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Comments and Reply on 'Axes of elongation of petrified stumps in growth position as possible indicators of paleosouth, Alaska Peninsula' : COMMENT

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FORUM

Comments and Reply on 'Axes of elongation of petrified stumps in growth position as possible indicators of paleosouth, Alaska Peninsula'

COMMENT

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Smirnoff and Connelly (1980) were appropriately tentative in determination of direction from measurements of eccentric growth in trees of Tertiary age. Their work, however, appears excessively speculative, and they neither elaborated on possible alternative causes mentioned nor dicussed many other possible mechanisms of eccentricity. In our opinion, much further study is necessary because eccentric tree growth has far too many causes to use it for reliable paleodirection determination without considerably more data. For example, information would be expected on fossilization, paleoslope, species, sample height above root flare, cellular character, reaction wood, general tree morphology, and so on.

Comprehensive reviews of the literature (Alestalo, 1971; Fritts, 1976, Shroder, 1980) demonstrate numerous known causes of eccentric ring growth; the fossil examples from the Alaska Peninsula should be thoroughly compared to these accepted causes before use of trees as indicators of paleosouth is promulgated. Perhaps Wright's limited and unpublished work on eccentric California redwood stumps, which Smirnoff and Connelly used as foundation, will eventually prove valid for certain species in special circumstances, but this work should be published and critically evaluated first. It is altogether possible that such eccentric growth occurs at the edge of clear cuts or stream channels where local competitor trees have been removed. Conversely, of thousands of trees sampled and precisely measured by dendrochronologists all over the world for more than 75 yr, this supposed sun-caused eccentricity has not proved significant. Giddings (1941, p. 51) sampled more than 3,000 trees in Alaska and noted that circuit uniformity in trees is good there, especially in those growing at timberline, along river margins, and on south-facing slopes. In addition, we have thoroughly analyzed somewhat more than 500 eccentric trees from high and middle latitudes, from sea level to above 3,350 m, and on flats and steep slopes, and we have noted only great diversity and multiple causes of irregular growth.

Numerous other possible causes of eccentric growth therefore could have affected the trees noted by Smirnoff and Connelly; about half of these causes are known to be significant elsewhere, whereas the remainder are speculations presumably as valid as theirs.

1. Rings are distorted in trees growing on flats in the muskegs of Alaska (Giddings, 1941, p. 51). Discussion of root substrate of the fossil examples therefore is needed.

2. Strong prevailing winds desiccate cells and may suppress growth on the windward side of trees (Polunin, 1960, p. 291–292). The Alaska examples therefore could have been suppressed by winds from the north if about 45° of subsequent plate rotation is assumed, or from the northeast (the present prevailing wind direction in winter) if no rotation is assumed. Alternatively, katabatic air drainage from the nearby volcanic highlands is also a possibility. We need much further paleogeographic information on this point.

3. Root disruption may suppress growth on the affected side of the tree (LaMarche and Wallace, 1972, p. 2667). Such preferentially oriented effects in the Alaska examples would suggest a local environmental factor controlled by geomorphic process or topography. Perhaps snowbanks, permafrost, water saturation, or variable soil character might be hypothesized; certainly many other such controlling factors could be possible.

4. Growth suppression could result from nearby volcanic activity (Smiley, 1958). The eventual burial of the upright trunks in the volcanic agglomerate shows the close proximity of the trees to local volcanic centers.

5. Smirnoff and Connelly probably are correct to doubt a cause of eccentricity from agents on slopes because the trees seem to have been rooted in flats and therefore would not be susceptible to snow creep or landslip movement. Nevertheless, considerably more paleogeomorphic information must be provided before most dendrogeomorphologists would be convinced.

6. If indeed the sun can increase directional tree growth, as Smirnoff and Connelly (1980) maintained, then other speculative alternative points can be made. Geiger (1965, p. 420–421) noted that Northern Hemisphere afternoon solar energy is most effective and produces the most warmth on slopes facing southwest. In the long afternoons of arctic summer, this effect would be even more pronounced, so that one might expect greater tree growth in that direction.

We therefore suggest that far too many unreliable assumptions and untested speculations are implicit in the use of axes of elongation in tree stumps as indicators of paleosouth. Dendrochronologists know that trees can be notoriously unreliable indicators of paleoenvironmental information unless the basic principles of tree growth, especially those of limiting factors and ecological amplitude (Fritts, 1976), are rigorously followed. Theories of rotation of the Alaska Peninsula since Tertiary time, based on Smirnoff and Connelly's technique, therefore seem most unreliable.

