

Dirachma socotrana

I am writing in connection with the article, *Dirachma socotrana* – back from the brink? (Bazara'a, Guarino, Obadi and Miller, 1991, *Oryx*, 25, 229–232). I am afraid that the authors have been misled concerning the vernacular name of this species. Dr Georg August Schweinfurth, besides being an outstanding botanist and extensive traveller, was also an accomplished Arabist, and lived for many years in Cairo. I would, therefore, be inclined to think twice about calling into question any information he gleaned on the subject of plant names in the Arab world and surrounding regions. He recorded two variants for the vernacular name of this species, namely 'Dirachma' and 'Rachman'.

However, there were on our 1967 expedition to Socotra two linguists, the late Dr Tom Johnstone of the School of Oriental and African Studies, University of London, and Mr Mike Tomkinson, at that time of the British Embassy in Jeddah, Saudi Arabia, and a Fellow of the Institute of Linguistics, as well as Mr J. J. Lavranos, an extensive traveller in Arab lands and also a fluent Arabic speaker. All of these three, independently, recorded names for the species that differed in no significant respect from the names recorded by Schweinfurth 86 years previously. These were: 'Dirahmim' (Lavranos, myself being present); 'Di-rahmim' (Johnstone, MS list) and 'di-rahmim' (Tomkinson, TS list). Q. Cronk in 1985, moreover, recorded the name 'Drahman', which is again similar.

None of us ever recorded this name for *Cephalocroton socotranus* (not *socotrana*, as in the paper, because *Croton* is masculine), which, as the paper states, is the 'tahn'. This latter is the only name, or slight variants of it, for the *Cephalocroton*, which, as far as my observations went, was never cut for timber (it is only a shrub anyway, and not a tree, as stated in the paper, and therefore its wood is not very substantial). The variants are: 'tehn' (Balfour, Cockburn and Scott); 'than' (Schweinfurth); 'ta'n' (Tomkinson) and 'ta'an' or 'tann' (Cronk).

The authors of the paper, unfortunately per-

petrated another error by ascribing the name 'tifit' to the *Dirachma*, thus obfuscating the issue still further! This is clearly a variant spelling of the 'Difeth', which is the name Schweinfurth recorded for *Placopoda virgata* Balf.f., an endemic rubiaceaceous genus on Socotra, since Lavranos and I recorded another and similar variant for this species, namely 'taifet', on two separate occasions. This shrub is rather rare and, as the specific epithet implies, only produces slender twigs; it is not therefore of any commercial value.

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To collect or not – continued

It is surprising to find Sir Christopher Lever (*Oryx*, 26, 173–174) falling back on one of the oldest devices of those running out of facts in an argument. In the past it was usual in a religious controversy to suggest that the other side was somehow lacking in 'Faith', which immediately discredited them. In the modern equivalent it has become usual for one side to accuse the other of lacking 'understanding'.

Surely with universal television, such accusations of incomprehension have become as obsolete as Bishop Wilberforce's attack on Darwin? Nearly everyone 'understands' conservation issues these days; the problem is that some do not agree with the solutions proposed, and they will not be converted by accusations of a lack of 'understanding'.

In point of fact, taxonomists have often been only too eager to adopt new techniques, sometimes before they were adequately investigated, and pictures in particular have been treated as acceptable types since the start of taxonomy. But now that it is being realized that in many groups of organisms there are sibling species that are difficult to tell apart, it is increasingly necessary to have reliable types, and some of the characters cited by Sir Christopher such as genetic material are not so easily related to ordinary experience of normal organisms.

If Sir Christopher wishes to resort to science in support of arguments against collecting,

this in fact merely demonstrates the need for it, because not only is it desirable to preserve a wide variety of good genetic material of threatened forms for use in future investigations, but one day it might also be possible to use this material to reconstitute interesting lost organisms.

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Lake Nakuru Black Rhinoceros Sanctuary

The letter on the Lake Nakuru Black Rhinoceros Sanctuary (*Oryx*, 26, 174–177) states, 'This sanctuary was always intended as a sanctuary for both black and white rhinos...'. Why? The white rhinoceros is not indigenous to Kenya, and those imported have been the southern subspecies, *Ceratotherium simum simum*, which is not endangered. There might be something to be said for Kenya trying to help conserve the seriously endangered northern subspecies, *Ceratotherium simum cottoni*, even though it too is not indigenous. But it seems misguided to expend what are no doubt limited resources on the southern form.

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As the author of the article, Lake Nakuru Black Rhinoceros Sanctuary (*Oryx*, 24, 90–94), may I please be allowed to comment on some of the points raised by Mr R. T. Elliott in his letter (*Oryx*, 26, 174–175)? I write as a patron of the Rhino Rescue Charitable Appeal Trust since its inception in 1985, and as a trustee from 1986 to 1991.

1. Mr Elliott says that 'this sanctuary was always intended as a sanctuary for both black and white rhinos ... this point should have been mentioned'. This is not so. Rhino Rescue's brochure clearly states that the Trust was set up specifically to try to halt the decline in numbers of the black rhino, and does not mention the white rhino at all; the later introduction to Nakuru of a few of the latter was purely incidental.

2. Mr Elliott says that 'it was, indeed, due to excess numbers of both species that the Rhino Rescue started'. This, too, is incorrect. The Rhino Rescue Trust was, as stated above, formed solely to try to protect the diminishing population of black rhinos. The only place in Kenya where there was an excess of numbers was on the privately owned Solio ranch – an excess that the transference of animals from Solio to Nakuru helped to alleviate.

3. Mr Elliott says that 'Peter Jenkins and I decided to ... set up rhino sanctuaries elsewhere', implying that he had a hand in the formation of the Lake Nakuru sanctuary. He had no part in it whatsoever.

4. Mr Elliott claims that my statement that 'a further 11 females are due to be added to the park shortly' is 'pure speculation'. This was the figure given to Count Coreth, the founder of Rhino Rescue, by Dr Richard Leakey and Dr Rob Brett of the Kenya Wildlife Service.

5. Mr Elliott is correct in saying that the voltage aimed at for the electric fencing is 7000 and not 700 as stated; this was a misprint.

6. Mr Elliott says that rhinos introduced to Nakuru were released directly into the sanctuary, and that only injured animals were first acclimatized in the holding pens; in general this is correct.

7. Mr Elliott claims that my statement that 'the founding stock was deliberately kept low to encourage the animals to breed more freely' was 'never true for black rhinos'. I am afraid that Mr Elliott has clearly not read my article very carefully, or he would have realized that I was referring to the founding stock of black rhinos at Nakuru, and not to that at Solio where he was Warden.

Mr Elliott concludes by saying that I was 'given false information by other people and that (I) did not check my facts with those directly involved with the operations'. As my article made clear, it was written after a visit to Nakuru, where I was able to see the Rhino Rescue Trust operating at first hand, and after discussing at length the work of the Trust with Count Coreth and the Trust's Administrator in the sanctuary, Mr Jock Dawson.

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