Letters

# *Nature* and *Science* know best

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For many researchers, getting work published in *Nature* or *Science* is akin to finding the Holy Grail. Aside from acquiring some high-profile international recognition for one's research, having a paper in one of these journals on your CV, particularly early in a career, can greatly (some might say disproportionately) improve your chances of future employment, promotion and funding. Employers and funding bodies might assume that individuals with *Nature* or *Science* papers are more likely to produce substantial bodies of internationally regarded work in future. Those of us who are less fortunate often wonder whether this assumption is justified.

To examine this issue, I perused departmental websites and the Web of Science (http://wos.mimas.ac.uk/) and examined the track records of academic and research staff from the life sciences departments of British and Australian universities who began their careers (i.e. started publishing) in the early 1990s (1990–1994). I focused specifically on workers in the fields of ecology and evolutionary biology, partly through personal familiarity with these subjects, but also because workers in other fields, such as medical research, have several journals of equivalent impact in which to publish original research (e.g. *Cell*, *New England Journal of Medicine*, etc.). Although ecologists and evolutionary biologists can also publish in other high-impact journals, such as *TREE*, these tend to be review journals. The researchers were divided into those who did, and those who did not publish in either *Nature* or *Science* from 1993 to 1998. I then counted the number of publications in scientific journals that the researchers had produced since 1999. I also noted how many of these papers were published in *Nature* or *Science*.

The differences were striking. Researchers who published in *Nature* and/or *Science* early in their careers (38 out of 259 included in this survey) were, on average, almost twice as productive in the past five years compared with those who did not (mean numbers of publications $=21.08$ and 10.99, respectively; one-tailed unequal variances t-test: $t=-4.27$, df$=40.471$, $P<0.0001$). Even more impressively, these researchers have had over six times as much work, in absolute terms, published in *Nature* and/or *Science* (mean numbers of articles$=1.87$ and 0.29, respectively. $t=-4.59$, df$=38.458$, $P<0.0001$). This equates to 8.9% of their recent research output being *Nature* and/or *Science* papers, compared with 2.6% for those who did not have an earlier article published in those journals. It made little difference whether the researcher was the primary author of their early paper or a co-author [mean numbers of publications$=21.41$ and 20.63, respectively: $t=0.17$, df$=36$, $P=0.44$; mean number of *Nature* and/or *Science* articles$=1.91$ and 1.81, respectively: $t=0.14$, df$=36$, $P=0.44$ (equal variances t-tests used)].

Of course, there is some circularity inherent in this analysis. Researchers who have published in *Nature* or *Science* are likely to attract more funding support, enabling them to produce more research output. Other, less fortunate, researchers could perhaps be equally productive given the same opportunities. Nevertheless, it is difficult to argue that the subsequent benefits received by those who publish in *Nature* and *Science* are undeserved. It would seem that such papers are good indicators of productive scientists, and that funding bodies and universities are justified in holding those papers, and their authors, in high regard. The rest of us (just over two thirds of researchers in this sample who have yet to publish in *Nature* or *Science*) can at least console ourselves with the fact that we are in the majority.

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# Mismatches between conservation science and practice

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The plea for a move towards evidence-based conservation made by Sutherland *et al*. [1] will have struck a chord with all conservation biologists who work closely with conservation practitioners. The proposal for central

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databases to act as repositories of evidence that can then be used to inform decision-making processes objectively is radical, but sadly, might be unworkable unless there are far-reaching changes made to how we carry out conservation. One crucial issue is that evidence inherently accumulates much more slowly than conservation problems arise. To illustrate this, I compared conservation practice (as measured by the number of mitigation projects carried out in response to development threats [2]) with conservation science (as measured by the number of peer-reviewed papers published) for one protected species – the great crested newt *Triturus cristatus* – in England over a 12-year period (Figure 1).

In spite of an exponential growth in the number of newt mitigation projects, the annual scientific output about this species has remained in single figures for most of this period. The only year in which the number of papers reached double figures (2000) was one in which a special issue of the *Herpetological Journal* was published, focusing specifically on great crested newt conservation and management. As protected species go, the great crested newt is relatively well researched, but it is clear that the accumulation of evidence has not managed to keep pace with the rate of increase in the conservation management of this species. This has led to ecological consultants who carry out much of the mitigation work believing that scientists are not addressing the most important questions, and scientists believing that their results are being ignored [3]. As alluded to by Sutherland *et al.* [1], critical analyses of evidence should also embrace data based on experience that might not necessarily have been published in journals. Indeed, a wealth of such information might be available from great crested newt mitigation projects. Unfortunately, consultants appear reluctant to release such data to country agencies, even when there is a legal obligation to do so – the reporting rate of great crested newt mitigation projects is $<50\%$, and when reports are provided the quality of the data is variable [2].

There will always be threatened species that need management decisions to be made quickly. In such cases, crisis management, rather than evidence-based conservation management, will invariably be implemented. When there is little evidence available, doing nothing might sometimes be the best 'worst' option, however politically unacceptable this might seem.

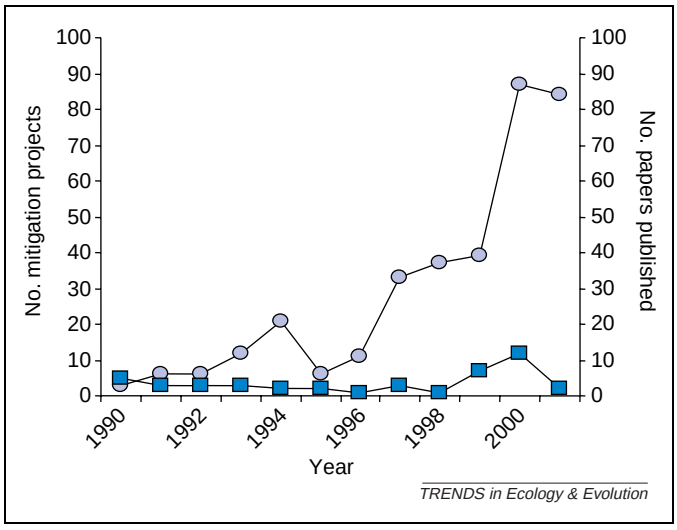


**Figure 1.** A comparison of the number of great crested newt mitigation (conservation) projects (circles) and the number of great crested newt scientific papers (science; squares) carried out between 1990-2001. Mitigation projects are based on the number of licences issued by government agencies [2]; the number of papers is based on a literature search using the Web of Science (http://wos.mimas.ac.uk/).

## References

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Letters Response

# Response to Griffiths. Mismatches between conservation science and practice

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We agree with Griffiths [1] that there is currently a serious mismatch between conservation science and practice, as well as a serious lack of monitoring of the consequences of conservation actions. For example, Griffiths shows that, between 1990 and 2001, there were 345 known great crested newt *Triturus cristatus* mitigation projects in the UK [2]. If the details of mitigation and some measure of success had been recorded for each and stored in a widely accessible manner then there would now be a wealth of information available, which could be systematically reviewed along with research and information from other countries to provide guidance that is based firmly upon evidence. If an appropriate systematic review had been undertaken ten years ago, and had concluded that

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