Reply[1]

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The comments by Dorman are interesting and informative, and they identify the crux of the problem with ocean rainfall estimates: after several decades of efforts, we still do not have a widely accepted climatological "standard" for oceanic precipitation. He seems to imply, however, that his recent work should be accepted as the standard. Furthermore, he fails to mention Elliott's and my criticism (Elliott and Reed, 1979) of the methods of Dorman and Bourke (1978). Hence, before addressing the specific points raised by Dorman, I will discuss briefly the differences between his and my methods and results in order to orient the reader.

Our criticism (Elliott and Reed, 1979) of the methods of Dorman and Bourke (1978) was based on their adjustment of the amounts from Tucker's (1961) assessments by a multiplier determined from measured rainfall at land stations. We argued that this was very apt to overestimate oceanic rainfall because of considerable evidence that the relative frequencies of light, moderate and heavy rainfall are not the same over the ocean as over land, which their method implicitly assumed. In addition, we were skeptical of their regression of these multipliers against air temperature, which could be interpreted as indicating considerable scatter and factors of approximately unity in midlatitudes and three times Tucker's assessments in the tropics [as used by Reed and Elliott (1979)]. Our maps are somewhat different than Dorman and Bourke's (1979); their values in the western tropics are greater than ours, and their amounts in the subtropics near 30°N are almost twice ours. Their annual precipitation volume for the North Pacific is 116×10^3 km^3 compared to our value of 87×10^3 km^3. Much of Dorman's criticism stems from my failure to adopt his "air-temperature correction"; this did not result from oversight on my part, however, but from serious doubts about its validity.

Dorman noted the shipboard measurements of precipitation analyzed by Reed and Elliott (1977), which gave good agreement with Tucker's method from 40 to 60°N but were about three times greater than Tucker's assessments in the tropics southeast of Hawaii. Since that time, limited data have become available for the subtropics; 110 days of comparisons (March 1977–January 1980), mainly south of 40 to 23°N in the eastern and central North Pacific, gave a gage catch of 18 cm compared to 19 cm from the weather codes by Tucker's method. This suggests that Tucker's assessments are adequate in the subtropics and are only deficient in the tropics, which was the rationale used (Reed, 1979; Reed and Elliott, 1979). Furthermore, a fairly sharp gradient in the intensity of rainfall between the tropics and subtropics is not implausible when one

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considers the zonal nature of the tradewinds and the resulting organized convection in the intertropical convergence zone (Kilonsky and Ramage, 1976).

Specifically, Dorman feels that my use of frequency data does not distinguish between different types of precipitation and would "cause serious errors." He is, of course, correct about the first point, but the standard error of estimate in *amount* is only ~10%, whereas the standard error for his estimates (Dorman and Bourke, 1979) using Tucker's method in the data-sparse tropics is appreciably greater than that. There seems to be some confusion in Dorman's points about "fair-weather bias." First, this bias was approximately eliminated from the ship-of-opportunity data in the *Marine Climatic Atlases* by careful intercomparisons and tests by analysts at NOAA's National Climatic Center; thus, the frequency data that I took directly from the atlases are not biased, but the original ship-of-opportunity data are. This poses the difficult question of how to eliminate these effects from the various codes (rather than a single frequency) used to derive estimates by Tucker's method. Second, I did not analyze the magnitude of the fair-weather bias on ship-of-opportunity data, but Quayle's (1974) results suggest that it may reduce precipitation frequencies by roughly 30% or more, whereas Dorman suggests that it may be an insignificant problem. A few other possible problems with the data base used in Tucker's method were noted by Reed (1979).

Dorman cites the work of other researchers that support his results, but Reed and Elliott (1979) also examined other work, including a number of gage measurements aboard ship, which support their assessments. Hence we are left with an incomplete resolution of the differences between these two recent estimates of oceanic precipitation. Additional shipboard measurements, which I am conducting, may prove useful, but I urge Dr. Dorman to attempt a complete recalibration of Tucker's parameters for different regions with the data base that he has.

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