The Iris Effect; its strange and disturbing history

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A pdf file of these slides is available upon request to rlindzen@mit.edu. Original papers can be found at the following URL: www-eaps.mit.edu/faculty/lindzen.htm

This talk focuses on what is often considered the most fundamental question in the field of climate change (namely, *what is the sensitivity of global mean temperature to global radiative forcing?*), and the response of the climate change community to this question. My work with M.-D. Chou and A.Y. Hou on the Iris Effect will be discussed as an explicit example.

This would appear to be an important and well defined question.

If the climate sensitivity is low, then presumably the problem of global warming is limited; if it is high, then urgency would appear justified.

Relatedly, if the sensitivity is low, the need for subsidiary studies of impacts may also be limited.

Since at least 1979, the common measure of sensitivity has been the change in global mean temperature that would be expected from a doubling of CO_2 once the climate system reaches equilibrium.

In 1979, the National Research Council's Charney Report offered a 'guesstimate' of about 1.5-4.5^{o}C. Note that there was no real basis for these bounds. The range could have been extended at both ends.

The latest IPCC WG 1 report claims that the range of model results continues to show the very same range.

The low end is *officially* reckoned to be tolerable, while the high end is judged to be ominous.

Last year, in the draft of the National Climate Plan, priority was given to reducing our uncertainty over climate sensitivity during the next 5 years.

A committee formed by the National Research Council and chaired by Thomas Graedel reviewed this draft.

The draft was initially criticized for including the reduction of uncertainty in our estimate of sensitivity as a priority. The draft and the final plan were both also criticized for giving inadequate priority to expanding the scope of impact studies.

In this talk, I will offer some reflections on this quantity which some of us seem so committed to keeping fuzzy. *These reflections, of necessity, will involve a tour (albeit very incomplete) of climate physics, itself.*

It should be said at the outset, that the concept of climate sensitivity is, indeed, simplistic, and has lead to a very naïve picture of climate wherein:

1) Climate can be summarized in a single index: global mean temperature;

2) Climate is forced by net radiative change associated with changes in greenhouse gases, solar ‘constant,’ etc.; and

3) Spatial structure of climate change follows from global mean temperature.

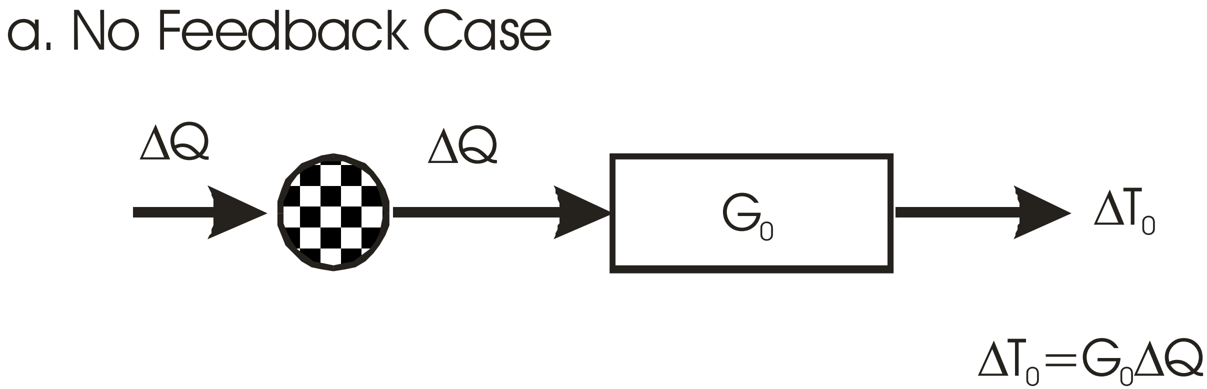
At best, such a picture might be applied to global forcing by changes in well mixed greenhouse gases, solar output, etc. It is almost certainly inappropriate to inhomogeneous forcing which acts to alter horizontal heat fluxes (Milankovitch forcing for example).

Climate sensitivity is generally defined as follows:

$$\text{Sensitivity} = \frac{\Delta T}{\Delta F}$$

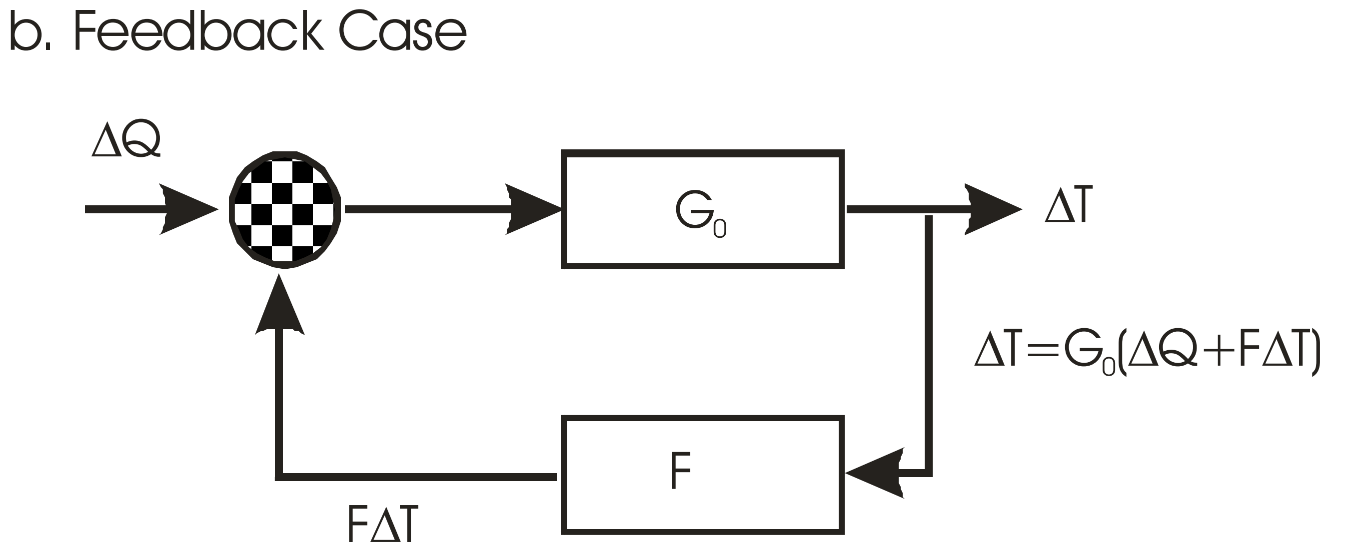
For a doubling of CO_2, ΔF is a bit under 4 watts/m^2, and if all other factors (humidity, clouds, ice and snow cover) can be held constant, the response is generally reckoned to be about 1^{o}C; ie, Sensitivity=0.25 ^{o}C/watt/m^2.

The higher sensitivities, characteristic of most climate models, arise from feedbacks. Unfortunately, the above formulation of sensitivity does not allow for the convenient isolation of the role of feedbacks. Electrical engineers have developed a more appropriate formulation.



As we have just noted, G_0 is generally reckoned to be about 1C for a doubling of CO_2.

