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Risk assessment for animals: should the routine assessment of negative effects of intervention in wild animals be built into research projects?

Juliet Clutton-Brock

From naked mole rats to elephants, research in wildlife biology increasingly entails interference with individual animals, and there are innumerable reasons to justify this interference for the proper management of populations. As Boyd (2002) has emphasized, however, there is a central question about the extent to which it is reasonable to intervene in a population when the species is endangered. This comment arose as part of a debate published last year in this journal on the possible harmful effects of immobilizing black rhinos and fitting them with radio-collars. Alibhai, Jewell & Towindo (2001) had prompted the debate with a discussion on the possible reduction of fertility in black rhinos caused by immobilization followed by an account of the wounding of a rhino by an ill-fitting radio-collar (Alibhai & Jewell, 2001).

Atkinson *et al*. (2002) responded to Alibhai *et al*. with the criticism that it would be unfortunate if there was a backlash against immobilization and radio-collaring, as they are essential techniques for the management of black rhinos. In his introductory contribution to this debate, Boyd concluded with the important recommendation that the negative effects of intervention should be introduced into models for wildlife management together with the positive effects for the preservation of the species. In fact, it is surprising that negative effects are not always considered an essential factor in models of wildlife management, just as risk assessment has become integral to the construction of models for human planning.

The debate on intervention for the sake of management of an endangered species, the black rhino, is now followed in this journal with a rather similar case of the possible deleterious effect of radio-collars on badgers by Tuyttens, Macdonald & Roddam (2002). However, this study of the ecology and behaviour of badgers is not being carried out for their preservation but for their management in relation to the spread of bovine tuberculosis.

Delahay *et al*. (2003) criticize the findings of Tuyttens *et al*. because they say these authors have confounded the effects of two types of collars, the leather and the nylon, and they claim that the leather has better welfare results than the nylon. To this, Tuyttens *et al*. reply (2003) that they were very well aware of the harmful effects of the nylon collars and they use the criticism as a platform to expand on Boyd's (2002) comment that the negative effects of intervention should be taken into account as part of all research in field ecology; the effects are measurable, and they will be increasingly revealed as techniques become more elaborate.

The Editors of journals such as this one have a responsibility to promote high standards of animal welfare and to reject the publication of all research that involves cruelty to animals. All papers that are submitted to this journal that appear to contravene the guidelines on welfare published in *Animal Behaviour* (1996, **51**: 241–246; 2001, **61**: 271–275) are therefore sent to the Ethical Committee of the Zoological Society of London for assessment. There are, however, many borderline situations when it is difficult to decide whether a research project has failed to reach acceptable welfare standards.

One case that often occurs involves the toe-clipping of animals. For much of the second half of the 20th century, the marking of amphibians, reptiles, and small mammals was routinely carried out by clipping their toes. Yet, as far as I know, there is no publication in which an attempt was made to take into account, or measure, possible negative effects of toe-clipping. This method of marking is no longer accepted as a routine by ethical committees, although it is sometimes the only possible means of identifying individuals.

As an Editor of the *Journal of Zoology* I welcome debate on issues to do with welfare science and I see no need, from the exposure of problems, to fear a backlash against the use of common techniques such as radio-collars. Just as toe-clipping has been mostly replaced by the recording of individual patterns for recognition, so there are always new designs and improvements to be made in any research procedure. Perhaps the way forward is for an assessment of the possible negative effects of intervention on individual animals to be included as an integral part of all research projects. This should be as routine a procedure as other forms of risk assessment are becoming in managing impacts on human health and welfare.

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Response to Tuyttens, Macdonald and Roddam

R. J. Delahay*, M. Waldram, P. J. Mallinson, P. D. Spyvee, D. Handoll, A. N. S. de Leeuw and C. L. Cheeseman

We are writing to respond to an article published in the *Journal of Zoology* **257**: 37–42 by Tuyttens, Macdonald and Roddam. The article describes analyses to investigate the potential effects on European badgers of the fitting and monitoring of radio-collars. The results suggest that there is a transient detrimental effect on body condition for up to 100 days post fitting.