COMMENT

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Smirnoff and Connelly (1980) derived the position of "paleosouth" from the "distinct preferential growth in the southwest direction" of Tertiary tree rings in Alaska. They suggested that paleosouth was located to S38° ± 14°W and S44° ± 15°W. Krames (1952, 1956) also studied Tertiary tree stumps, but, unlike Smirnoff and Connelly, he interpreted the axis of elongation to indicate not paleosouth but paleonorth (incidentally, corresponding to Holocene north). This supposition was supported by measurements on 200 Holocene trees.

Smirnoff and Connelly (1980) mentioned that there are several possible explanations for preferential growth of tree rings:

direction of prevailing winds, downslope direction, and direction of prevailing sunlight. They considered the third explanation as "most plausible," as did Krames.

We must ask, Is the axis of elongation directed to south or to north? Krames measured many more recent trees than Smirnoff and Connelly. Moreover, he gave a reasonable explanation for north preference: trees tend to be *inclined* to the equatorial side and consequently the stem must grow thicker on the opposite side. Thus, his conclusion concerning the "compass problem" seems to be a little more sound than that of Smirnoff and Connelly.

The second serious objection to the studies of both Smirnoff and Connelly and Krames concerns the very small number of fossil stumps measured: 28 (at two sites) in Alaska and 24 in Germany. In my opinion, the evidence presented in both cases is too incomplete for any reliable conclusions to be drawn. Key words like "paleosouth" (in Alaska) and "geological compass" (in Germany) seem somewhat hasty. I agree more with Smirnoff and Connelly's characterization, "a possible technique for determining paleosouth" (provided it is south and not north!).

The papers of Krames (1952, 1956) are almost inaccessible in the United States. However, the problem of a geological compass is mentioned briefly in my book *Climates of the Past* (Schwarzbach, 1963, p. 80, 248; see also Schwarzbach, 1974).

REPLY

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Shroder and Sewell describe many variables that effect eccentricity of tree rings. We discussed several of these variables, but others were not considered, so we appreciate their expert criticism of our speculative hypothesis.

Schwarzbach cited work by Krames (1952, 1956) describing Holocene and Tertiary tree stumps studied in Germany that displayed eccentric tree-ring growth with the axis of elongation to the north. We will venture farther out on a limb and offer a possible explanation for this apparent discrepancy. Our hypothesis that trees growing on flat ground should have preferential tree-ring growth in the direction of prevailing sunlight (assuming that all of the masking variables discussed by Shroder and Sewell are eliminated) is based on the phototropic response of trees that tends to cause them to lean slightly toward prevailing sunlight. Creber (1975) noted that gravity has a marked asymmetric effect on branches and leaning trunks that leads to what is known as *reaction wood.* ["Wood that formed under transverse or oblique gravitational stimulus is known as *reaction wood*" (Creber, 1975, p. 75).] "Angiosperms [flowering plants] and gymnosperms [for example, conifers] differ as to the position of the reaction wood in the affected parts. In angiosperms . . . it develops on the upper sides of branches and leaning trunks. Conversely, the . . . wood of gymnosperms forms on the lower sides of branches and leaning trunks" (Creber, 1975, p. 82). On the basis of this theory of reaction wood, we would modify our hypothesis to say that *gymnosperms* growing on flat ground should have preferential tree-ring growth toward the direction of prevailing sunlight, and that *angiosperm* trees should have eccentric tree-ring growth away from the direction of prevailing sunlight. The Krames (1952, 1956) papers are not readily available to us, but we suggest that his German measurements (showing preferential growth to the north) were made on angiosperm trees, whereas our Alaska measurements and A. Wright's (unpub.) California measurements (showing eccentric growth to the south) were made on gymnosperms.

G. T. Creber recently sent us a rather obscure publication by Kossovich (1935) describing eccentric tree-ring growth with the longest radius to the south in Holocene pine trees growing in the Northern Hemisphere. Kossovich noted that the most consistent preferential southern growth comes from the first 30 annual rings that develop when the trees are young and not yet covered with a thick bark. Similarly, most of the measurements described in our article came from the cores of the stumps, because the eccentric growth was much better defined there.

Since publication of our article, A. Wright visited the Unga Island locality and measured axes of elongation of many more trees over a much larger area than we were able to during our brief visit. Wright reported significantly more dispersion in her measurements than in ours; perhaps she did not limit her measurements to the cores of stumps where eccentric growth is best defined. In addition, she collected oriented siltstone samples from the formation encasing the petrified trees for paleomagnetic analyses. Results of the paleomagnetic work are not yet available but should offer an independent test of our hypothesis.

