

- Sørensen, T. 1958. Sexual chromosome aberrants in apomictic *Taraxaca*. Bot. Tidsskr. 54:1-22.
- Sørensen, T., and Gudjonsson, G. 1946. Spontaneous chromosome aberrants in apomictic *Taraxaca*. Biol. Skr. K. Dan. Vidensk. Selskab. 4:1-48.
- Stebbins, G. L. 1957. Self-fertilization and population variability in the higher plants. Am. Nat. 91:337-354.
- Steiner, E., and D. A. Levin. 1977. Allozyme, S. I. gene, cytological and morphological polymorphisms in a population of *Oenothera biennis*. Evolution 31:127-133.

RICHARD J. ABBOTT

BOTANY DEPARTMENT
THE UNIVERSITY, ST. ANDREWS
FIFE, KY16 9AL
SCOTLAND, UNITED KINGDOM

Submitted October 25, 1977; Accepted June 21, 1978

JANZEN'S REPLY

Abbott (1979) states that "Janzen (1977) provides no details of which species or sections of the genus *Taraxacum* he is considering." The last three sentences of the first paragraph of my note are a succinct ecological description of the kind of organism I am considering; what Latin binomial has been attached is absolutely irrelevant. Incidentally, by Abbott's cited calculation it appears that my model dandelion is representative of 87% of the 2,000 microspecies in the genus, which is considerably more in focus than nature allows most ecological models.

It is very easy to see why I stated "there should be selection for only minimal outcrossing among EI [evolutionary individual] dandelions, since the habitats and resource bits for an EI dandelion are highly invariant. I have trouble thinking of a more monotonous resource than 6 square inches of new mud." I'll bet Abbott a case of beer that the traits of 6 square inches of new mud have not changed since the Cretaceous, or at least the Pliocene, if you must. Of course there is marked variation between this and that small patch of bare ground; but the EI dandelion is trying to harvest all or most patches of a certain kind within its reach, and there is no evidence that this resource in aggregate changes over a time scale such as to select for constant high levels of outbreeding to keep the genome picking up new genetic information required to deal with an ever-changing environment. While the environment "new diseases, new competitors, new pollinators, new herbivores, new dispersal agents, and the loss of any or all of them" is in a constant state of flux, the environment "specializing on harvesting new bare bits of earth" has probably not changed in a good 50,000,000 yr or more. Even with global changes in climate all that changes is the location of the bare bits of earth, not the essential trait of being bare.

I think I remain quite happy with Abbott's observation through Solbrig (1970) that there are only four genetically distinct types of dandelions in North America; this is, irrespective of the character states used to ascertain it, rather less variation than I would expect to be found in a continent-wide genus of regularly outcrossing

plants. Even if there were ten times as much genetic variation encountered in North American dandelions, it would not change my feeling that "the EI dandelion is easily viewed as a very long-lived perennial organism" ... that "survives by the harvest of resources most easily described as 6-square inch bare bits of ground. Its searching strategy [horrible word, should have said 'searching pattern'] is to repeatedly spread itself very thinly over an area that is likely to have a number of these resource bits." Likewise, with this in mind, I repeat the focus of my note. The population ecology of dandelions, as evolutionary individuals, as units of selection, is essentially unknown. What is the death, invasion, replacement, turnover, genetic change, growth or population growth rate of the set of dandelion clones that occupies North America and Europe? That is the population biology of dandelions. Odd, the aphid people have long recognized that it is useful to think in terms of clones as "superindividuals" (e.g., Blackman 1978) and worry over the fate of these organisms, but somehow taxonomically-oriented biologists want to view aphids and dandelions as different beasts.

LITERATURE CITED

- Abbott, R. J. 1979. Janzen's Dandelions: a criticism. *Am. Nat.* 114:000-000.
 Blackman, R. 1978. An aphid ... is an aphid ... is an aphid. *Aphidol. Newsl.* 14:1-3.
 Janzen, D. H. 1977. What are dandelion and aphids? *Am. Nat.* 111:586-589.
 Solbrig, O. T. 1970. Genotypic variation within and between populations of the common dandelion, *Taraxacum officinale* Wigg. *Isozyme Bull.* 3:43-44.

D. H. JANZEN

DEPARTMENT OF BIOLOGY
 UNIVERSITY OF PENNSYLVANIA
 PHILADELPHIA, PENNSYLVANIA 19104

Submitted September 22, 1978; Accepted October 10, 1978

FACTORS AFFECTING REPTILE BIOMASS IN AFRICAN GRASSLANDS

Janzen (1976, p. 394) draws attention to the, in his opinion, "very low density of reptiles in a wide variety of African habitats" as compared with the Neotropics. He argues on the basis of much anecdotal information (p. 394) that this "is due to exceptional predation pressure on reptiles by a large array of carnivores that are maintained in two ways by the exceptionally large biomass of large herbivores in these habitats." First, herbivore dung and herbivores as carrion provide alternative food sources for the carnivores that are reptile "specialists," and thus support higher population densities than would otherwise be possible. Second, Janzen discusses the "incidental depressant impact" (p. 371) of carnivores which are herbivore specialists but occasionally take reptiles. Finally, Janzen also briefly mentions reduction of cover by herbivores.

Irrespective of the validity of the basic premise concerning relative reptile density