Investigation of the effects of field methods on the behaviour and condition of study animals is generally unpopular amongst scientists and the authors should be commended for attempting to address this important issue. Unfortunately however, the results of this paper will not help field biologists to refine radio-collaring studies of badgers, principally because the analyses are confounded by the simultaneous consideration of two fundamentally different collar designs.

During the study, staff from the Central Science Laboratory participated in fieldwork, including the capture of badgers and fitting of radio-collars. Some captured badgers were fitted with a radio transmitter mounted on a nylon collar with a diecast alloy chrome-plated buckle, whilst the remainder were fitted with a leather collar and brass buckle of the type described by Cheeseman & Mallinson (1979). The leather collars were supple and biodegradable, and incorporated a core of untanned hide that prevented stretching. The two types of collar differed fundamentally not only in material, but also in weight and dimensions. Furthermore, the personal experience of CSL field staff with both types of collar strongly suggests that the leather design is far superior to the nylon in terms of animal welfare. The leather collars were developed by CSL field staff following many years of practical experience in radio-collaring badgers. They have been used for many years in the CSL study area at Woodchester Park, and their use in the project described by Tuyttens *et al*. (2002) superceded that of the nylon collars.

The analyses conducted by Tuyttens *et al*. (2002) confounds the potentially different effects of these two collar types, and the results therefore offer little constructive advice for improving this technique with respect to animal welfare. We therefore wish to invite the authors to present results of these analyses with the type of collar included as an explanatory variable. If this is not possible then our advice to ecologists wishing to carry out radio-tracking studies of badgers is to use the leather collar design described by Cheeseman & Mallinson (1979).

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Response to Delahay, Waldram, Mallinson, Spyvee, Handoll, de Leeuw and Cheeseman

F. A. M. Tuyttens*, D. W. Macdonald and A. W. Roddam

The underlying aim of our paper (Tuyttens *et al*., 2002) was to draw attention to the desirability – for reasons of science and animal welfare – of applying the scientific method to test for effects of techniques in field biology. Therefore, we are grateful to Delahay *et al*. (2002) for their response which not only advances that aim, but provides us with the opportunity to elaborate on the importance of the issue. In this reply we focus first on the questions they raise about our analysis and then, second, and more importantly, we set the answers in a broader context of what we foresee as a new era of animal welfare science as a component of conservation biology.

First, Delahay *et al*. (2002) raise the possibility that collar-type (in this case leather versus nylon designs) might affect the effect we described (namely, a short-lived reduced body condition score of radio-tagged badgers). They are absolutely correct that this is a possibility, as was clear in that our paper stated unambiguously that our analyses are based on badgers fitted with leather or nylon radio-collars with brass or alloy buckles and that our findings refer to this dataset only. We were, of course, aware that collar type, amongst other variables, is confounded within our analyses – cognisance of diverse sources of potential confoundment led us to be conspicuously cautious in the conclusions we drew, and in their interpretation. It is also noteworthy that the results we report were not from a study designed specifically to test whether collaring affected the badgers, and therefore the statistical power of the tests to detect any effects is likely to be low. Indeed, we cannot rule out the possibility that these statistical findings are simply the result of the play of chance. All that having been said, while we readily acknowledge the possibility that further study might reveal that comparison of these collar designs explains fully the observed effect, the current evidence inclines us to the view that this outcome is improbable. The fact is that after 1–2 years of using the nylon collar (and alloy buckle) design, we had encountered serious neck injuries in two badgers; discussing these with our collaborators at CSL, and the manufacturer, we decided it was safer to swap to leather collars (with brass buckles). Incidentally, we were familiar with the latter design in that one of us had participated in the development of their prototypes, edited the book in which it is published by Cheeseman & Mallinson (1979), and had for many years used the antecedents of both contemporary leather and nylon designs. However, although no neck injuries were noticed after the swap to leather, in our published analysis we did not include collar design as a variable because (1) the sample sizes were inadequate for statistical treatment, (2) the durations limited, (3) the comparison was confounded by an order effect (nylon first, leather second) during a period when the passage of time introduced another variable (badgers killed for TB control) known to affect various measures of biometry and demography (Tuyttens *et al*., 2000). The sequence of designs (first nylon then leather) raises the possibility that adjusting for age and year in the analyses might act as a proxy for collar-type, but the sample sizes are far short of what would be necessary to detect such an interaction. In short, we judged that our data sustained an analysis of the effects of radio-collaring, but were insufficient to partition these effects by collar-design. However, of course we are eager to explore further whether any effect of collaring can be detected using the leather design, and to this end members of our team continue to collaborate in the field with Delahay *et al*.'*s* team to generate a larger sample for such analysis. However, until that is complete, the advice to use the leather collar design is based solely on a precautionary principle, born appropriately of field experience (exactly as stimulated our initial swap from nylon to leather). While we obviously do (and did) place weight on the insight of such experience, the purpose of our original paper was to illustrate the merit of subjecting it to critical analysis, and to urge researchers to evaluate critically such matters with analyses that go beyond signs of obvious injury or behavioural changes.

