values. These data will not be normally distributed, and will violate the second and third key assumptions of this statistical approach. Such coding also gives rise to pseudo-replication across tests and renders interpretation difficult. For example, a difference in the mean value of 'shot resulted in a kill' versus 'shot resulted in a serious wound, light wound or miss' would not relate directly to differences in welfare standards since 'misses', which are of no welfare concern (Fox *et al* 2005, p 94), are included with those categories that could be of welfare concern.

Independent analysis of different factors is also only valid where other factors are assumed, or have been shown, to exert no effect. Therefore, it is inappropriate to test, for example, the effect of shotguns versus rifles (Table 4) without controlling for the effects of skill level, given that skill level had previously been shown (erroneously) to affect outcomes (Table 3). The only appropriate way to examine these data would have been to utilise a multivariate approach, such as ordinal logistic regression, incorporating the effect of all independent variables simultaneously. Yet, given that the data are not independent at the most basic level, a rigorous analysis can only be achieved by reducing the data set to those situations where each participant is included equally in all regimes. In a similar vein, the figures presented in the columns headed "Probabilities" and "Wounding 'tax'" in Table 2 appear to have been erroneously calculated from the total number of shots fired by all participants, regardless of whether each participant fired the same number of shots.Consequently, we believe that the results as presented in this manuscript are not a valid investigation of wounding rates arising from fox shooting practices in Britain: the premise of the regimes tested does not reflect the pattern of shooting actually occurring, there is no evidence of widespread wounding of foxes in this country, and the statistical analyses are fundamentally flawed.

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Response to Baker and Harris's letter 'Shooting in the dark'

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In commenting on our paper 'Wounding rates in shooting foxes' (Fox *et al* 2005) Baker and Harris make a number of unfounded assumptions. First of all, we have *not* claimed that the majority of the shooting regimes we tested "reflect practices actually occurring in Britain". Nobody knows how many foxes are shot in Britain, by whom or with what regimes. What we *do* claim is that all of the methods we tested were legal, that most are in common use, and that further, less welfare-friendly, regimes are also used. We know this because some of the 199 shooters involved in the study volunteered this information. For example, some claimed to have shot at foxes up to 120 yards with shotguns, hoping to wound or kill; others had used .410s beyond 25 m, and foxes have even been admitted to animal hospitals with wounds from air rifles (Harris 1978). We did not test these extreme regimes, nor do we claim — as Baker and Harris imply we do — that these extreme regimes are in regular use. Our regimes were chosen to explore not only those within government guidelines, but also those commonly used. The British Association for Shooting and Conservation is now using our protocols and targets to explore additional regimes and we commend them for their pro-active approach to improving animal welfare.

Baker and Harris claim that "over half of the foxes shot in Britain are killed by gamekeepers". We don't accept this claim. It may be true in parts of south and east England, but in other areas, such as Wales, there are few gamekeepers and most foxes are shot by other people. Again, nobody actually knows. Baker and Harris then claim that the gamekeepers "might reasonably be expected to be skilled shots". This may be so, and we have therefore explored the welfare benefits of skilled versus semi-skilled versus unskilled shooters in our study. It turns out that skilled shooters, although they kill a higher percentage, do not wound much less than unskilled shooters. But the real crux of the matter is the *motive* of the shooter, which we could not measure. Those that shoot for pest control as their first priority, inevitably have welfare only as a secondary priority. This is reflected generally in Britain's approach to pest control; the break-back mouse trap would not pass ISO fur-trapping standards (Harrop 2000). Yet we campaign against fur-trapping but not against mouse-trapping, and Defra licenses the use of some rodenticides despite admitting that they are 'markedly inhumane' (Pesticides Safety Directorate 1997; Fox 2002). Unlike deer-shooters, who wish to retrieve the carcass for the meat, people shooting pest foxes have no

https://doi.org/10.1017/S0962728600029572 Published online by Cambridge University Press

interest in recovering the carcass; they simply want to put the fox out of commission, dead or alive.

Baker and Harris (paragraph 3) average our wounding tax across the regimes to produce a figure of 2.1 animals wounded per animal killed. This is a gross abuse of our data. Our regimes were not intended to represent the spectrum of regimes used in real life, because nobody knows what they are! Thus Baker and Harris erroneously compute a figure of 168 000 foxes wounded each year. Obviously our figure would be much lower because of the use of second shots and dogs, which we discuss later in the paper. But curiously, they themselves (Baker & Harris 1997) admitted that according to their own estimates 115 000 fox deaths per year remain unaccounted for. Is there a connection between these two figures? For example, Bertsden (1999) found that 25% of rural foxes in Denmark carried shotgun pellets from previous injuries (equivalent to part of our wounded group E and all of group F). For statistical reasons one cannot extrapolate; but for this incidence to have accumulated, judging by the ratios of fatal to survivable wounding that we found, clearly a further significant percentage of Danish foxes must also have died of their wounds. That few wounded foxes in the UK survive to reach RSPCA hospitals is not surprising: statistically, attempts to derive initial wounding data from hospital admissions (as Baker and Harris do) are hopelessly flawed and have not been accepted by the scientific community. To attempt this one would need to measure, in reverse order, a succession of probabilities: how many foxes are wounded by shot (the parameter we are trying to measure); how many of these survive additional shots; how many of these escape; how many of these survive and recover; how many of these have a further injury; how many of these survive; how many of these are found by the public; how many of these are taken to a wildlife hospital; and finally, how many of these have their original injuries detected and enter the dataset. Not all shot animals have lead fragments still present or show bone damage.

