FOREST MONOCULTURES — HOW SAFE ARE THEY? AN ENTOMOLOGIST'S VIEW

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ABSTRACT

The literature dealing with the risks of insect outbreaks in mixed and pure stands of trees and forest monocultures is briefly reviewed. The widely held belief that forest monocultures are courting disaster because of their extreme vulnerability is refuted.

INTRODUCTION

Arguments about the advantages and disadvantages of forest monocultures have gone on since plantation forestry began, originating in debate on mixed versus pure stands of trees. Pure stands and monocultures are often considered to be synonymous but for this paper monocultures are defined as artificial, even-aged stands of a single crop-tree species. Most of these monocultures, however, also contain many other plant species, ranging from small grasses or ferns to the heavy understorey of indigenous woody shrubs that commonly develop in eucalypt plantations in the central North Island.

None of the following discussion takes into account the risks associated with the narrowed genetic base of the crop species, that may result from poorly conceived breeding programmes. This has been a real problem in agricultural crops but has not, as far as I know, shown up with forest tree crops. I can only endorse the words of Gibson and Jones (1977): "It is to be hoped that forest tree breeders and geneticists will learn from the experience of their counterparts in agriculture and ensure that the risk of new pest and disease problems is reduced to a minimum in their pursuit of improved quality and yield."

REVIEW OF RELEVANT LITERATURE

It is of interest to note what two widely used forest entomology textbooks have to say on the subject of mixed versus pure stands. Although not mentioning monocultures specifically, their remarks are applicable. Graham (1963) says, "Biocoenotic (*i.e.*, community) nonsusceptibility [to outbreaks of forest insects] depends

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on the inherent stability of a complex life community, such as richly mixed forest. Structural and biocoenotic nonsusceptibility are suggested by some as the reason for lack of outbreaks in tropical rain forests." Graham and Knight (1965) are more lucid when they state, " . . we may safely say that the greater the diversification of tree species, the less frequent will be insect outbreaks. This is an illustration of the general ecological principle that other things being equal, the degree of environmental stability is in direct proportion to the number of species living together in an environment."

It is not only entomologists who have favoured mixed stands: pathologists have agreed. A widely used and quoted textbook on forest pathology (Boyce, 1961) says, "Pure stands are more susceptible to diseases . . . a pure stand forms an ideal situation for a pathogen to build up to epidemic proportions . . . the most hazardous pure stands are even-aged . . . " I think it would be fair to say that the pathology texts are far more pessimistic than the entomology ones, and would add that it is therefore no wonder that there is a widespread belief that forest monocultures are very risky. However, having established that, at a basic level at least, the entomologists and pathologists are in agreement I will confine my remarks to the entomological viewpoint.

I agree with Van Emden and Williams (1974) when they say that, although pest outbreaks are often regarded as a general consequence of reducing diversity through monocultures, it is surprisingly hard to find documented examples. The picture is further confused by the fact that the many broad generalisations made almost seem to contradict each other — *e.g.*, Graham and Knight (1965) tell us that the greater the diversity, the fewer the outbreaks, but elsewhere in the same book say, "Certain species are normally found in pure stands, *e.g.*, ponderosa, longleaf, slash, red and jack pines. Natural stands of these are not subject to devastation by pests."

In the literature, generalisations are the rule and specific examples to support the argument are few and far between. The tropical rain forest has long been put forward as the classic example of the stability/diversity relationship. This thesis was however, seriously challenged by Gray (1972) who, in addition to citing various examples of insect outbreaks in these forests, put forward the notion that light outbreaks and local attacks were seldom observed because of the paucity of entomologists in these areas. Gray goes on to ask just what is the "complex system of buffers and checks" that is widely cited as suppressing potential out-

breaks in tropical forests. Way (1977) reckons that the key functional links conferring stability [to communities in general] are likely to be few.

Van Emden and Williams (1974), arguing along community energetic lines, put forward the view that the relationship between diversity and stability is not causative but parallel, and that for this reason the chances are greater that a stable system will turn out to be more diverse than an unstable one. Murdoch (1975) is even more explicit. He states, "the only valid conclusion to be drawn from a comparison of different communiies *of the same type* (*e.g.,* comparing natural communities with other natural communities) is that no correlation has been found between numbers of species (or any other more complex measure of diversity) and stability" — *i.e.,* that there is no good evidence for the dogma that diversity leads to stability.

Most of the above argument refers to natural systems and not to man-made or artificial communities. There is a wealth of literature available on the stability/diversity relationships of man-made communities but with very few exceptions this deals with agro-systems. I suggest that care is needed in drawing conclusions from these types of situations and applying them to forests, which have much longer time scales. When restricting discussion to artificial communities care must once again be taken with generalisations. It appears to be generally agreed that simple artificial communities are less stable (more prone to pest outbreaks for one thing) than natural communities. But exceptions do occur. The large wheat monocultures in the Canadian prairies have been portrayed as being highly stable compared with many natural ecosystems (Turnbull and Chant, 1961). There is evidence to indicate that severe pest problems are aggravated at intermediate stages in the process of simplification of the ecosystem but can diminish in the ultimately simple system (Way, 1977).