If, however, changes in global mean temperature elicit changes in other factors influencing the global mean temperature, we have what are called climate feedbacks.



$$\Delta T = \frac{G_0 \Delta Q}{1 - F} \quad \text{or more generally,} \quad \Delta T = \frac{G_0 \Delta Q}{1 - \sum_i F_i}$$

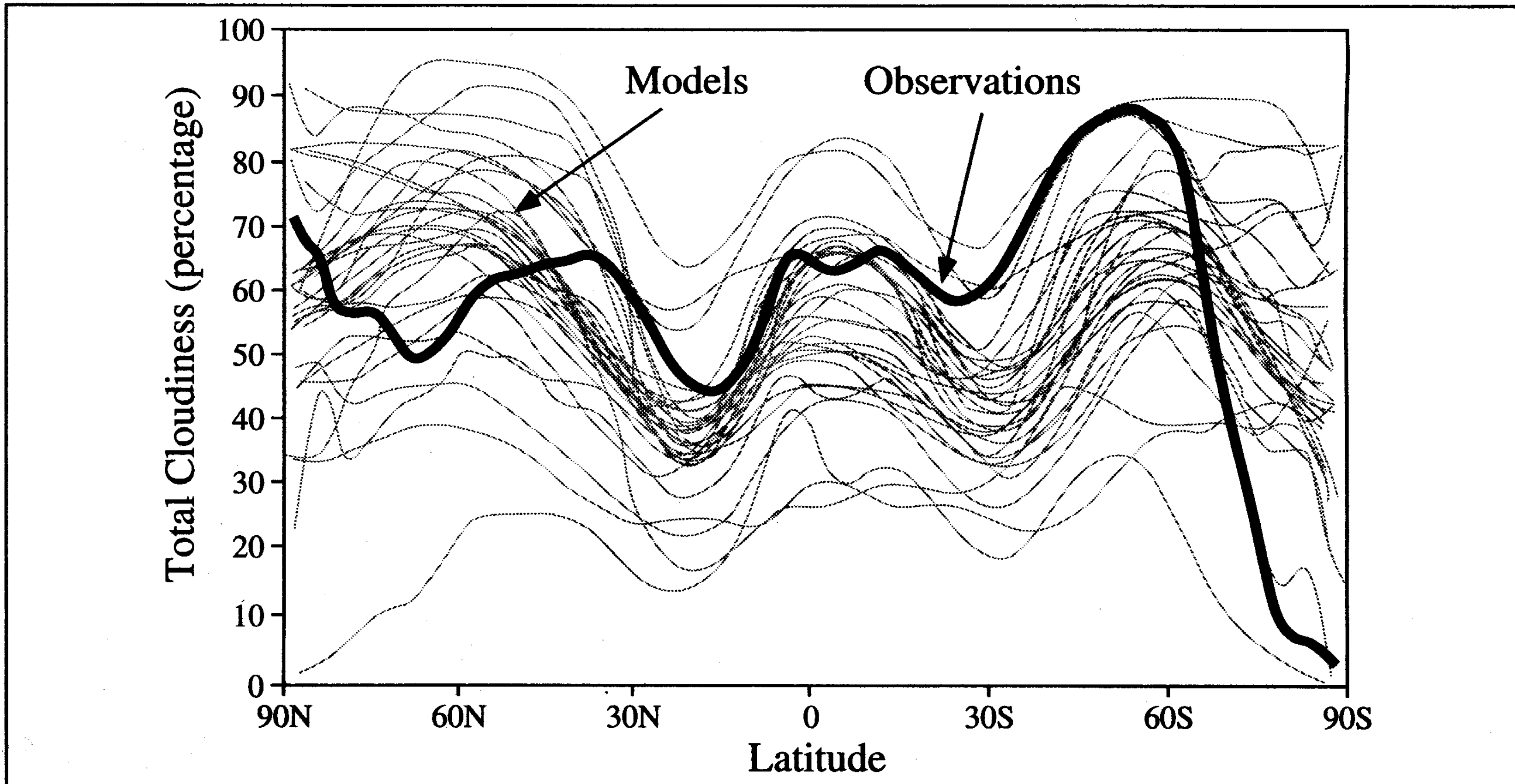
Clearly, our long persisting ‘official estimates’ of climate sensitivity demand positive feedbacks. These arise, *in models*, primarily from water vapor, clouds, and snow/ice albedo. A water vapor feedback factor of about 0.5 is common to most models, and is the largest of the three feedbacks. (Recall that water vapor and clouds are, by far, the most important greenhouse substances in the atmosphere.)

The water vapor feedback was first proposed by Manabe and Wetherald (1971). They noted, using a one-dimensional model, that keeping relative humidity fixed led to a doubling of the response to increasing CO_2. The point was that the same relative humidity at a higher temperature meant more water vapor, given the Clausius-Clapeyron Relation. However, the Clausius-Clapeyron Relation refers to the saturation vapor pressure, and the atmosphere is not saturated. Nevertheless, existing models behave as though relative humidity were, indeed, fixed.

Several things are worth noting about the water vapor feedback:

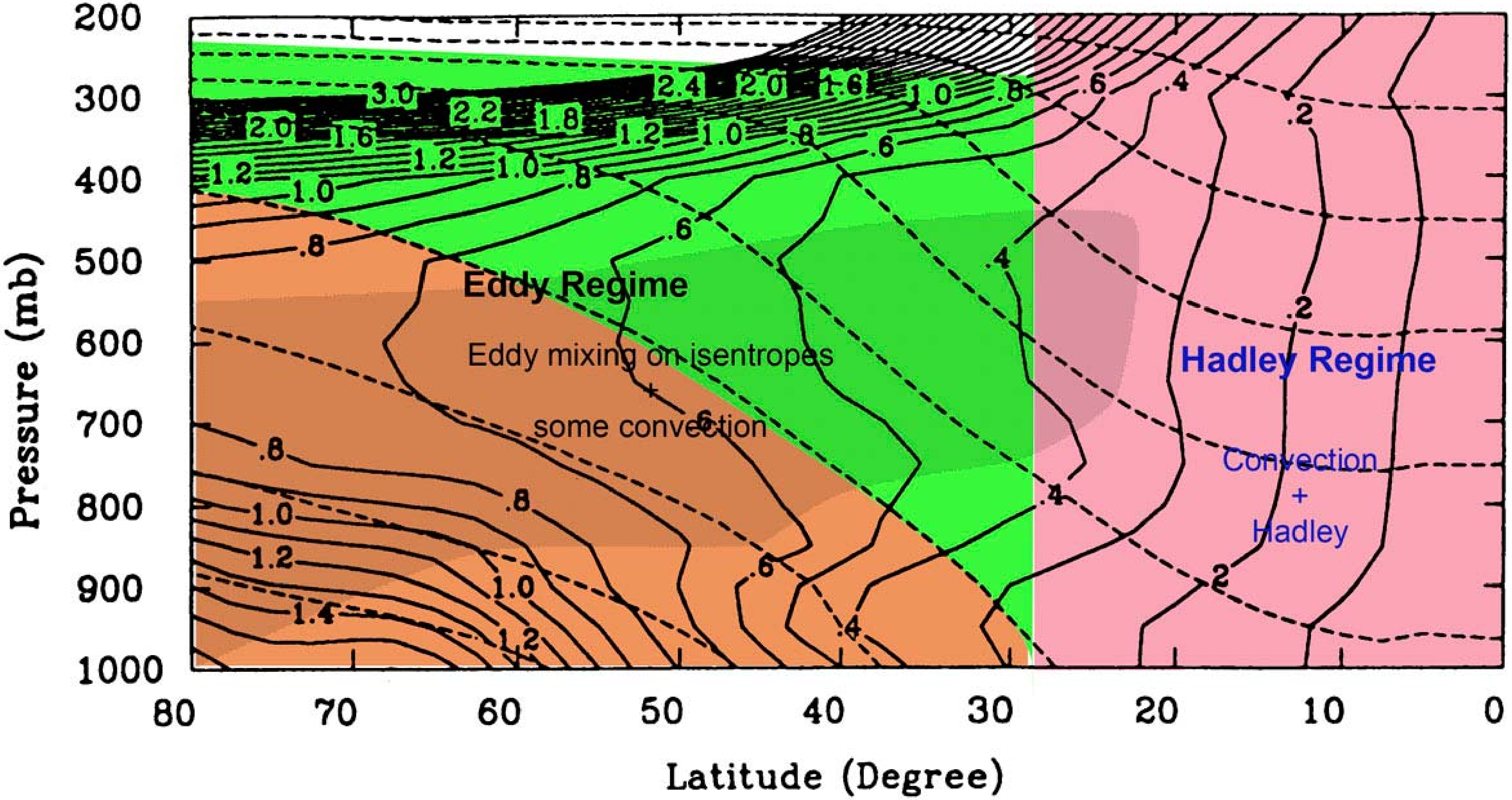
1. Once it is present, relatively small contributions to the feedback factor from other processes can lead to large changes in climate sensitivity. A feedback factor of 0.5 leads to a sensitivity of 2C. Adding another feedback factor of 0.5 leads to an infinite sensitivity! Thus, our uncertainty over the behavior of clouds is largely responsible for the wide range of sensitivities displayed by models -- despite the fact that their contribution (in the models) to the feedback factor is smaller than that of water vapor.

