. Indeed, 8 of the 11 paragraphs in the Results section relate to the impact of these factors.

Given that this *was* a principal aim of the study, and as we outlined (Baker & Harris 2005) contrary to the opinion expressed by Fox *et al* (2005b), it is clear that any such study *does* necessitate the use of an appropriate multivariate analysis to account for relationships between factors. Failure to do so is both naïve and erroneous statistical practice because it fails to account for the possibility that statistically significant differences in some comparisons will be attributable in part, or in whole, to other related factors. This is particularly relevant where, as in Fox *et al*'s (2005a) study, data have been pooled across regimes with different subset sizes (regimes used 8–31 individual shooters). Indeed, the authors appear to have acknowledged the problems inherent with the unbalanced nature of their study by substantially revising the analyses originally presented in the report that they submitted to The All Party Parliamentary Middle Way Group (Fox *et al* 2003).

As an example, the first comparison outlined in Table 3 of Fox *et al* (2005a) compares the effect of shooter skill on the proportion of shots using shotguns classified as 'kills' (we would like to make it clear that we have selected this example simply because it is the first one listed in Table 3 and not, as Fox *et al* [2005b] appear to imply, as some sort of attempt to misrepresent their data). However, there was a disparity between the three skill levels in (1) the total number and relative proportion of shots fired at, for example, different distances to the target, and (2) the number of regimes conducted at different distances, this indicating further divergence associated with the other factors varying between regimes (Table 1 above). All these differences act to confound the degree of variation attributable to shooter skill level (if any) rather than the combined effects of differences in distance to target, weapon calibre and choke, and type of ammunition.

Table 1 The number of shooters of different skill level participating in regimes using 12 bore shotguns at different distances to the target (calculated from Table 2 of Fox *et al* 2005a). Figures in parentheses indicate the percentage of shooters within each skill level, and n is the number of regimes.

Distance to target	Unskilled	Semi-killed	Skilled
25 yards	48 (38%) (n = 4)	85 (37%) (n = 4)	51 (36%) (n = 4)
40 yards	65 (52%) (n = 6)	121 (53%) (n = 6)	79 (56%) (n = 6)
60 yards	12 (10%) (n = 1)	22 (10%) (n = 1)	11 (8%) (n = 1)
Total	125 (100%)	228 (100%)	141 (100%)

Similarly, Fox *et al* (2005a) should have acknowledged the need for an appropriate correction of the alpha significance level used because of their multiple testing approach: they perform 36 separate tests, presented in Tables 3–9 (and 124 in their original report [Fox *et al* 2003]), yet consistently use a significance level of $\alpha = 0.05$. As any undergraduate science student knows, such statistical 'fishing' will increase the likelihood of Type 1 errors. In actuality, the situation is made worse by the fact that Fox *et al* (2005a) always perform four separate tests on the same basic data, on each occasion re-coding the same data point as 'yes' or 'no' for each corresponding test: (a) was the shot a 'kill' shot?; (b) did the shot result in a 'serious wound'?; (c) did the shot result in a 'light wound'?; (d) did the shot 'miss' the target? This non-independence of tests in each row in each of Tables 3–9 is likely to inflate the number of instances where multiple significant differences are detected because the data are essentially proportions.

Fox *et al* (2005b) also state that they have used the properties of the Central Limit Theorem to justify the analysis of their binary coded data using ANOVA. However, as we highlighted previously, there are problems inherent with their data that renders this approach invalid. The quotation of Gotelly and Ellison (2004), presented by Fox *et al* (2005b), actually reiterates the first fundamental property required to adopt this approach that the authors violate; namely that the data utilised must be independent. As two shots from each shooter were included from each regime, this basic assumption is not met; in fact, Fox *et al* (2005a) have artificially doubled their sample sizes by pseudoreplication. Furthermore, given that different participants were included in different numbers of regimes, but not all regimes, the data utilised in Tables 4–9 are not independent between groups. Interestingly, on this point, Fox *et al* (2005a) state that

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they limited their analyses to "two shots per shooter per regime … to avoid practice improving their performance", yet the probabilities presented in Table 2, which in their own words were the main focus of their study, utilise data from all the shots fired, with shooters apparently firing up to five shots in some regimes.