While waiting for data to accumulate to sustain further analyses of the particular case of collaring badgers, it is worth asking in general whether there are grounds to expect questions about the effects of methods in field biology to be fruitful. We believe the answer is yes. While it is our opinion that the benefits of field research often outweigh overwhelmingly the costs, it seems obvious that amongst the costs of interventions such as capture and handling is an impact on the subject's welfare. Both scientifically and ethically it is desirable to measure and minimize such costs (Macdonald & Dawkins, 1981). Wild mammals are likely to show acute stress responses when they are captured and handled (e.g. Millspaugh *et al*., 2000; Place & Kenagy, 2000; Read *et al*., 2000; Engelhard *et al*., 2002). The stress response is a wide ranging physiological event, involving metabolic, immune and hormonal change (Moberg, 2000). In general, stress

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responses are associated with increased metabolic rate, and the magnitude and length of this increase depend on the intensity of the stressor (Ågren, 2002), and the energetic cost depends upon the frequency and intensity of the stressor (Laugero & Moberg, 2000; Moberg, 2000). To cover this energetic cost an individual must either use up stored biological reserves or, if these are insufficient, it must divert resources that would otherwise be used by other bodily functions (Moberg, 2000). For example, adult rats subjected to a moderate stressor of 3 hours restraint for 3 consecutive days suffer a reduction in body weight which can consist of both lean and fat tissue, and in addition reduce their food intake (Harris *et al*., 1998; Zhou *et al*., 1999). In their experiments Harris *et al*. (1998) found that during the 3 days in which restraint was applied, mean body weight loss in restrained rats was equal to approximately 5% of their initial weight (a reduction of approximately 20 g), and much of this loss occurred 1 day after the first period of restraint, although restrained animals continued to have lower mean body weights than controls for as long as 40 days after the stressors were applied.

Against this background, we would predict that many standard techniques in field ecology (spanning everything from radio-collaring to Longworth trapping) have measurable effects, and that these will be increasingly revealed as attention focuses on this topic and as new techniques become available to probe it. While we do not know the mechanisms that underlay our original finding of altered condition with badgers, we note that Harris *et al*. (2002) demonstrated that there were three stages to weight loss in response to stress: a period of weight loss during stress; a period of reduced food intake following the end of stress; and an extended period of normal food intake but reduced body weight. The mechanisms responsible for these observed patterns are complex and probably involve interactions between stress-related hormones and other hormones that affect food intake, including growth hormone and prolactin (Harris *et al*., 2002). Analytically, such effects can be a source of confoundment – fieldwork involves many sources of confoundment and they do not necessarily render it scientifically or ethically invalid, but it is desirable that they are recognized and measured and adjusted for wherever possible. In the case of our findings for badgers we certainly did not conclude that the effect we detected invalidated our research – actually, it was rather small and short-lived – but we felt it important to be alerted to its existence. It may have been partly attributable to collar design, and the continuing collaboration between our team and Delahay *et al*.'s should afford an opportunity to explore that further. However, for the reasons given above, we will not be surprised to discover that features of our methods, beyond collar design, have affects on our subjects. Knowing this may allow us to mitigate some, and to be alert to the consequences of others – both outcomes are advantages of the healthy incorporation of welfare science as an addition to the armoury of field biology in general and conservation in particular.

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