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An 'elusive' leaf beetle from Mexico

[Kumar, A. R. V., Curr. Sci., 1992, 63, 729–739]

'Mexican beetle' is not an internationally established name for the beetle introduced into India from Mexico for suppressing *Parthenium hysterophorus* L. (personal communication, Editor-in-Chief, *Agriculture, Ecosystem and Environment*, 1992). Another leaf beetle, *Epilachna varivestis* Muls is commonly called Mexican bean beetle[1]. None of the workers abroad have labelled *Zygogramma* as 'Mexican beetle'. We are not sure if this is the only beetle introduced into India and other countries from Mexico. We suggest that use of 'Mexican beetle' for *Zygogramma* would be a cause for confusion.

When *Zygogramma* beetles collected on *Parthenium* during 1992 from three different districts in Karnataka, South India, were sent to CAB International Institute of Entomology, London, UK, and to the selected three Entomologists in the USA, the beetles were identified as *Zygogramma conjuncta* (Rogers) by the CAB and one entomologist from USA. The other two entomologists were unable to identify the beetle To be sure of the identity of the beetles, further correspondence confirmed that indeed the species is *Z 'conjuncta* So *Zygogramma bicolorata* Pallister was not the species introduced into India[2] in 1984 and into Australia[3] in 1980 for suppressing *P. hysterophorus* (personal communication, International Institute of Entomology, UK, 1992). *Z. conjuncta* was first described by Rogers in 1856 from Kansas[4]. Later in 1953, Pallister[5] collected and described this species from Mexico while on a David Rockfeller Mexican Expedition of American Museum of National History. *Z conjuncta* is a species different from *Z. bicolorata* and only the experts (beetle taxonomists) can reveal reason for the change of the species.

As *Z conjuncta* is found feeding on *Xanthium*[6] which belongs to the same family as the host plant in India – *Parthenium, Xanthium* might well be within the host range of the beetle. The information serving as a basis for feeding is already stored within the central nervous system and is within the innate host range of the insect[7]. Jermy *et al*[8] outlined conditions under which a modification of feeding behaviour may occur. Thus, 'expanding its host range'[6] is inappropriate. Kumar[6] indicates that the beetle was mass-released in Karnataka In fact, the beetle was released at a site in Bangalore[2]. Kumar[6] indicates that the beetle feeding on sunflower was 'strongly contested', citing two references, one of Jayanth and the other of Pandey. The contests were made largely based on the literature published in the past and not based on pertinent experimental data. Also Sridhar[9] replied to the queries raised by Pandey. We expressed our views based on experimental data and field observations and did also circulate a note (mimeographed) indicating that the beetle is indeed 'a pest' on sunflower in the meeting (cited by Kumar on 12 February 1992). The author may also recollect a sequence of colour photographs showing rate of defoliation by the beetle at 24-h intervals on 'Morden' cultivar of sunflower under confined conditions, circulated at the same meeting. Therefore, Director-General, Indian Council of Agricultural Research, New Delhi, constituted a 'Fact-finding Committee'. The Committee after conducting surveys and investigations found the beetle feeding on all the 28 genotypes of sunflower As a result, the Government of India stopped mass multiplication and all releases of the beetle throughout the country[10, 11].

Host specificity tests conducted by authors cited under refs 4, 8, 9 and 10 of ref. 6 on five species of *Zygogramma* pertain to only before or at initial stage of release. Specificity for feeding by an insect depends on a number of factors[12] and it is crucially important to conduct proper feeding tests for achieving meaningful results. Feeding tests using cuttwigs yield erroneous results It is a pre-requisite to starve the beetles before initiating feeding tests. It is also always rather essential to mention at least briefly the conditions under which the feeding tests were conducted. It is better to express the amount of food fed by an insect for a period than on a day basis to account for the damage on the crop-plant. Also in *Z conjuncta*, feeding is discontinuous[13]. Migration implies a two-directional movement What Kumar[6] has observed is probably immigration or orientation (short distance)

The leaf beetle from Mexico, *Z. conjuncta* seem to have eluded entomologists in Australia, where it first proved effective and host-specific at a site but later failed as a bioagent against *Parthenium*[15] and switched on to *Ambrosia*, a weed. In India, the beetle first appeared to be host-specific and monophagous[16], but later the adult turned oligophagous[13]. Now, by feeding on *Xanthium* it has prompted the entomologist[6] to suggest that it has extended its host range.

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What is in a name: The case of Mexican beetle

A. R. V. Kumar replies:

The central theme of my article[1] seems to have 'eluded' the attention of Chakravarthy and Bhat[2]. Yet, I wish to record my reply mainly to clarify a fresh issue they have raised, the nomenclature of Mexican beetle

What is in a name?

All scientific names (binomens) are governed by International Codes[3]. However, biologists know that few countries, if at all, have their own 'lists' of common names for convenience but no 'codes' as such (e.g. ref. 4). I wonder if the editor of *Agriculture, Ecosystems and Environment*[2] will agree if we call *Parthenium hysterophorus* L. as 'congress grass'. Yet, we all use it, the way 'Mexican beetle' is being used for *Zygogramma bicolorata* Pallister[1, 5–7].

The history of the two latin names of the 'Mexican beetle', as traced by the authors, clearly suggests that it is the same entity (or the species) which carries both the names. The problem, therefore, is to choose the right name[3], as rightly pointed out by them: '. . . experts (beetle taxonomists) can reveal the reason for the change of *species*' (italics mine, it is important to note that the word should be 'name' since no evolutionary questions like speciation are being addressed). Therefore, the conclusion that '. . . *Z. conjuncta* is a species different from *Z. bicolorata* . . .'[2] is premature and unfounded.

