leys to render them immune to hay fever is now under way and the serums prepared from the black walnut pollen are the ones most used. It is expected that by this means the spring type of the malady can be largely eliminated in those districts. A more direct method would be to remove the trees, or, better yet, to graft the tops over to English walnut, which rarely, if ever, causes hay fever. By this latter method the beautiful and stately trees along the highways and in the parks could be preserved, but it would doubtless be difficult to bring about unanimity of action.

The relation of the eastern black walnut to hay fever should now be determined since that species is closely related botanically to our western form. It may also be pointed out that perhaps the most significant result of our studies, which cover the region from the Rocky Mountains west, is the discovery that hay fever is here produced by an almost entirely different flora from that which causes it in the eastern states and in Europe, and that the exact species involved must be determined in each case before treatment for immunity is undertaken. Botanical surveys and clinical tests have been carried on by Dr. Selfridge and the writer in order to determine the most important species for each district and these will be continued as opportunity offers.

HARVEY MONROE HALL
DEPARTMENT OF BOTANY,
UNIVERSITY OF CALIFORNIA

THE CANONS OF COMPARATIVE ANATOMY

IN a recent number of this journal[1] Professor E. C. Jeffrey uses "an article on the vessels of *Gnetum* in the January number of the *Botanical Gazette*" as a "flattering testimonial" to the soundness of what he has called the canons of comparative anatomy and at the same time (to modify his pun) as an illustration of poor marksmanship in the use of those canons. As the author of that article and as a firm supporter of those canons, I am glad to offer my work as a testimonial to their soundness and to their effectiveness in anatomical offensives. But, according to Professor Jeffrey, my marksmanship was defective because I stated—and in so doing showed "surprising ignorance"— that the vessels of *Gnetum* are different from those of angiosperms. Aside from the fact that this statement does not involve the use of the canons at all, the whole theme of the article was that the *same* type of vessel has been evolved in *Gnetales* and angiosperms in entirely different ways. On page 90 for example I wrote:

> The possession of vessels by the two groups . . . is to be used as a remarkable illustration of development by different plants of the *same* highly specialized structure.

Again on page 89 after speaking of the perforation of the Gnetalean vessel I said:

> We have also seen that the similar single large perforation of the angiosperm vessel, etc.

Professor Jeffrey seems to have misunderstood what was in my mind because of my statement that the vessel of *Gnetum* is like the *highest* angiosperm type except that as a rule it exhibits a narrow border. Yet every anatomist will agree that this statement is absolutely correct because the *highest* type of angiospermic vessel *has no* border on its perforations. Of course every anatomist knows that the perforations of many angiospermic vessels do show a border as do those of *Gnetum*, but these are not of the highest type.

It appears, therefore that our modern scientific promulgator of canons is in certain respects remarkably like his ecclesiastical predecessors.

W. P. THOMPSON

[1] SCIENCE, N. S., Vol. XLVII, No. 1214.

ALBINO TURKEY BUZZARDS

IN a recent issue of SCIENCE[1] there appeared an interesting note on the supposed occurrence of albino turkey buzzards (*Cathartes aura aura*) in Mexico, to which Mr. E. W. Nelson has called the writer's attention. This was based on the account of white "Carrion Crows" given by Captain William Dampier in his "First Voyage to the Bay of Campeachy." That Dampier mentions these white birds as of more or less common occurrence in that locality at once raises a doubt of their identification as turkey buzzards; and this

[1] Gudger, SCIENCE, N. S., Vol. XLVII., No. 1213, March 29, 1918, pp. 315–316.

doubt is strengthened almost to a certainty by recollection of the fact that the king vulture, which is well known in that region, is of about the same size and general habits as the turkey buzzard, and in plumage almost wholly white or whitish; as Dampier expresses it, "their Feathers looked as if they were sullied." Furthermore, he states that the inhabitants of Campeche called them "King-Carrion Crows." It is evident, therfore, that we must identify Dampier's white "Carrion Crows" as king vultures (*Gypagus papa*).

It might be well, moreover, in this connection, to mention that albinos of the turkey buzzard, or, as it is more properly called, the turkey vulture (*Cathartes aura*), are by no means extraordinary, though of course not common. The present writer has, during the course of several years, examined a number of specimens; and among recorded instances we might mention that of Nauman in Florida[2] and Gundlach in Cuba.[3]

HARRY C. OBERHOLSER
BIOLOGICAL SURVEY,
WASHINGTON, D. C.

SCIENTIFIC BOOKS

The Early Mesozoic Floras of New Zealand: By E. A. ARBER. New Zealand Geological Survey, Paleontological Bulletin No. 6. 1917.

Fossil plants have been known from New Zealand for over half a century, but there has never been a comprehensive account of them published, and the wide variations in the opinions of the local geologists and paleontologists regarding the ages of the important stratigraphic units has made it impossible for students elsewhere to reach any intelligent understanding of the situation. Such an understanding is especially important in the case of so interesting and strategic a region, and its former relations to Gondwana Land, Antarctica and Australia have long been controverted questions.

The present comprehensive account of the older Mesozoic floras is therefore of great value to paleogeographers and to students of geographical distribution. The more important localities from which the fossil plants are described are the Rhætic of Mount Potts and Clent Hills in Canterbury and the Hokonui Hills in Southland; Owaka Creek in Otago is doubtfully referred to the Rhætic; Mokoia and Metaura Falls in Southland are referred to the lower Jurassic; Malvern Hills in Canterbury is doubtfully referred to the lower Jurassic; Waikawa in Southland to the middle Jurassic; and Waikato Heads in Auckland to the Neocomian.

The disputed question of the occurrence of *Glossopteris* is definitely answered in the negative and it is shown that there was considerable specific variation between the Rhætic flora of New Zealand and that of Australia, India and South Africa. The Jurassic floras appear to show less specific differences when compared with other areas. The author concludes that New Zealand was surely united with Australia during Rhætic and Jurassic times, but he objects strongly to using the term Gondwana Land for anything post-Paleozoic, although it is obvious that the existence of Gondwana Land as a geographical region did not cease with the close of the Paleozoic. Many geologists have also reached the conclusion that the evidence for the lower Permian age of the glacial period is sufficiently good to warrant the dropping of the term Permo-Carboniferous for it, although doubtless this practise will survive indefinitely in more conservative countries like Great Britain.

It would seem to the reviewer that it would have been preferable to use Mesozoic instead of Mesophytic for the floras discussed, since the latter term has a well-understood ecological significance. The perpetuation of the use of *Sphenopteris* for post-Paleozoic fern fragments is also to be deprecated, and it is questionable if clearness of understanding is facilitated by substituting *Tæniopteris* for *Oleandra* and *Macrotæniopteris*. Of great interest is the discovery of two forms of dicotyledonous leaves in beds referred to the Neocomian. These are described by Professor

[2] *American Naturalist*, IV., August, 1876, p. 376.
[3] *Auk*, VIII., April, 1891, p. 190.