Applying the Central Limit Theorem in this manner is also reliant on the second fundamental assumption that all individual (Bernoulli) trials have a constant outcome probability. For example, in an analysis of the effect of shooter skill level on the likelihood of 'killing' the fox outright, there must be a constant likelihood between (independent) trials that each shot results in a 'kill'. This presumption of constant probability is not expected where data have been pooled across regimes that vary in the types of ammunition used, distance to target etc; comparing the probabilities listed in Table 2 (Fox *et al* 2005a) for skilled shooters firing rifles, the probability listed for kill shots ranges from 40% to 90%. Indeed Fox *et al* (2005a) themselves implicitly concede that this profound variation is present in the data they have pooled, because they subsequently analyse these data to determine the effects of these other factors! Even had this approach been valid, the authors appear to have overlooked the possible problem of non-equality of variances; this is particularly relevant where the outcome probability is high or low, as this leads to skewed distributions of points. Therefore, in summary, the authors have failed to conform to the fundamental basic assumptions of the statistical approach used to collect and analyse their data, rendering their conclusions of little or no value.

Although we agree that there may be a number of practical issues to address when implementing a thorough scientific examination of the impact of factors associated with wounding in foxes, these problems are not insurmountable. Certainly, given the political sensitivity of the issue being investigated, they are not a valid excuse for the limitations present in Fox *et al*'s (2005a) study. For example, the approach that we suggested (Baker & Harris 2005) could easily be designed to account for the potential problem of improving individual performance across regimes by (1) allocating participants to regimes in a random order, and (2) including an additional variable that would indicate the temporal sequence in which individuals completed regimes, ie trial number; both approaches are common statistical practice. Although such a study would require careful planning and the use of large numbers of participants, it would generate a balanced, rigorous, robust and statistically valid data set on people's ability to hit paper targets. Whether this has any relevance to the situation in the field is another issue.

In conclusion, and to borrow a set of phrases from Fox *et al* (2005b), the wider scientific community can see that the issues we have highlighted are not statistical fine detail but are over-riding violations of the fundamental basic properties of the analytical procedures utilised. As such, any conclusions derived from such an analysis can be seen to be seriously flawed. Furthermore, by their own admission, Fox *et al* (2005b) "[do not claim] that the majority of the shooting regimes tested 'reflect practices actually occurring in Britain'", that the regimes tested "were not intended to represent the spectrum of regimes used in real life, because nobody knows what they are", that "real life is seldom as tidy as controlled trials" and that "the real crux of the matter is the motive of the shooter, which we could not measure"; therefore, one has to question the meaningfulness of a study in which one attempts to enhance our understanding of this issue by testing a series of practices that may or may not be occurring (and hence their relative importance) in the wider countryside, using a technique that may or may not reflect the true outcome of these practices, but which does not (apparently) examine the fundamental issue. As these authors say, "the key issue is the animals that escape wounded", but we believe their study makes little contribution to further our understanding of this welfare issue, not least because they limit their study to a single shot, whereas in the field a high proportion of wounded foxes (probably a large majority) will be swiftly dispatched with a second shot.

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Response to Baker and Harris' letter 'Still shooting in the dark'

NC Fox*, N Blay, AG Greenwood, DG Wise and E Potapov

International Wildlife Consultants Ltd, PO Box 19, Carmarthen SA33 5YL, Wales, UK

* Contact for correspondence and requests for reprints: office@falcons.co.uk

We are glad that Baker and Harris (2005) accept that our Table 2 (Fox *et al* 2005a) does outline the probabilities that we claim in our study, and that presumably they also accept the other points we explained in our last letter (Fox *et al* 2005b).

Although the most accurate or most appropriate way to analyse these results, whether by the method we have used or by a multivariate approach proposed by Baker and Harris, will no doubt remain an area of contention, the results in Table 2 speak for themselves.

However, Baker and Harris's complaints of our statistical treatment are unjustified. They accuse us of pseudoreplication by artificially doubling our sample size because we allowed

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