Figure 1. Each thin gray line shows an individual model's hindcast of percentage cloud cover averaged by latitude. The black line shows the observed cloud cover.



The above chart shows cloudiness for all the models referred to in the IPCC TAR. We see that none of the model results is close to observations or, for that matter, to other model results.

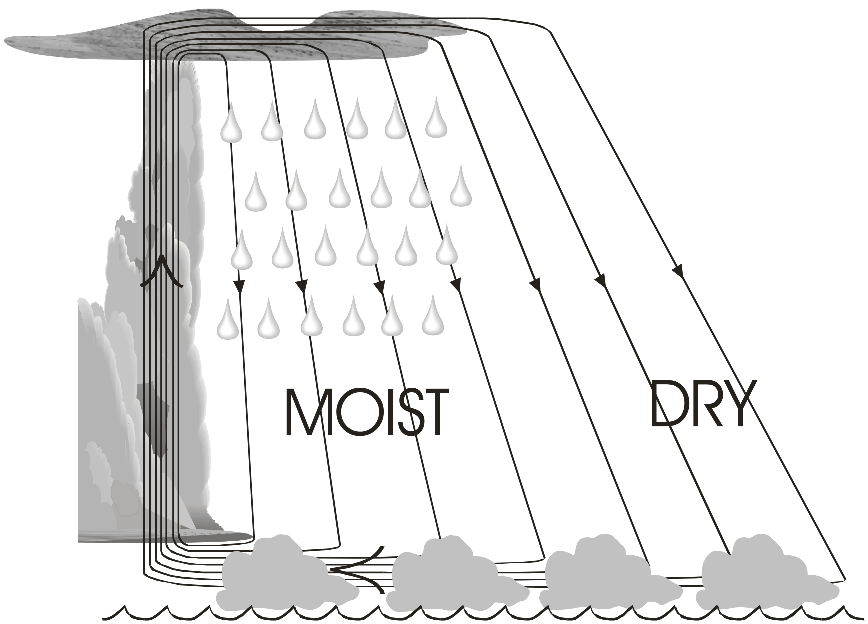
2. While one may speak of an average water vapor feedback, the nature of the water vapor budget is radically different in the tropics and in the extratropics, as is the intimate relation between clouds and relative humidity.



In the extratropics, transport is mostly by wave motions on isentropic surfaces. Rising air cools and relative humidity increases leading to saturation, condensation, and the formation of stratiform clouds. Precipitation leads to a loss of water substance. Descending air is associated with decreasing relative humidity and dryness. Nothing in this process suggests any obvious feedback (Yang and Pierrehumbert, 1994). *Note that in the extratropics, high relative humidity leads to the formation of stratiform clouds (which are of primary importance to the radiative budget).*

In the tropics, transport is mainly due to cumulus towers, and large scale quasi-steady circulations (nominally the Hadley and Walker circulations). Regions of large scale descent are dry, and regions of large scale ascent are moist. However, ….

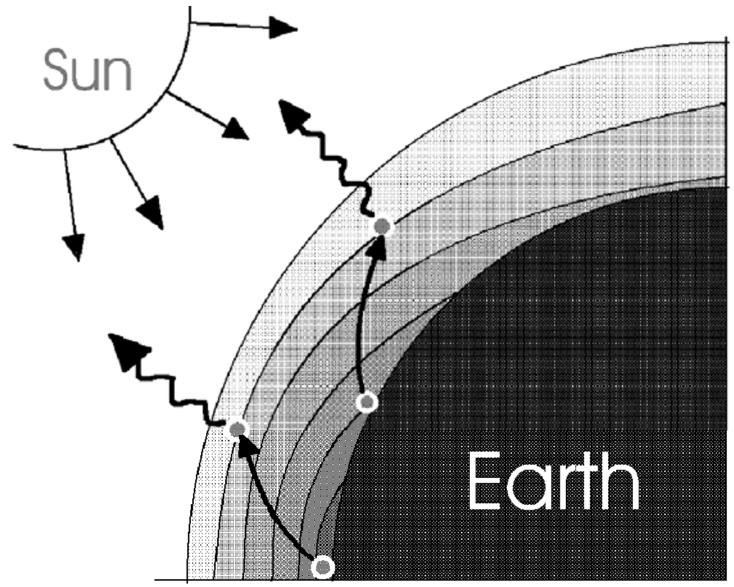
Within the ascending regions, the actual ascent is concentrated in the cumulonimbus towers. Air, outside these towers, is mostly descending. The air outside the towers is moistened by the evaporation of precipitation from stratiform clouds formed from the detrainment of ice from the cumulonimbus towers. *Note that in the tropics, stratiform clouds lead to high humidity*.



Given the acknowledged poor performance of models with respect to clouds, it is surprising that it is often claimed that water vapor is well represented.

3. Water vapor is *bounded* by the Clausius-Clapeyron Relation, and specific humidity does decrease sharply with latitude and altitude.

Infrared opacity is greatest at the ground over the tropics, and diminishes as one goes poleward and upward. Air currents bodily carry heat to regions of diminished infrared opacity where the heat is radiated to space -- balancing absorbed sunlight.



Lighter shading schematically represents reduced opacity due to diminishing water vapor density.

The fact that changes in circulation can lead to the deposition of heat at different latitudes is probably a major reason why changes in dynamics can lead to changes in global mean temperature (even without external forcing) as heat is deposited in regions with different infrared opacity.

Although we shall return to the issue of the physics of the feedback processes, it is reasonable at this point to ask if climate sensitivity might be determined without dealing with the complexity of the feedback processes themselves.

Ideally, one might look at observed changes in global mean temperature and compare these with forcing. It is crucial, in any such comparison, to take account of the fact that ocean delay depends on climate sensitivity. Here, the original definition of sensitivity is of use.

$$\text{Sensitivity} = \frac{\Delta T}{\Delta F}$$

For a given change in ΔT, a more sensitive climate will present a smaller ΔF. Hence, it will take longer for the ocean (with its large heat capacity) to reach ΔT.

A number of papers have looked at this, including

Lindzen, R.S. and C. Giannitsis (1998) On the climatic implications of volcanic cooling. *J. Geophys. Res.*, **103**, 5929-5941.

