## LETTER TO THE EDITOR

The Editor
Sir

**Conservation status of New Zealand birds, 2008: science or assertion?**

The publication in *Notornis 55(3)* of the paper *Conservation status of New Zealand birds, 2008* (Miskelly *et al.* 2008) brings to mind the distinction made between a fact in law, which is established by argument and enshrined by precedent, and a fact in science, which is established by observation and enshrined by repeatability.

This paper arises from the deliberations of an "expert panel", convened by the Department of Conservation (DoC) and following DoC-prescribed guidelines (Townsend *et al.* 2007), and provides conclusions and analyses arising from a focal categorising exercise. Whether this paper should be viewed as an argument seeking to derive enshrinement (and credibility - see Townsend *et al.* 2007, p.16) from its publication in a science journal, or as a summary and interpretation of verifiable observations arising from a robust scientific process, rests on the evidence provided to support the conclusions drawn. Regrettably, no evidence verifying the allocation of any species to any one of the defined threat categories is provided or referenced, and yet the bulk of this paper, an analysis of species shuffling between threat categories, rests entirely upon the validity of those assignments.

The various categories of "threat" to which 428 taxa have been assigned are defined in the legend of the paper's Appendix 1 and in its Table 1. The primary criteria are number of mature individuals (presumably birds older than the minimum recorded female breeding age although this term is not defined in the paper) and trend in population over the past 10 years or 3 generations, whichever is longer. Classification to category, as presented in this paper, is thus based entirely upon number even though definitions of threat categories in Townsend *et al.* (2007) prescribe 2 additional criteria – number and size of sub-populations (a measure of population fragmentation) and total area of occupancy (a measure of distribution).

The sources of data for this study "included that used for the previous listing (Hitchmough *et al.* 2007) and the *Atlas of bird distribution in New Zealand 1999-2004* (Robertson *et al.* 2007), supplemented by public submissions and expert opinion". The placement of taxa into the prescribed threat categories "was based on…submissions, panel knowledge and referral to recent publications (especially Robertson *et al.* 2007)". No other sources of information are identified and no "recent publications" are included in the literature cited except for two that comment on, but do not quantify, *Anas superciliosa* x *A. platyrhynchos* hybridisation.

The above quotations from the paper's Methods section indicate there was specific reliance on the *Atlas* as an indicator of change. But the *Atlas* is a record of recent distribution, not of numbers. Furthermore, it can only indicate distributional change of a taxon if there is a previous record of its distribution with which to compare. For a great many taxa the only other (but less intensive) distributional record is likely to be the previous *Atlas* (Bull *et al.* 1985) covering the period 1969-1979. Although some comments on distributional change are made in the 1999-2004 *Atlas* (see Robertson *et al.* 2007; Appendix K), its authors are at pains to point out (p. 6) that absence of a record of a taxon from a locality (recorded at a 100 ha scale) is not evidence of its absence. Presumably the same point can be made even more emphatically about the 1969-1979 *Atlas*.

From Hitchmough *et al.* (2007) one is directed to a web-based spreadsheet (Dept of Conservation, undated) in order to scrutinise the other primary source of information used in this paper. This spreadsheet lists 289 taxa and provides comments on status change between 2002 and 2005 for 100 of these. No references to data, reports or papers which inform these changes are given. Likewise, in Hitchmough *et al.* (2007, pp. 14-15) where changes in the assigned threat status for 15 species are discussed, no references of support for comments about population change are provided.

My attempts to find the equivalent spreadsheet for the 2008 deliberations (summarised in Appendix 1 of Miskelly *et al.* 2008) have been unsuccessful (as at 13 July 2009). I understand one exists within the DoC (E. Neal *pers. comm.*) but it was not publicly available 4 months after publication of this paper and 12 months after its submission. There is, however, no indication in the paper that any such explanatory spreadsheet is available to be viewed and scrutinised.

Being unable to appraise the sources of population information used in this paper also precluded me from understanding the techniques used in population surveys presumably completed for many species over the past 3 years; this paper reports that the threat status of 32 species have altered because of numerical changes since 2005. Possibly for some species their present numerical status is an extrapolation of two or more historic counts since Table 1 indicates population trend as being ""predicted and ongoing due to existing threats" but there is no indication as to which species were treated in this way, and what methodology was used. This is important since changes between categories can be based on an identified 10%

population change, an exceedingly brave call. Those who conduct determined population surveys would be absolutely over the moon if they thought their methods were robust enough to detect a ±10% population change with 95% confidence, especially of birds that are mobile, cryptic, patchily distributed, occur at low densities, or have a wide geographic presence.

In the absence of verifiable information supporting the allocation of taxa to the various threat categories, the veracity of subsequent constructions of patterns of extinction, threat or rarity in relation to taxonomy, habitat grouping or geographic distribution (Tables 2-5) defy appropriate scrutiny.

This paper reports the outcome of a DoC categorising process. It follows an honourable line of international (e.g. Fisher *et al.* 1969) and national (e.g. Williams & Given 1981; Bell 1986) attempts to draw attention to the conservation plight of birds by assessing their risk of extinction. These were advocacy documents, not science papers, even though categorisations in subsequent treatises have attempted to use increasingly robust, science-based criteria (e.g. Mace & Lande 1991). However, the lists *per se* contribute little to ornithological or conservation science. Rather they serve as reminders of avian plight and as crude measures of change over time.

The Department of Conservation's requirement for, and purpose of, threat classifications of New Zealand biota have been identified thus

> *" An effective species threat classification system provides a fundamental framework to biodiversity recovery programmes. In order to demonstrate the value of conservation, (we) must establish objective benchmarks to determine the risk of extinction faced by each species, and then assess each species over time. This provides a demonstrable measure of the level of conservation management, and its effectiveness……..The risk of extinction faced by a species is one of the main factors used when prioritising use of conservation resources"* (Morrison *in* Townsend *et al*. 2007).

In short, the exercise described in this paper was intended as a prioritising and measurement tool suited to DoC's particular bureaucratic needs. In addition, it has been used as an advocacy tool, as the DoC media release of this paper coincidental with its publication confirms (Department of Conservation 16 April 2009). Doubtless it will find a similar use in other advocacy circumstances including informing judicial processes, e.g. environmental consent hearings.

Perhaps this paper should not have been published within an overtly science environment. By neither declaring its analytical procedures nor identifying its sources of data beyond a publication that is inappropriate for the intended task, this paper falls short of the standard I expect of a science publication. The threat ranking outcome, presented in Appendix 1 is much better suited to publication within the DoC's own publication system in the same way that previous versions of this exercise have been (e.g. Hitchmough 2007) and for listing, with explanatory comments, on the DoC website (e.g. Department of Conservation undated).

The belief that "formal publication (in peer-reviewed literature) enhances the scientific credibility of the lists" (Townsend *et al.* 2007) is sadly misplaced. Better that public-funded works of this nature are placed where the greatest public benefit can be delivered and where credibility can be derived from the standard of the scholarship on display.

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