Letters to the Editor

COMPRESSION WOOD FORCE GENERATION

Sir,

In the recent paper by J. D. Boyd entitled "Compression Wood Force Generation and Functional Mechanics" (N.Z.J. For. Sci. 3 (2): 240-58) the author suggests that differentiating compression wood cells tend to extend their length when their microfibril angle is greater than about 40°. He arrives at this figure from a consideration of a theory of anisotropic shrinkage proposed by Barber and Meylan (1964).

Experimental work by Harris and Meylan (1965), however, showed that most compression wood, in *Pinus radiata* stems, at least, had a mean microfibril angle less than 40°. Boyd suggests that this was because the samples contained a mixture of both normal and compression wood cells. Thus, because the X-ray method used to measure microfibril angle averages a considerable number of cells the effect of including normal cells would be to substantially depress the mean value below that of the "true" compression wood cells. If by "true" compression wood cells Boyd means those having prominent striations this is most definitely not the case. The angle of the striations in these cells (which is easy to measure directly) does not differ significantly from the mean value measured by X-ray diffraction (Meylan, 1967).

There seems little doubt that compression wood force generation can and does occur at microfibril angles smaller than those called for on Boyd's hypothesis. This does not necessarily mean that his concept of lignification as a cause of stem straightening is wrong. If the hypothesis had been based on the experimental data (Harris and Meylan, 1965, Meylan, 1968, 1972) rather than on particular theoretical curves given by Barber and Meylan, then the longitudinal expansive tendency occurs in the range 30-40°. A more likely explanation of the discrepancy lies in the simplifying assumptions of the theory rather than in measurement errors. A more detailed anisotropic analysis taking into account chemical composition, thickness of the various layers in the cell wall and cell geometry, has recently been completed (I. D. Cave, 1973). This shows a very close agreement with the experimental curves. Very probably Boyd's arguments could then be applied with the change from positive to negative strain appearing at microfibril angles much closer to those observed in practice.

REFERENCES

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COMPRESSION WOOD FORCE GENERATION: A REJOINDER

Sir,

It is pleasing to have B. A. Meylan's comment, partly because he and his colleagues have made outstanding contributions to the analysis of anisotropic dimensional changes during shrinkage of wood, and because I believe major benefits would result from wider appreciation of the value of their work for other important applications. However, while referring to microfibril angles which are not unusually large, as marking a critical change from normal to compression wood in *Pinus radiata,* indirectly Meylan has drawn attention to the very wide variations of microfibril angle that have been reported within and between species; my initial response is on this.

Recently, a study was made of the main causes of microfibril angle variations in a number of species and throughout the wide spectrum of tissues which are designated or accepted as normal wood (Boyd, 1974a). It was shown that the most careful measurements of microfibril angle could appear to give anomalous results and lead to quite misleading conclusions unless many complementary data were provided. Desirable details include: species, tree age, form and vigour, height at which samples are taken and their position in the growth ring, cross-sectional shape of cells and their wall thickness to diameter ratios, and whether data are for radial or tangential faces of the cell wall. As with differentiation of tissues in other biological systems, many interactions are involved, and their relative levels change greatly with conditions. Consequently, a sharp demarcation line cannot be drawn between normal and abnormal or reaction wood ("compression" or "tension" wood). In my paper on compression wood, I sought to demonstrate main points qualitatively, while providing approximate quantitative values.

Relative to Meylan's comment—that my estimates, as based on Barber and Meylan's (1964) analysis, were less precise than *apparently* they might be—I was well aware of this situation. However, in the paper and through its references to others, I demonstrated that that basis for analyses gave results qualitatively in accordance with the facts. Also, because of lack of adequate data at that time, I felt that it was not practical to show that a more complex, theoretically-precise analysis would give results of greater *actual* precision. At the same time, I considered that demonstration of the general importance of prime factors in differentiation was urgent, and could encourage scientists to apply greater effort to obtain adequate data.

More critical comment on precision was made in another paper (Boyd, 1974b). In that paper, references were made to the several papers, excepting I. D. Cave's thesis (unseen), on relationships between data and method of analysis, that are listed in Meylan's comment. However, I added—"Nevertheless, precision and range of practicable predictions of shrinkage are limited by inadequate knowledge of the swelling and elastic properties of the matrix materials (Cave, 1972). In addition, it may be a great disadvantage that allowance for lignification, in existing (mathematical) models, is limited to the extent of its average contribution to the elastic properties of the matrix materials." In fact, intensity of lignification varies around the cell wall. It was shown, also, that details of the average value of maximum transverse separation of adjacent