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Technical Comments

Microwave Sounding Units and Global Warming

In their research article "Precise monitoring of global temperature trends from satellites," R. W. Spencer and J. R. Christy assert (*1*) that satellite microwave sounding units (MSUs) exhibit superb stability and may be used to monitor global warming in a way that is not possible by other means.

During the first 10 years of MSU measurements, global warming models predicted surface air temperature increases of approximately 0.2 K (*2*). The average temperature of the troposphere was expected to increase by about the same amount. The analysis by Spencer and Christy shows that globally averaged MSU brightness temperatures exhibit fluctuations of approximately 0.4 K over months and years. If one equates brightness temperature with average tropospheric air temperature, as the authors do, it becomes difficult to discern such a small "signal" embedded in so much "noise." That is why the authors can state that "[t]here is no obvious long-term trend . . ." in their data. They do not state that the expected global warming signal is not present in the MSU data, just that it cannot be detected. The main value of their analysis is to point out that short-term fluctuations are so large that it may be difficult for any investigator to evaluate the magnitude of long-term trends without more than one decade of data.

The data in Spencer and Christy's figure 5A (*1*) exhibits a slope of +0.06 K per decade (*3*) and a standard error of 0.07 K per decade (our estimate). [Spencer finds smaller slopes for an upgraded MSU data set (*3*)]. Because of the sinusoidal components apparent in the MSU time series, the slope uncertainty could actually be much greater than 0.07 K per decade. Statistically speaking, the 10-year trend is almost as consistent with the "expected warming" hypothesis as it is with the "no warming" hypothesis.

Five MSU instruments contributed to the data set used by Spencer and Christy. Another recent publication by these authors (*4*) describes in detail how the respective data sets were combined. Instrument intercalibration is a crucial issue for detecting trends, and it is important to know the sensitivity of trend solutions to alternative intercalibration approaches.

The reader who is looking for evidence of the expected 0.2 K global warming signal in the MSU data should be clearly informed that global warming models predict secondary effects, in addition to tropospheric warming, that can influence long-term brightness temperature trends. These include increasing water vapor, changing cloud cover and liquid content, and changing soil moisture (which, in turn, changes land emissivity). Most of these small effects tend to decrease the magnitude of any increase in the brightness temperature of MSU channel 2 resulting from global warming. Spencer and Christy's reference 8, stating that "[o]ther, smaller signals are also present in the measurements. . . . and have been determined to be small for MSU channel 2 (0.01°C or less)," should have been expanded to reveal their modeling assumptions. Such information may be critical to estimates of second-order contributions (*4*).

In response to the research article by Spencer and Christy, we have modified existing computer programs for calculating microwave observables and have studied several potential second-order effects, two of which are *not* noted in Spencer and Christy's reference 8. Specifically, we have evaluated the impact of MSU channel 2 frequency drifts and the effect of stratospheric cooling which others have suggested will occur with tropospheric warming (*5*). All effects that we have evaluated are small (0.01 to 0.05 K), but these and other potentially important effects merit further study.

Our tentative conclusion is that remote sensing using satellite microwave radiometers can in fact provide a means for monitoring troposphere-averaged air temperature, but for this to be successful more than one decade of data will be needed to overcome the apparent inherent variability of global average air temperature. The provocative data set reported by Spencer and Christy should be subjected to careful and rigorous review before it is interpreted as evidence of the presence or absence of global warming.

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Response: We agree with the major theme of the comment by Gary and Keihm that, because of the importance of monitoring global temperature by satellite microwave methods, all aspects of satellite data processing and interpretation should be critically evaluated.

It is true that, in a statistical sense, the larger the year-to-year variability in globally averaged tropospheric temperatures, the more uncertain is any calculated trend over 10 years. However, Gary and Keihm's statement that the slope uncertainty "could actually be much greater" than the standard error they have calculated for our 10 years of data (0.07°C) seems to have no statistical basis. Even if we had observed a large upward trend during our 10-year period of analysis, the last 100 years of thermometer data suggest that a 10-year trend is probably not useful for predicting what might happen in the coming decades.