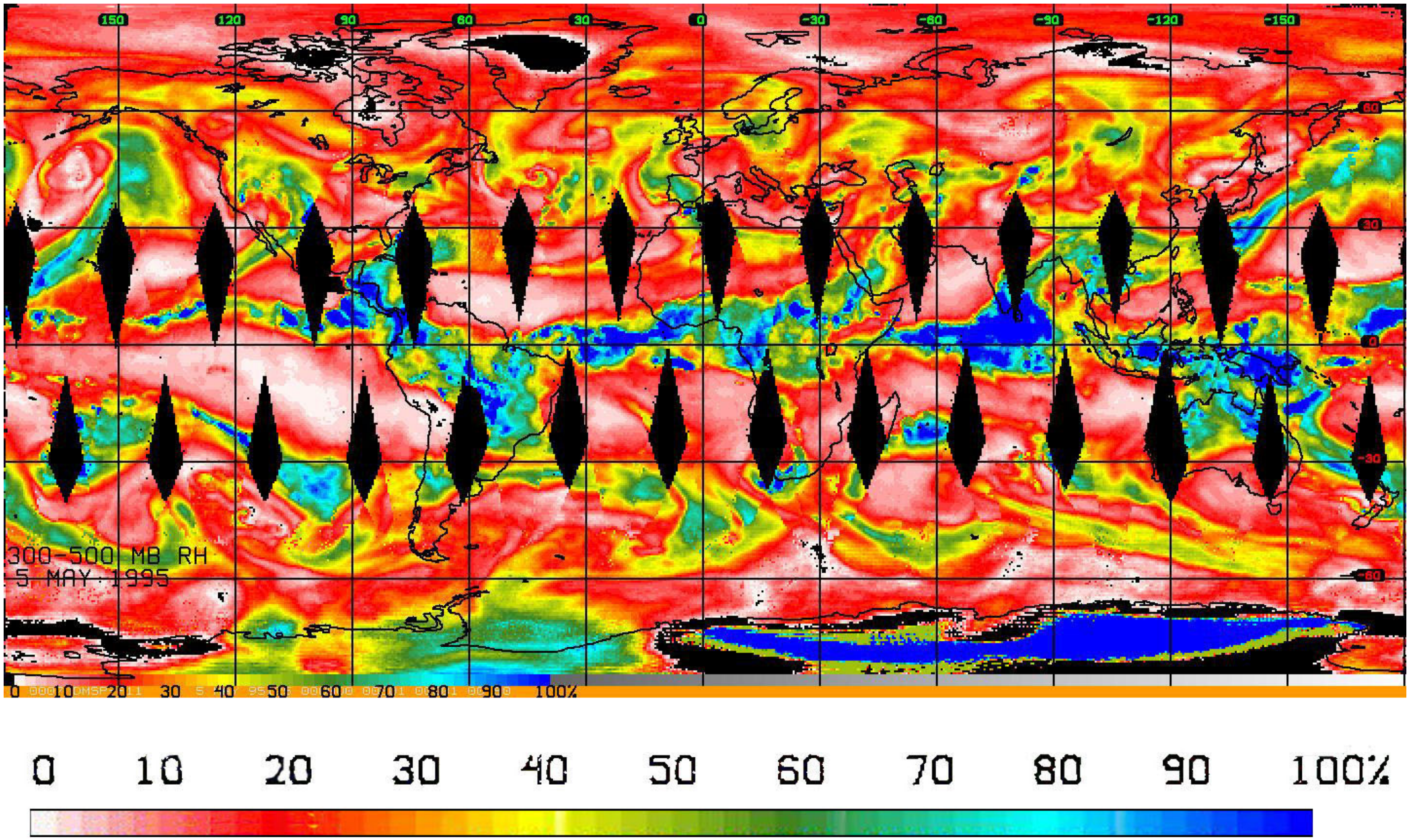
Lindzen, R.S. and C. Giannitsis (2002) Reconciling observations of global temperature change. *Geophys. Res. Ltrs.* **29**, (26 June) 10.1029/2001GL014074

Douglass, D.H., E. G. Blackman, and R.S. Knox (2004) Temperature response of Earth to the annual solar irradiance cycle. *Phys. Lett* A **323**, 315-322 (see also erratum)

All the results require low sensitivity and negative feedbacks…. which finally brings us to the Iris Effect.

For over a decade, I have been looking at how the water vapor feedback might actually work. In Sun, D-Z. and R.S. Lindzen (1993) (Distribution of tropical tropospheric water vapor. *J. Atmos. Sci.*, **50**, 1643-1660), we noted that the source of tropical water vapor was evaporation from precipitation from cirrus detrained from cumulus, and that the humidity would depend on the efficiency of precipitation formation within cumulonimbus towers. However, it soon became evident that it was probably inappropriate to consider the horizontally averaged humidity.

Both microwave and ir soundings showed that humidity varied sharply with well delineated dry and moist regions.



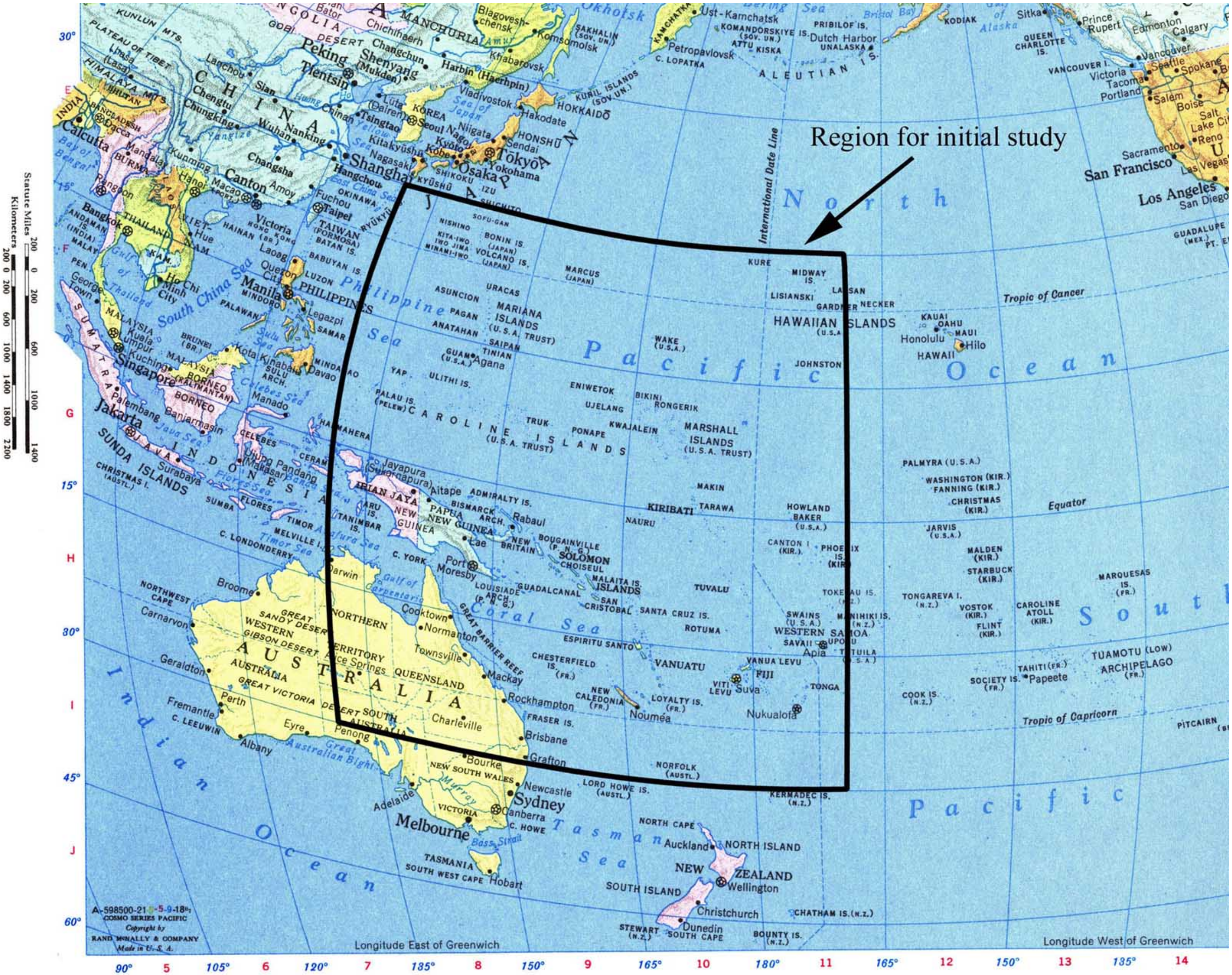
Humidity in the dry regions is found consistent with source free advection. Humidity in moist regions is high. The potential for changing humidity in either region is limited. However, in the tropics there is ample possibility for changing the relative areas of moist and dry regions. With this realization, we began the search for the Iris. Essentially, we wanted to see how the area of moist air changed with underlying surface temperature.