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Comment and Reply on 'Late Wisconsin and Holocene tectonic stability of the United States mid-Atlantic coastal region'

COMMENT

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Blackwelder (1980) proposed that the lack of deformed shorelines of age 9,000 to 12,000 yr along the southeastern United States coastline implied tectonic stability during that time period. He stated that during more recent times the observed deformed shorelines and differential submergence rates might be due to glacial isostatic processes, but that the lack of deformation of the 9,000 to 12,000 yr B.P. shorelines implied glacial isostatic stability as well. I believe, however, that glacial isostasy can explain the Holocene deformed shorelines as well as the earlier undeformed shorelines. More important, the undeformed shorelines do not imply stability of the coastline.

Figure 1 is a compilation of predicted sea-level curves for the eastern United States from Clark and others (1978). In that paper it was clear that the total amount of predicted submergence was too great when compared to observations of Holocene sea levels. Blackwelder's claim that eustatic sea-level rise was too large in the model used in that paper and subsequent modeling efforts is therefore well taken. However, relevant to Blackwelder's claim of coastal stability is the *form* of the predicted curves in Figure 1 and, in particular, the fact that all of the curves from the region considered by Blackwelder converge at about 11,000 yr B.P. There is a physical explanation for this convergence. If flow within the mantle occurs at depth, then regions near the edge of an ice sheet, but not necessarily under the ice sheet, will initially rise after ice retreat, but at more recent times they will subside (Cathles, 1975; Clark and others, 1978). As the distance from the ice sheet increases, the amount of uplift decreases, whereas the amount of subsidence increases (Clark, 1980). These predictions are confirmed by sea-level observations in Arctic Canada (Dyke, 1977) and the Maritime Provinces of Canada (Grant, 1980). The slight amount of predicted uplift shown in Figure 1 for 10,000 to 16,000 yr B.P. for New Jersey and Virginia is also a result of this process. The change from uplift to subsidence results in convergence of subsequent sea-level curves from different regions.

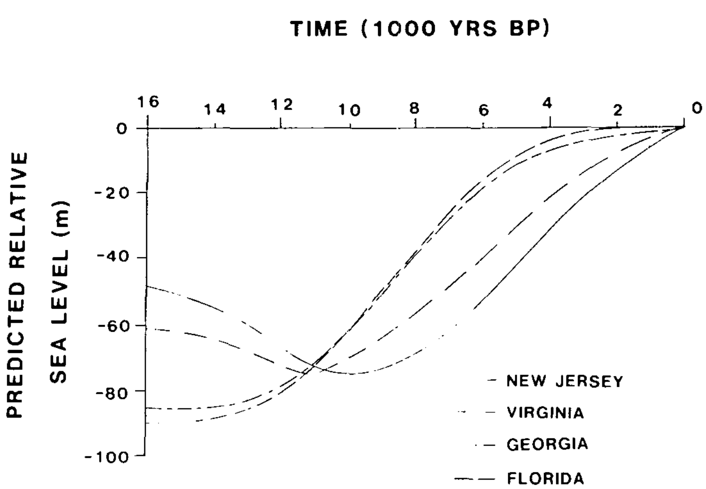

Figure 1. Predicted relative sea-level changes of the United States mid-Atlantic coastal region. Model assumed uniform mantle viscosity and total eustatic sea-level rise of 75 m. Predicted curves converge at about 11,000 yr B.P.

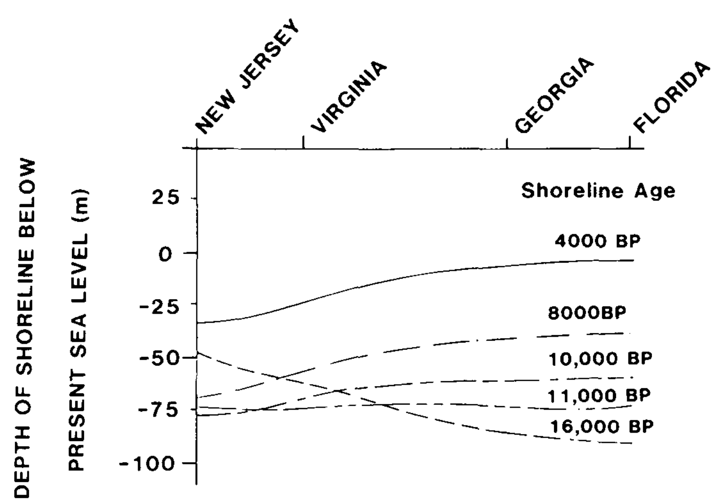

Figure 2. Predicted deformation of shorelines caused by glacial isostasy. Shoreline for 11,000 yr B.P. is undeformed, but this does not imply stability. Older shorelines are predicted to tilt toward south, whereas younger shorelines tilt toward north.