Gary and Keihm also address the importance of our satellite intercalibration procedure. As our original paper pointed out, overlaps between successive satellites resulted in agreement of 0.01°C per month for all five periods. Such agreement improves with the length of the overlap. On the basis of small levels of uncertainty of the intercalibrations, we estimate a cumulative uncertainty of 0.02°C for the 10-year period. The lack of any trend in the difference between anomalies from different MSUs is itself evidence against any significant drift in frequency of the instrument channels. As discussed in our more recent paper (*1*), weather balloon comparisons over 5 years have shown no change in the NOAA-6 MSU response to the statistical noise level of those comparisons (0.01°C). The differences in response of about 0.5°C between instruments is irrelevant to the study, since we were concerned only with temperature anomalies about the mean for a given instrument.

The small effect of the MSU weighting function being partly in the stratosphere (which is predicted to cool if the troposphere warms) will need to be taken into account if future MSU channel 2 brightness temperature trends are to be accurately interpreted as thermometric temperature trends of the troposphere only.

Gary and Keihm's final point regarding the small effects due to other geophysical signals (water vapor, cloudiness, and soil moisture variations) in the data has also been addressed in detail in our recent paper (*1*).

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Lipid Flow in Locomoting Cells

J. Lee *et al.* conclude (*1*) that the "retrograde lipid flow (RLF) hypothesis is no longer tenable as a general model for cell locomotion." In their experiments, they marked a line in the plasma membrane lipids of a moving polymorphonuclear leukocyte (PMN) that is parallel to the advancing edge of the cell. They then observed how this line moved with respect to the advancing edge as the cell moved forward. The membrane flow hypothesis [reference (*2*), itself a refinement of the lipid flow scheme (*3*)] predicts it would move backward. In 9 out of 16 cases this is what they actually found. However, is the observed rate of rearward movement that which is predicted by my hypothesis? They state that the membrane flow hypothesis demands a rearward line migration that moves two times as fast as the leading edge advances—all measured with respect to the substratum [note 21 in (*1*)]. This is incorrect. In a commentary (*4*) on an earlier paper from this group (*5*), I explained that the membrane flow hypothesis predicts that a particle on the dorsal surface of a cell (or in this case, a line drawn in the cell surface) will migrate rearward with respect to the leading edge. How fast it should do so depends on a variety of factors, including how fast the cell is potentially moving and where on the cell surface the particle is. I say "potentially moving," because the advancing edge, in the process of extending, may or may not actually attach to the substratum. Whether it does or does not attach to the substratum makes no difference to the *mechanism* of the motor, but does affect the rate of locomotion. In other words, the cell may move forward if the front attaches, or "slippage" may occur if it does not. [An example of a cell in a purely slipping mode is one on the edge of a stationary colony of spread epithelial cells: the advancing edge can no longer advance and so slips, the slippage often being seen as ruffling of the advancing edge (*6*).] A particle just behind the leading lamella would be expected to remain stationary with respect to the substrate if no slippage occurred, and to move backward with respect to the substrate if the cell were slipping. In assuming a rearward line migration with respect to the substrate, Lee *et al.* assume their PMNs are slipping badly: given the rate at which they move on glass this seems improbable.

Lee *et al.* (*1*) draw the line in the cell's plasma membrane near the middle of the cell; the predicted rearward membrane flow there would be one-half that at the front (*7*) (assuming these cells are flat sheets, which surely they are not). Their marker line might therefore be expected to move rearward with respect to the leading edge at half the speed that the leading edge advances over the substrate. In their terminology, this would give an R factor of 0.5, not the 3 they state. The scatter observed in their data (in their figure 4) is such that one cannot distinguish between an R of 0 or 0.5

In figure 4 of the paper by Lee *et al.*, it is stated that two cells (1 and 15) have R values of 0 and about −0.6. Following their method of calculation, I find these figures should be about −20 and −40; if these experimental measurements are actually correct, they suggest that none of the models considered by Lee *et al.* can be valid.

In conclusion, the report by Lee *et al.* sheds little light on whether the membrane flow model (*2*) applies to PMNs or not.

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