There were immediate problems:

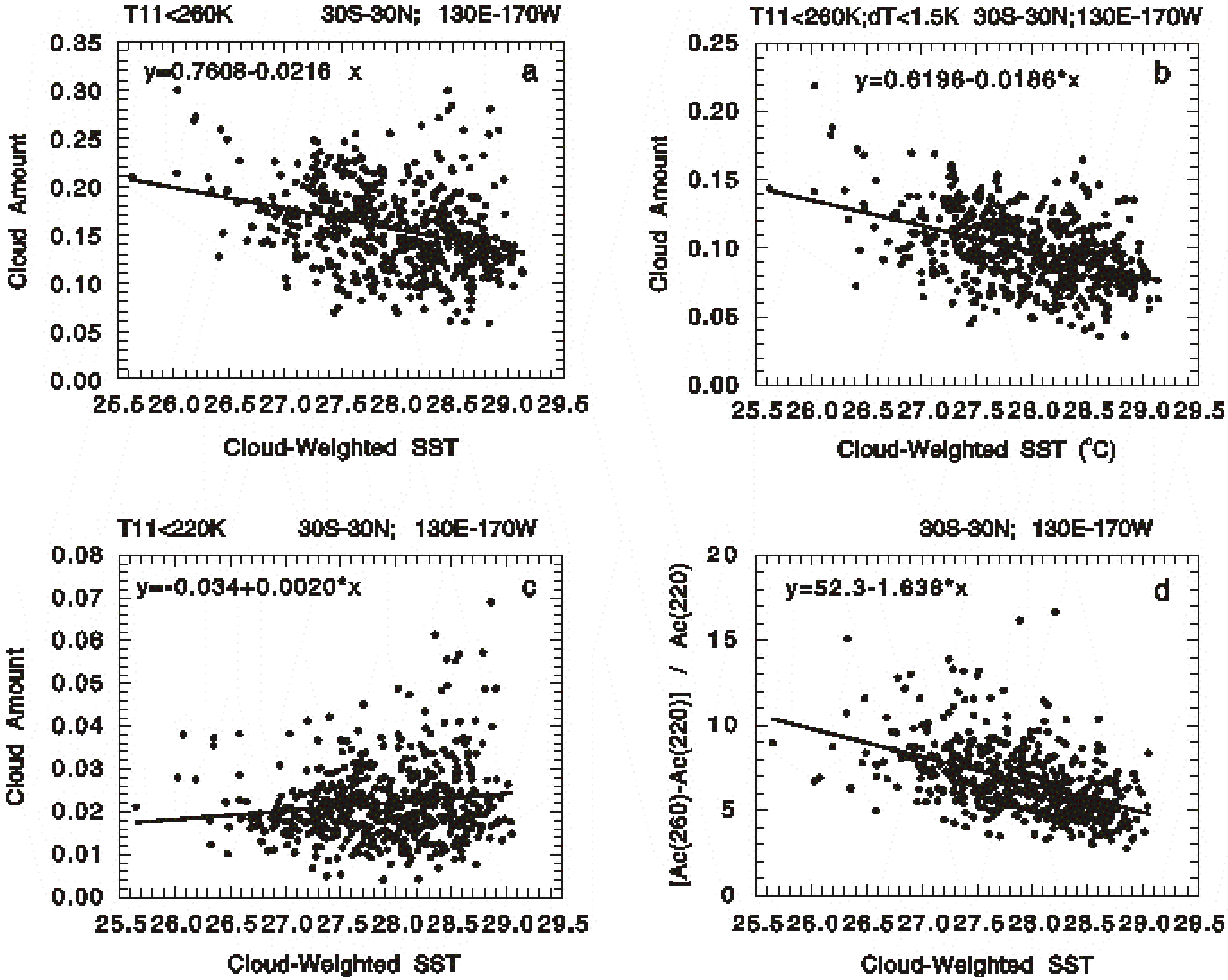
Cloud time scales are short; one needs hourly data. One needs continuous spatial coverage on scales appropriate to cumulonimbus clouds. Satellites that can provide useful measurements of humidity don’t satisfy these needs. Geostationary satellites do satisfy these needs, but they don’t measure humidity or even thin clouds.

There were also problems with surface temperature data. NCEP surface temperature data only has weekly resolution. However, to the extent that SST changes relatively slowly, our hope was that this would suffice. Nonetheless, it was essential to avoid land surfaces where the low heat capacity allows rapid temperature variations.

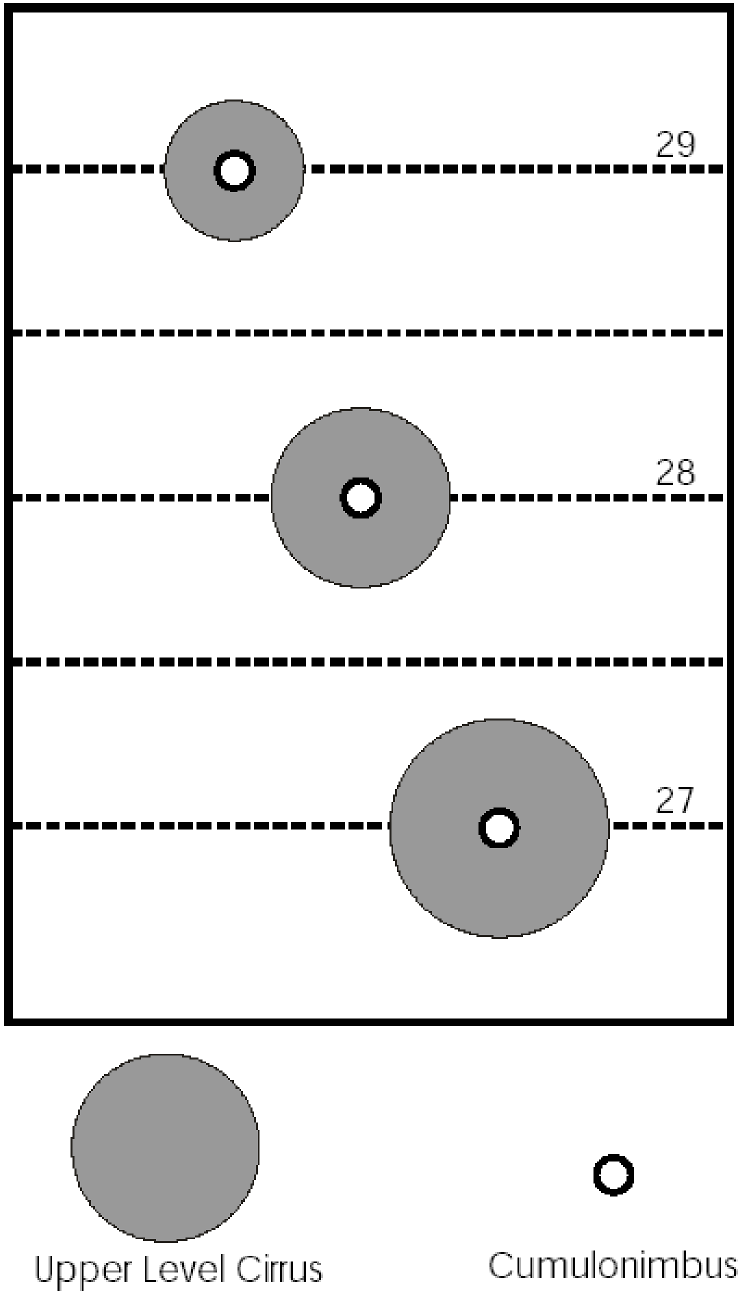
Approach taken: Use GMS5 data to observe area of relatively thick clouds, and assume that this can serve as a ***surrogate*** for all stratiform clouds associated with deep convection and for humidity arising from precipitation from these clouds. Test sensitivity of results to actual choice of measure for thick clouds. Consider different degrees of proportionality between cloud area and humidity.