However, preliminary taxonomic studies involving a comparison of different populations of the introduced beetles[1] clearly showed that there was perhaps no mix-up of species at the time of introduction. The claim that *Z. exclamationis* (a third species!?) has been introduced along with *Z. bicolorata*[8] is therefore baseless and highly subjective. Consequently, it is absolutely necessary to retain the name *Z. bicolorata* Pallister for the introduced beetle until such time as detailed taxonomic studies of the type material of the three species become available.

'Christening' the beetle may be a matter of contention for Chakravarthy and Bhat[2], but my conclusion that *Z. bicolorata*, by any other name, would feed on *Xanthium strumarium* L. still stands

Expansion of host range and the central nervous system

It is not clear how the authors have related the 'innate host range-central nervous system' theory to my findings This is because their references[9, 10] deal with 'induced preference' of herbivorous insects and are quoted out of context. While it is widely known that phylogenetic considerations may serve as useful guides in testing host plants for a herbivorous insect[11], it is also accepted that 'innate host range' is not necessarily governed by taxonomic affiliations[12]. Therefore, finding an insect taking to a plant to which it has never been exposed previously is nothing but a record of expansion of host range[1].

Claims and counter-claims

There are a few other minor points which I shall try to clarify.

The authors seem to be convinced that they have established the proof of Mexican beetle being a 'pest' on sunflower[2]. As we shall see, this is not true. The fact that the Government of Karnataka and also the Government of India[2] constituted committees to look into this matter amply proves that the issue was not resolved at the time of the first meeting convened by the Institution of Agriculture Technologists[1]. The initial evidence[13] was withdrawn by the authors and their supporters, which in essence called for a re-evaluation of their claims[1, 5, 6]. Although there is no published evidence of *Z. bicolorata* feeding on sunflower to date, I do accept that it nibbles at sunflower (but see refs. 6 & 7), based on my own field observations and experiments by members of the committee[6]. Unfortunately, sustained attempts have been made through the local press[2, 13] to push the idea that Mexican beetle is a 'pest' on sunflower. However, the committee appointed by the state government has summarily rejected this claim after thorough investigations[6].

I do not agree with the suggestion on how the feeding rate data should be presented[2]. No insect feeds forever at a uniform rate and hence mean and standard deviation of feeding rate as 'area fed per day per beetle' is perfectly valid. I find no other reasonable way of presenting these data.

The objection to the use of cut twigs of plants and lack of pre-starvation of the beetles in my experiments is not sustainable. The reduction in attractiveness of cut twigs and the lack of pre-starvation of beetles may have influenced the feeding rate and resulted in underestimates. If I had taken care of these two factors, the beetles would have certainly fed more! And, anyway, it was not my intention to force-feed the beetles.

The authors argue that there were no mass releases of the beetles and yet go on to claim that the Government of India is trying to stop it[2].

Thus, every claim made by the authors[2] seems to be on loose grounds, it is a pity that they made little effort to critically examine my findings. In addition, what they miss out is the implication of such a finding on which Ganeshaiah and Uma Shaanker[15] have elaborated. Therefore, to me, more than the 'Mexican beetle', the purpose of the rejoinder[2] seems elusive.

It is widely accepted that wherever it was released, *Z bicolorata* has significantly reduced the population of *P hysterophorus*[5, 6], but a rigorous proof is lacking. There is an urgent need now to establish quantitatively its impact on *P. hysterophorus* and to investigate the resultant changes in the composition of flora and fauna. A proper analysis of the changes that might have cascaded through the plant community, due to introduction of *Z bicolorata*, should not only serve as an excellent guide for any such biological control programme in future[15] but also tell us more about the functioning of biological communities[16].

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After split genes it is now split proteins

Protein splicing or 'protein carpentry[1, 2]', a term used by some, is the formation of a functional and mature protein by the removal of an intervening segment of a polypeptide from a precursor followed by joining of the flanking regions. It differs from the well-documented proteolytic cleavage in that the latter involves the removal of the polypeptide segment(s) either from the carboxy- or the amino-terminus to create the functional protein. Protein splicing has been reported from both prokaryotic and eukaryotic systems, viz. yeast[3, 4], *Thermococcus litoralis*[5] and *Mycobacterium tuberculosis*[6, 7] which implies that this phenomenon though very widely distributed shares remarkable similarities.

Protein splicing was first reported[3, 4] in 1990 by two groups working independently on the gene encoding the catalytic subunit of vacuolar type proton-translocating adenosine triphosphatase (TFPI or VMA1) in the yeast *Saccharomyces cerevisiae*. Surprisingly, the gene contained a single open reading frame (ORF) encoding a putative protein of 1071 amino acids (119 kDa) which displayed an actual molecular mass of 67 kDa on SDS-polyacrylamide gels. Analysis of the predicted amino-acid sequence revealed a high degree of homology to the catalytic subunits of H^+-ATPases from several different species. Alignment of the deduced sequence with that of carrot and *Neurospora* revealed that the regions of homology mapped to the amino-terminal and the carboxy-terminal while the middle spacer region encoding a putative 50 kDa protein of 454 amino acids did not exhibit any homology. Scientists at the New England Biolabs[5] while trying to clone the Vent DNA polymerase gene from *Thermococcus* found the single ORF apparently coding for a protein of approximately 180 kDa actually coded for one with a molecular mass of 93 kDa. Again comparison of the deduced amino-acid sequence with other DNA polymerase sequences showed that the Vent DNA polymerase contains two intervening protein sequences (IVS1 and IVS2) that interrupt the conserved motifs. In a very similar situation, Davis *et al.*[6] identified a *recA*-like gene from *Mycobacterium tuberculosis* with a single continuous ORF and deduced that the protein encoded by it would have a predicted molecular mass of 85 kDa which, however, turned out to