Reply

H. M. JOHNSON

National Environmental Satellite Service, Washington, DC 20233

DONALD R. COCHRAN

NESS, Satellite Field Services Station, Honolulu, HI 96819

19 May 1978 and 13 June 1978

Two main purposes of our short summary (Cochran and Johnson, 1976) of a rapidly changing satellite-observed atmospheric system over the North Pacific Ocean were 1) to underscore the fact that a large part of this data-sparse area was then (as now) frequently and well observed by an excellent geostationary satellite and 2) to describe and share one of the many interesting cloud system developments observed soon after the Honolulu Satellite Field Services Station opened.

A third purpose, a demonstration of the power of this analysis tool, is assisted by Professor F. Sanders' four paragraphs of critical comments, for this case provides a good example of why (especially in this age of high-quality satellite observation) a synoptic meteorological analyst *should* always relate the satellite data to the existing conventional data, and do so in detail and with care and skill if he is to understand, so far as may be possible, what has been going on in the atmosphere. This is a concept that we have long stressed and applied to the point where one of us (Johnson) has been repeatedly criticized for the very substantial amount of conventional data regularly used. An essential aspect of this combined data use is to work *primarily* with on-time (or near on-time) conventional data. This we have also done. From this point of view, our combined data are in good agreement with what we have said and, in fact, permit us to say substantially more than we did.

Two of our principal points here are as follows: 1) the vortical cloud pattern that became well organized in about 6 h represented a real vortex, a cold low, with "maximum development" taken to be "in the middle troposphere" (as indicated in the IR pictures by the cold middle and high clouds); and 2) the circulation extended downward (weakening downward) at least to the 850 mb level (as indicated by the low cloud displacements). We did not say *where* the center of a surface low (if present) was or probably was. In general, such positions were certainly not pinpointed nor even approximately well located. We were, however, a good bit more constrained by data and reasonable continuity than was Professor Sanders, contending as he was with large data gaps. Applying the principle that such centers, in the case of early stages of development such as those here, are *usually* a moderate distance approximately east of the related upper center (often near the western edge of or just under the solid overcast that is normally east of the upper center), a fair first approximation position might be about 1° (latitude) approximately east of the upper center and under the cloud maximum there. This position is not well to the south of the upper center, as Sanders states we have it; nor is it over 2° east and under a major cloud minimum as Sanders shows it in Fig. 2.

For the purposes of this analysis, Professor Sanders' Figs. 1 and 2 are of very little help. Fig. 1 is off time by about 4 h, presents a large data gap at and near the 5° square of maximum interest, lacks the ship near 33°N, 162°W with a 30 kt south wind and rain, and has, generally, a rather faulty analysis in the area of interest. Fig. 2 is even worse, being off time by about 12 h, including a large gap in the crucial area, showing surface data in wrong relation to the 1245 GMT cloud position, and all this with respect to a moving, fast changing, relatively small system—thus little can be learned from it. We not only agree with Professor Sanders' fourth paragraph but feel that he should have honored the concept proclaimed there much more scrupulously than he did—particularly since he is, we believe, one of the best synoptic meteorologists in the United States today.

While most of our additional data, and consequent insights, were derived from satellite movie loop imagery and motions for this time period, some additional information was provided by the Honolulu operational surface charts. The 1200 GMT chart added little because of a large data gap at the area of primary interest, but the 0600 and 1800 GMT charts did help substantially, being on time or nearly so and including data in the area of interest. The 0600 GMT ship near 32½°N,157½°W with a 10 kt northwest wind, low overcast, showers in past and present weather, 1016.0 mb pressure and falling tendency was under the major cloud band, near the

inflection point. The 1800 GMT ship near 33½°N,157½°W with a 20 kt northeast wind, thunderstorm and multilayer higher clouds, fog, 1018.0 mb pressure and zero tendency was under the hook cloud maximum and can be interpreted to be west of the surface front and near and northwest of a surface center (as on the operational chart).

Additionally, it may be noted that 1) the "development" was in the atmosphere, not "in the satellite imagery"; 2) our case is substantially different from Johnston's; 3) "not able" means "did not" (we are always happy to share our data); 4) "it seems" appears to mean "I suggest"; 5) a frontal wave moves along a frontal surface (or baroclinic zone), not "along the major initial cloud band"; 6) "above the lower troposphere" is one of our major points, but more weakly stated here; 7) the upper center is not "well to the north of the surface center" but more likely a little west (or west-southwest) of this assumed and poorly located center; 8) "growth of the circulation" (or widening of it) may be reasonable but is not required by the data of Sanders' Figs. 1 and 2, and thus is conjectural; 9) "no deepening" can only be merely a guess; 10) pressure gradients and surface winds remain fairly strong at several areas, but our ship data include light winds in the area of interest; and 11) we did not intend "to imply otherwise" (concerning light winds or strong winds or changes in strength).

The importance of care, and skill, in this type of combined-data synoptic analysis clearly must be further emphasized.