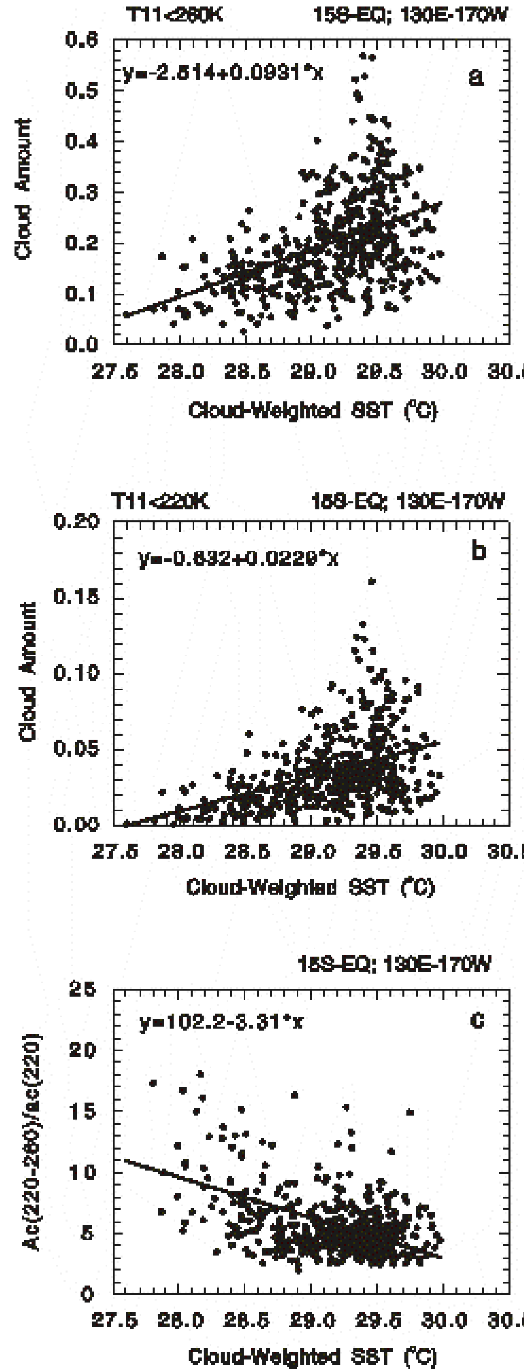
Region for initial study
North Pacific Ocean
South Pacific
Indian Ocean
Coral Sea
Tasman Sea
International Date Line
Tropic of Cancer
Equator
Tropic of Capricorn
AUSTRALIA
NEW ZEALAND
HAWAIIAN ISLANDS
MARSHALL ISLANDS (U.S.A. TRUST)
CAROLINE ISLANDS (U.S.A. TRUST)
MARIANA ISLANDS (U.S.A. TRUST)
SOLOMON ISLANDS
KIRIBATI
VANUATU
FIJI
TUVALU
WESTERN SAMOA
JAPAN
CHINA
PHILIPPINES
INDONESIA
Statute Miles
Kilometers
A-598500-21-5-9-18
COSMO SERIES PACIFIC
Copyright by
RAND McNALLY & COMPANY
Made in U. S. A.
Longitude East of Greenwich
Longitude West of Greenwich


T11<260K 30S-30N; 130E-170W
y=0.7608-0.0216 x
a
Cloud Amount
Cloud-Weighted SST
T11<260K;dT<1.5K 30S-30N;130E-170W
y=0.6196-0.0186*x
b
Cloud Amount
Cloud-Weighted SST (°C)
T11<220K 30S-30N; 130E-170W
y=-0.034+0.0020*x
c
Cloud Amount
Cloud-Weighted SST
30S-30N; 130E-170W
y=52.3-1.636*x
d
[Ac(260)-Ac(220)] / Ac(220)
Cloud-Weighted SST