Figure 2 is a redrawing of Figure 1, showing the predicted deformation of shorelines of different ages. It is of interest that predicted shorelines older than about 12,000 yr tilt toward the south, whereas those younger than about 10,000 yr tilt toward the north. The 11,000 yr B.P. shoreline is undeformed, in general agreement with Blackwelder's observations. The depths of the actual shorelines will differ from the predictions of Figure 2 because of errors in the assumed rise of eustatic sea level. The amount of tilt is a function of the assumed glacial history of the North American ice sheets and may also be in error. However, the reversal in tilt direction is relatively insensitive to these assumptions. As reliable dates on 14,000 to 17,000 yr B.P. sea levels become available, the predicted tilt reversal can be tested.

Qualitatively, at least, glacial isostasy can explain both the greater rate of submergence in the north than in the south during Holocene time, as mentioned by Blackwelder, and the apparent lack of deformation at slightly earlier times. Blackwelder's latest results further substantiate the glacial isostatic model as the dominant cause of variations in North American sea levels. An undeformed 11,000 yr B.P. shoreline is therefore not an indication of stability but rather a fortuitous occurrence embedded in a very dynamic process.

REPLY

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Clark suggests that late Wisconsin and early Holocene undeformed shorelines alone do not indicate stability. He contends that these earlier shorelines must be examined in conjunction with subsequently formed shorelines. I have considered such subsequent shorelines (Blackwelder, 1980), and I have indicated that the observed present-day rates and trends of deformation, as determined from precise leveling and tidal-gauge measurements, could not be extrapolated back to the early Holocene. In addition, I determined that shorelines of similar age which formed over the time span of 12,000 to 9,000 yr B.P. lie at similar elevations throughout their extent; this contrasts with Clark's model, in which only 11,000 yr B.P. shorelines would be horizontal. From these observations and from the recognition that only relatively minor subsequent deformation has taken place, I concluded that there has been little vertical deformation of upper Wisconsin and Lower Holocene deposits in the area between New York City and South Carolina. I did not state that glacial isostasy could not explain the distribution of past sea-level data, nor did I use the term "glacial isostatic stability." In fact, I noted that tilting of shorelines younger than 6,000 yr was possibly the result of "geoidal adjustment related to deglaciation of the Northern Hemisphere" (Blackwelder, 1980, p. 536–537) and that there was a geographic onshore-offshore factor that must be considered in interpreting deformational trends.

Figure 1 plots actual sea-level rise to show that this region has been much more stable than suggested by Clark's glacial isostatic model of predicted local sea-level rise on the Atlantic Continental Shelf. Used in my interpretation are selected samples that were the basis of published local sea-level curves and that include data from Blackwelder (1980), Newman and Munsart (1968), Ellison and Nichols (1976), Brooks and others (1979), Stuiver and Daddario (1963), and Scholl and others (1969). For the most part, the observed data plot within about 6 m of the values shown by the dotted line in Figure 1 that indicates general sea-level rise in this part of the United States Atlantic coastal region. The similar elevation of similar-age samples shows that the coastal region has been relatively stable for the past 12,000 yr; this conclusion contrasts with that shown by Clark's predicted curves. Therefore, these largely undeformed shorelines show the tectonic stability of this region relative to other areas. The recognition of the relative stability of a region does not imply that the area is unaffected by dynamic processes, only that the effects of these processes have a limit, as graphically depicted in Figure 1. On the basis of Clark's model, the mid-Atlantic coastal region would be considered a tectonically active area which has had more than 30 m of northeastward shoreline tilting in the past few thousand years. However, as Clark recognizes, corrections of the magnitude of sea-level rise in the glacial isostatic model would result in more agreement between his predicted curves and the observed data.

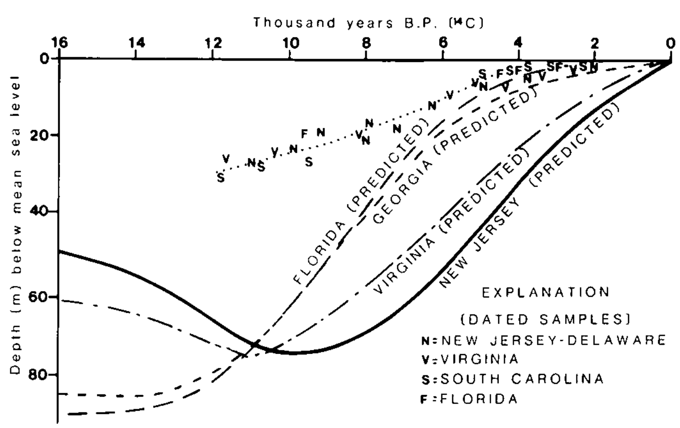

Figure 1. Comparison of predicted local sea-level rise for parts of United States Atlantic coastal region (from Clark's Comment above) and observed sea-level rise (dotted line) based on data from published local sea-level curves.

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