With this welter of opinion to contend with and bearing in mind that caution is needed in applying conclusions drawn from natural stands to man-made stands, we should now turn our attention specifically to forest monocultures in an attempt to analyse and define what risks are involved with them. Fortunately there is a recent review article on this topic, namely Gibson and Jones (1977) — "Monoculture as the Origin of Major Forest Pests and Diseases." In the conclusion of this paper, the authors pose the question: "Has the adoption of monoculture systems led directly to an increase in the number and severity of pests and diseases of forest crops?" They then state that the answer would

have to be "Yes" and surmise that much of it is due to the uniform and crowded conditions in plantations. They then qualify this conclusion by observing that few, if any, of the most disastrous outbreaks of forest pests and diseases can be attributed to the introduction of monocultures. Gibson and Jones evidently place much emphasis on the crowded conditions in plantations but this is a characteristic shared with many natural stands; indeed, many natural stands are far more crowded than plantations.

While not denying that *some* pests are likely to have become worse because of monocultures, I do not agree with Gibson and Jones when they say " . . . the most pessimistic forecasts of traditional foresters on the dangers arising from forest monocultures have been fully vindicated". The views of de Gryse (1955) — " . . . to ignore the notorious susceptibility of *P. radiata* to attack by insects and fungi, the extreme vulnerability of extensive monocultures in which it occurs . . . is tantamount to challenging all the laws of Nature . . . " have certainly not been vindicated! The success of *P. radiata* in New Zealand and in other countries where it is planted as an exotic is witness to this. De Gryse's assertion that *P. radiata* is notoriously susceptible to insects and fungi cannot, nor could at any time, be supported. In California *P. radiata* does not seem to have any more than its fair share of insects and fungi (Furniss and Carolin, 1977; Offord, 1964). It is also hard to reconcile Gibson and Jones' pronouncement that these pessimistic views have been vindicated with an earlier statement in their paper which points to the evident success of "tree farming" in most places where it has been tried.

DISCUSSION AND OPTIONS

When considering forest monocultures, I think a distinction must be made between those species grown in plantations within their native range and those grown as exotics. Most of the examples cited as problems by Gibson and Jones (1977) are the former and perhaps, just as or even more importantly, are species that are not normally found in pure (or near pure) stands. This is what we would expect as most of their examples are drawn from the tropics. Exotic plantations on the whole have a far better track record. This should perhaps be expected because in nearly all cases the trees are growing without their natural enemies; but just as important is the fact that most of them comprise tree species that are naturally found in pure stands, *P. radiata* for example. It could be argued that the native pests will eventually catch up with these exotic plantations. Yet while this is likely to

be true, it seems just as likely that exotic pests will damage indigenous forests. Pest outbreaks in artificially established stands are no more common than they are in natural forests. This is particularly true in the temperate zone.

What, then, are the alternatives to our exotic monocultures? Three immediately come to mind:

(1) Manage our native forests.
(2) Plant diverse exotic forests. I do not mean plant compartments of different species, this would merely be a collection of small monocultures. A diverse forest (in this context) is one where different species of trees are intermingled in one compartment.
(3) Establish plantations of our native species.

(1) Our native forests are complex communities which in general we do not understand all that well. There is good reason to believe that the most intractable conditions could be where natural forest has been altered, perhaps relatively little, leaving a dislocated climax system which has lost any natural stability that it may have had (Way, 1977). Native forests have their share of entomological problems anyway (Milligan, 1974).

(2) It has been argued on theoretical grounds, and backed up by observation, that diverse forests are not necessarily any more stable than simple systems so there would be little advantage in this alternative. The risk could be spread over several tree species but there is no basis for concluding that diversity *per se* would make pest outbreaks less likely. It is important to consider what types of risks we are dealing with. The worst possible case would be an insect capable of devastating the entire crop over a relatively short time period. While not denying that such an occurrence is a possibility, the likelihood of its happening must be considered remote. There are no reliably documented instances of this happening to forest trees. It is far more likely that a new pest would, at worst, kill overmature or unthrifty trees and/or cause loss of increment. If this is accepted as true, along with the proposition that diversity will not make pest outbreaks less likely, then decisions regarding the risks are a management problem rather than an entomological one. It is a question of how many baskets one wishes to put one's eggs into.

(3) Most of our native species have their share of pests. Some of them might be ameliorated in a plantation situation but it is equally true that some of them could be aggravated so there should be no inherent advantage in this alternative.

So it would seem that none of the alternatives available offer any real advantages over our present monocultures. By planting more species the risk would be spread but not necessarily reduced at all. I am not saying that our *P. radiata* monocultures are not at risk. They certainly are. I contend, however, that they are at *no more risk* than any of the alternatives would be. The risks that do exist can be greatly reduced by planting the right species on the right site and carefully tending it.

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