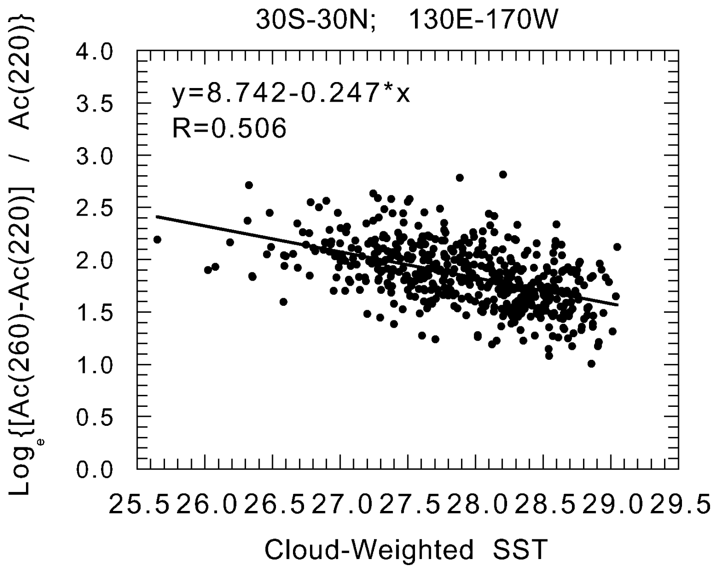
5

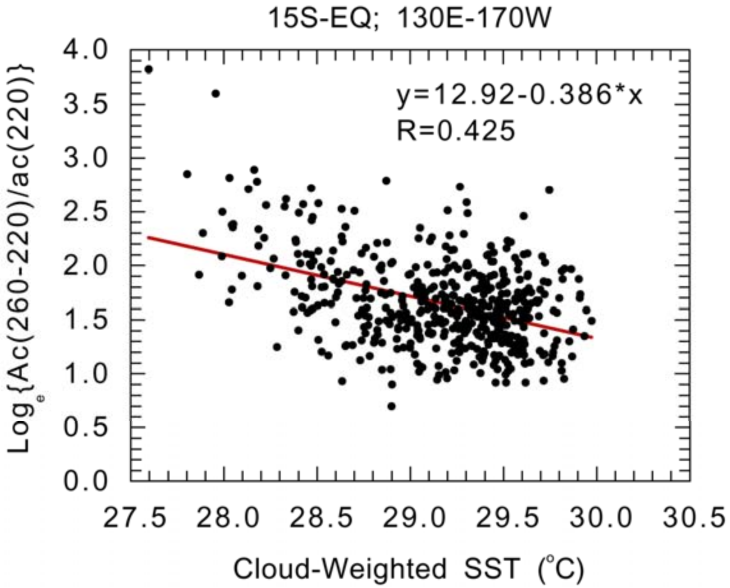


29
28
27
Upper Level Cirrus
Cumulonimbus

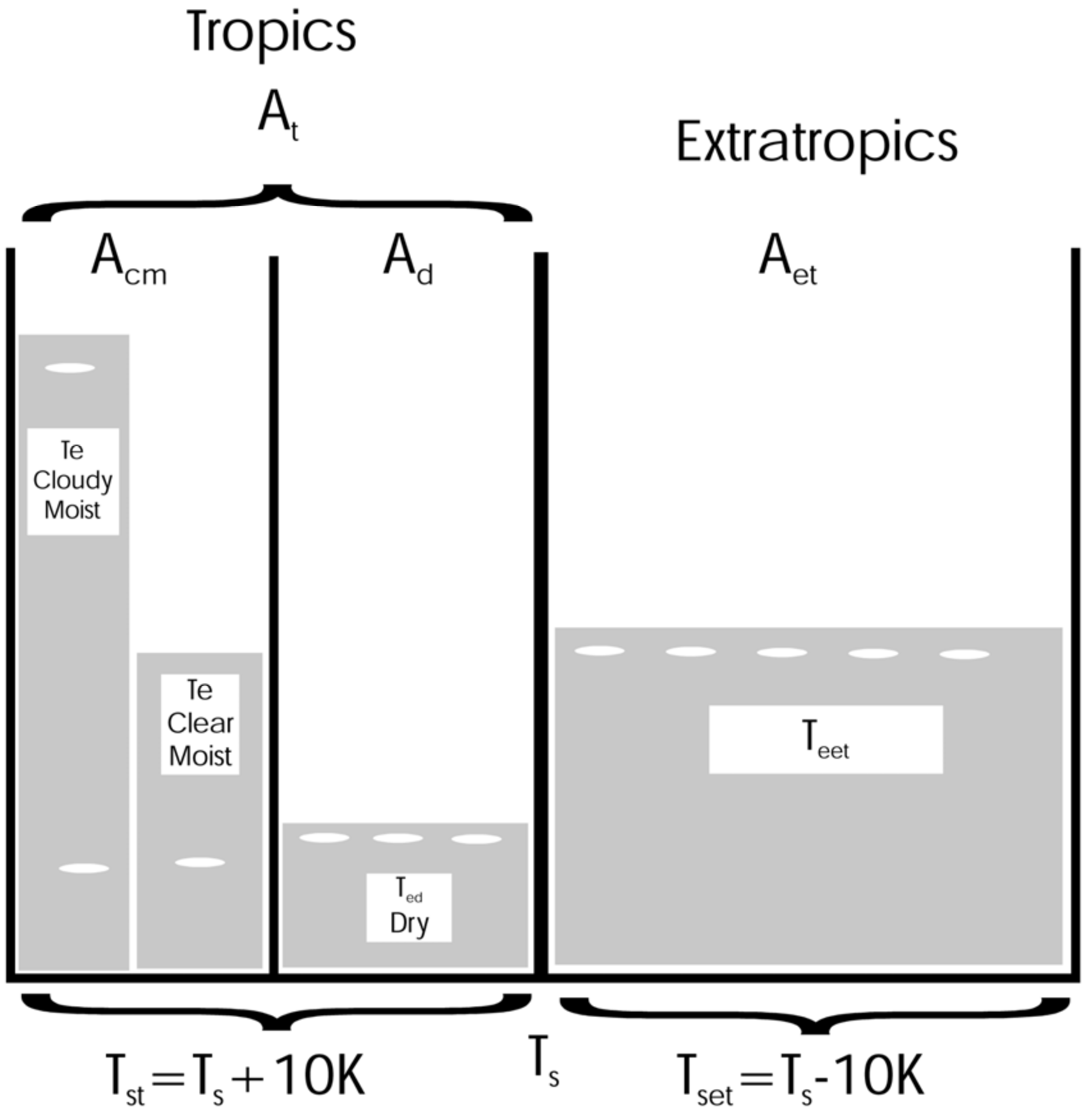


T11<260K
15S-EQ; 130E-170W
y=-2.514+0.0931*x
a
Cloud Amount
Cloud-Weighted SST (°C)
T11<220K
15S-EQ; 130E-170W
y=-0.632+0.0229*x
b
Cloud Amount
Cloud-Weighted SST (°C)
15S-EQ; 130E-170W
y=102.2-3.31*x
c
Ac(220-260)/ac(220)
Cloud-Weighted SST (°C)



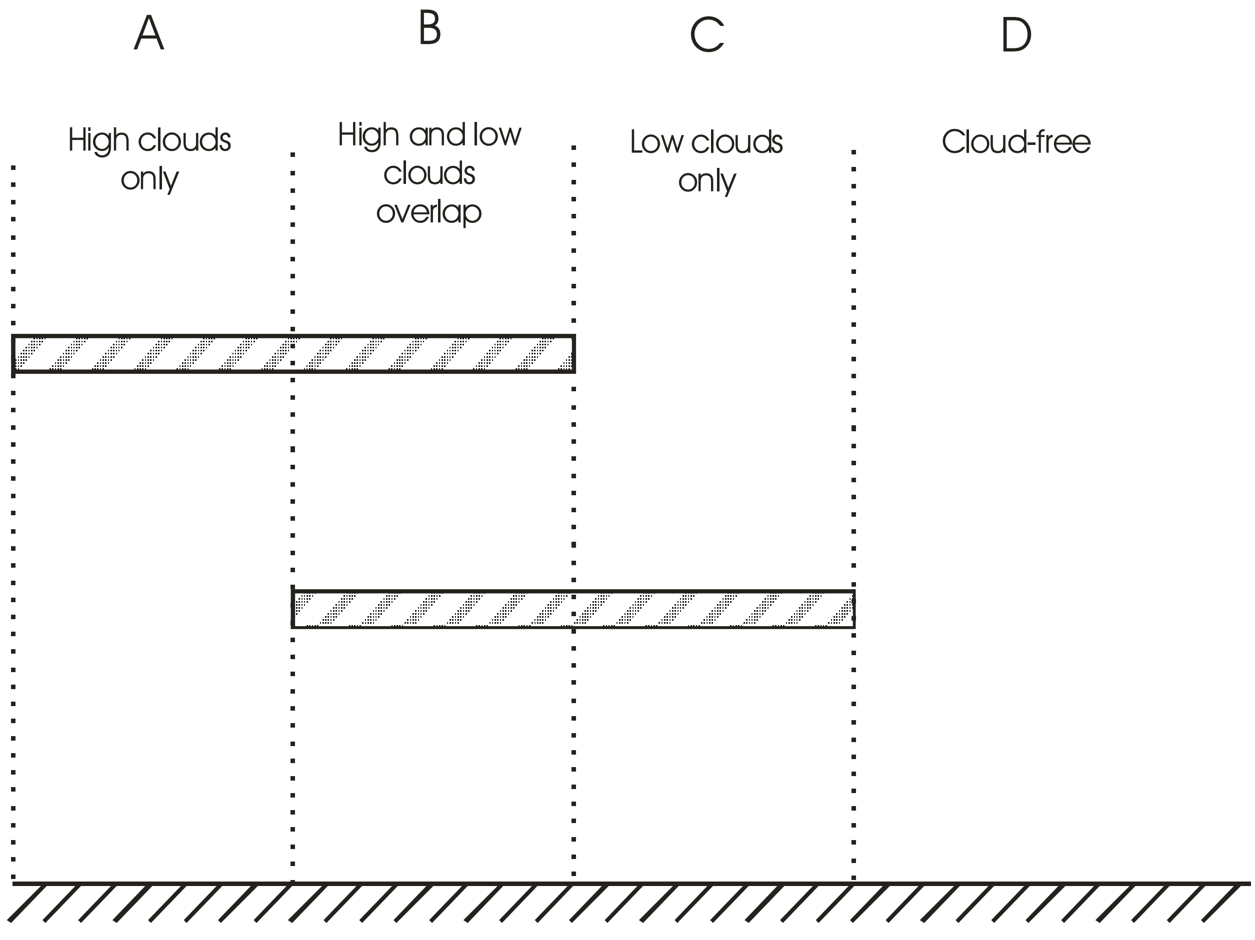


The Iris Effect refers, in the first instance, to the observation that the stratiform cloud area per unit cumulonimbus activity decreases as SST beneath the clouds increases. How does this translate into a climate feedback?

