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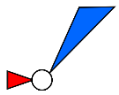
<1745/c>	importance either in clover or in some related species, though sometimes under very specialized circumstances. The scientific method may sometimes mislead. We commonly test for the selective value of a particular feature by holding background variation of both genotype and environment at a minimum. We thereby maximise our chance of demonstrating what we are looking for. The real measure should be whether selection is significant against normal levels of background variation. This is why it was important to test the effects of cyanogenesis and reaction to neighbours in the field. It is difficult to believe that any of the characters examined by Burdon could be selectively neutral. However, the contribution of each property to fitness must vary dramatically from year to year as well as from place to place within the field. Most winters at Henfaes (only 400 m from the sea) are mild and frost is rare. Occasionally, as in the winter of 1982, there is severe frost. Populations of molluscs fluctuate wildly from year to year in North Wales. In some years spring growth of the sward is vigorous and exceeds the capacity of sheep and the other grazing animals to keep it fully grazed. In other years, as in the spring of 1982, a protracted spring drought slowed the growth of the sward and it became tightly graze. A severe drought is not a common feature on the field but when it occurs it may be in any month from April to September. During the period of our observations we have detected three significant leaf pathogens on **white clover** in the field, Uromyces trifolii, Cymadothea trifolii and Pseudopeziza trifolii. It seems unlikely that these three diseases attack with equal intensity in all seasons and in all years. In a field that is patchy in space and time, be it ever so small, we may expect that the populations of a species such as **white clover** will, at any time, reflect selective forces from its past. The genotypic composition of the population may in some cases dimly reflect forces that operated twenty or thirty years ago. Other selective forces may have operated quite recently and left a strong memory or image in the structure of the population's genetics. If this is the case, we would expect to find only a few of the many polymorphisms readily interpretable as responsive to present proximal selective forces. Much of the polymorphism could be transient and, without an even more detailed history of the field, uninterpretable. It is doubtful whether such an explanation of naturally occurring polymorphism could be tested without long-term, detailed recording, not only of the variety of genetic changes occurring within **clover populations**, but at the same time of a detailed recording of the known hazards in
	**the life of the clover plant**
Key: **Footprint** **ConEn1** **Footprint** **ConEn2** **Footprint** **ConEn3**	over the seasons and the years. Conclusions The studies that I have described, concentrated in the field at Henfaes, are now being extended by deliberate experimentation within the field. We are destroying the site as a long-term study on a supposedly stable system by introducing a variety of perturbations such as transplant experiments, the creation of islands for invasion and further perturbations are planned. The study has involved a curious concentration of effort in one very specialized environment. The type of observations that have been made have been

	quintessentially Darwinian. Another great naturalist, Thoreau, has focussed attention at the same scale: 'Nature will bear the closest inspection. She invites us to lay our eye level with her smallest leaf, and take an insect-view of its plain'. If we are to see evolutionary processes in action in plant communities and the proximal events determining their character we must focus our attention away from an anthropomorphic scale of acres or square metres and onto a scale appropriate to the organisms with which we are concerned. The appropriate scale is determined by the organism and not by us. It will be different for different species. We ask for a plant's eye view of life and death in a sward and hope ultimately to be able to collect these reductionist observations into statements about the population, the species or even possibly the community. I doubt if it is possible to hold the view of Margalef (1968) that 'Relevant evidence does not consist of a massive accumulation of trivia' and reconcile it with his 'Ecology … is the study of systems at a level at which individuals or whole organisms may be considered elements of interaction …'. It was, indeed, from the massive accumulation of trivia and tiny details, that Darwin assembled the evidence for The Origin of Species. In a volume commemorating Darwin's death, I have tried to show how his way of looking at the behaviour of individual plants in nature can be extended. A hundred years after his death his approach seems more relevant to botanical studies than it has ever been. This part of his intellectual legacy has not yet been fully invested. A part of the legacy, however, ceases to bear interest. He was writing in the Origin for readers most of whom were steeped in Victorian optimism, religion and the romantic movement. It was necessary in 1859 to write about the process of evolution as if it produced the best of all possible worlds, a substitute for the finger of the Almighty at work. If the process of evolution had not been presented in this way (though with careful caveats) it is very questionable whether it could have been accepted so rapidly by Victorian society. It was then appropriate to show how '… from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved'. But 'beauty' and 'wonder' are in the eye of the beholder and that eye has itself evolved. The teleology of evolution as a goal-seeking activity persists in indefensible form a hundred years later in the writing of biologists. This particular heritage may be a millstone around the neck of scientific natural history. Most particularly, it harms biology