Our wounding rates relate to the probabilities of wounding per shot fired because this is the only statistically sound starting point. The probabilities of further shots or interventions (such as dogs) quickly killing a wounded fox are real and we have discussed them, but could not measure them. Baker and Harris's paragraph 4 presumption that probabilities for second shots are the same as those for first shots is unfounded. We know of no studies of the effects of second shots on foxes. Data from the Scottish Gunpacks (Fox *et al* 2003) show that 32.7% of 547 shots fired at live foxes were repeat shots. In one case we documented 11 shots fired at one fox, and in the film of the study we recorded five shots at one fox that was eventually caught and killed by dogs.

Baker and Harris select the wounding rate (not wounding tax) from the most humane regime we measured (R12), carried out using skilled shooters recommended by Robert Bucknell. With their single shot they 'killed' 90% of the foxes and a further 8% were 'heavily wounded', and would have probably either been shot again or died quite soon (our groups B–D and a few of group E). Certainly, most of these would be 'bowled over' as Harris claims, but not the 2% that were 'lightly wounded' (group F), nor many of our group E. These could have run off, and the probability of hitting them with a rifle would be much less than that for the original 4 s static target. Real life is seldom as tidy as controlled trials: for example, our data are based on a broadside view of the fox, yet for the second shot the fox is more likely to be a rear view and moving fast; much harder to kill cleanly with a rifle. Experience shows that it can be very hard to find wounded foxes that run off, and sadly, it is these lightly wounded foxes that are likely to suffer for longest.

Baker and Harris could equally have selected our regime S18 in which skilled shooters used AAA pellets at 25 yards (within government guidelines). Their wounding rate was 52% and their wounding tax was 1.3 foxes wounded per fox killed. Even with second shots they would have wounded perhaps 27%.

People learn with practice; some more so than others. Therefore we only allowed two shots per shooter (right crossing and left crossing) per regime and each only did a few regimes, to avoid practice improving their performance. Baker and Harris's proposal for a multivariate analysis requires "each participant to be included equally in all regimes". To do this would require all shooters to fire over 100 shots each without their performance improving in the process. Clearly this would be neither practical nor statistically valid. The aim of our paper was not to test multivariate relationships, such as whether choke is grouped with shooter skill, or to assess shooters as individuals. It was to assess the probabilities of outcomes ('kill', 'serious wound', 'light wound' or 'miss') for a number of regimes based on limited randomised shots fired by large groups of shooters. Baker and Harris's claim that dichotomous variables cannot be normally distributed is wrong. In our experiment we have Bernoulli distributed variables (such as 'light wound' present or absent, 'miss' present or absent). To quote Gotelly and Ellison 2004: "Regardless of the underlying distribution (eg Bernoulli, binomial, Poisson, normal), the Central Limit Theorem asserts that the sums, or averages of large independent samples will follow a normal distribution...". Before running ANOVA we ran normality tests using the Shapiro-Wilk statistic ('*W*'), which is recommended by SAS for samples <2000. This test showed convincingly that all variables tested were normally distributed. W varied from 0.21247 to 0.89232, $P < 0.0001$, across all regimes so there is no doubt that the application of ANOVA was valid. For interest, we also ran non-parametric (Kruskal-Wallis) ANOVA, which produced similar results.

Similarly, it is not strictly appropriate to compare the effects of shotguns versus rifles, as we do in Table 4, not because of the statistical fine detail that Baker and Harris mention, but because they are totally different weapons used in different circumstances. We do so only as a matter of interest; but imagine if we had reversed the tests, offering the shotgunners a static 4 s target and the riflemen a moving target. The results would have been very different. In real life, the shooter does not know how the fox will behave,

https://doi.org/10.1017/S0962728600029572 Published online by Cambridge University Press

whether it will stay still or not, and he can only do his best with the gun at his disposal. Many shooters who are skilled riflemen are unskilled with a shotgun, and vice versa, so even the groups are different.

So we are struggling to apply some science to myriad different scenarios and we are glad that Baker and Harris agree with us that the key issue is the animals that escape wounded. Although we have not attempted to quantify suffering, we do not agree that, as some have suggested, there is no evidence that wounded foxes suffer. On the contrary, we believe that our wounded groups C, D and especially groups E and F, represent a genuine amount of suffering. Anything that can be done to reduce this suffering, whether by repeat shots, the use of dogs, or shooter education, will provide a welfare benefit for foxes.

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