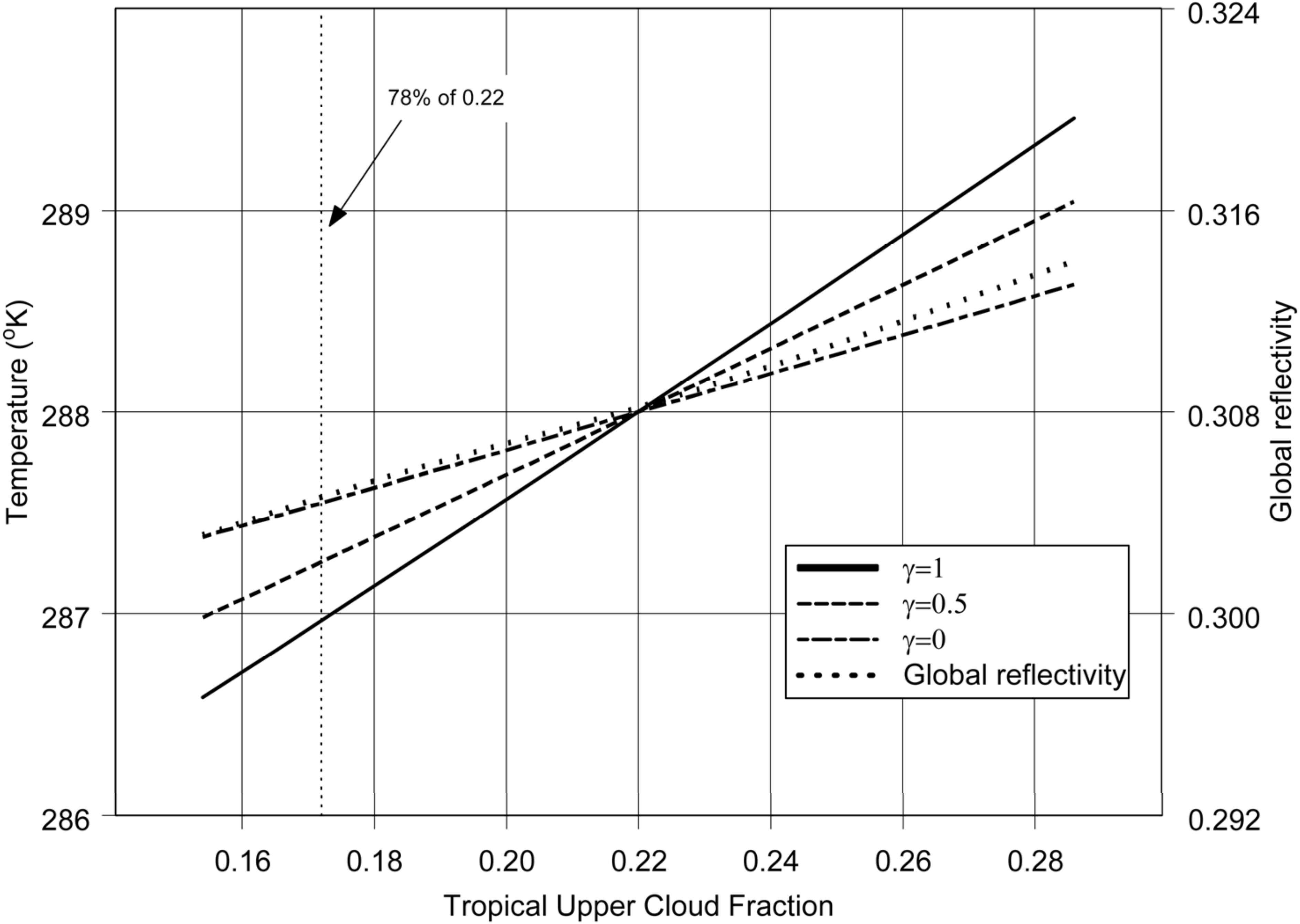


Simple 3.5-box model for the Greenhouse Effect due to clouds and water vapor

The areas are somewhat arbitrary. The region Acm is taken to contain all stratiform clouds (not just Ac(260)) as well as the cloud free moist regions. The properties of both clouds and the moist clear region are chosen to be both plausible and consistent with ERBE observations. As long as one chooses values consistent with ERBE, the results are fairly independent of the precise details of other choices.


A
B
C
D
High clouds only
High and low clouds overlap
Low clouds only
Cloud-free

To determine the feedback factor resulting from the Iris Effect, one changes the area of the cloudy moist region in accordance with Iris observations for 1^{o}C warming, and one changes the area of the clear moist region in accordance with the assumed degree of proportionality. One then calculates the change in global mean temperature that would result from the changes in the two areas. This gives one the appropriate feedback factor for the Iris Effect.



78% of 0.22
Temperature (°K)
289
288
287
286
Global reflectivity
0.324
0.316
0.308
0.300
0.292
0.16
0.18
0.20
0.22
0.24
0.26
0.28
Tropical Upper Cloud Fraction
γ=1
γ=0.5
γ=0
Global reflectivity

TABLE 2. Modification of climate sensitivity in presence of both model feedbacks and various modifications of the iris feedback.

Iris feedback factor (f)	GCM feedback factor (f) (high)	GCM feedback factor (f) (low)	Total feedback factor (f) (high)	Total feedback factor (f) (low)	Net gain $1/(1-f)$ (high)	Net gain $1/(1-f)$ (low)	Response to $2 \times CO_2$ (°C) (high)	Response to $2 \times CO_2$ (°C) (low)
–1.1	0.7	0.2	–0.4	–0.9	0.71	0.53	0.852	0.636
–0.7	0.7	0.2	–0.0	–0.5	1.0	0.67	1.2	0.804
–0.45	0.7	0.2	0.25	–0.25	1.33	0.8	1.596	0.96

The preceding material constitutes the essence of the Iris Paper.

After an unusually intense but useful review (by four reviewers), the paper was published in the Bulletin of the American Meteorological Society, and simultaneously, the editor of BAMS was replaced.

(Lindzen, R.S., M.-D. Chou, and A.Y. Hou (2001) Does the Earth have an adaptive infrared iris? *Bull. Amer. Met. Soc.*, **82**, 417-432.)

What was the response to this paper (other than the replacement of the editor of BAMS)?

Over the next year or so, several ‘attacks’ on the paper appeared, as well as several papers confirming the predictions of the paper while maintaining that the results were not due to the Iris Effect.

(N.B. Criticism in red; response in green.)

Harrison, H., 2002: Comments on "Does the earth have an adaptive infrared iris?" *Bull. Amer. Meteor. Soc.*, **83**, 597.

Bell, T. L., M.-D. Chou, R.S. Lindzen, and A.Y. Hou (2002) Response to Comment on "Does the Earth Have an Adaptive Infrared Iris?" *Bull. Amer. Met. Soc.*, **83**, 598-600.

The above was the only criticism to appear in the conventional form of a letter so that our response could appear simultaneously. It dealt with an obscure statistical issue that was easily dismissed, and the article has not, to the best of my knowledge, been seriously referred to since.

NO EVIDENCE FOR IRIS

BY DENNIS L. HARTMANN AND MARC L. MICHELSEN

Careful analysis of data reveals *no* shrinkage of tropical cloud anvil area with increasing SST

Lindzen et al. (2001, hereafter LCH) present observational analysis suggesting that the area coverage of anvil clouds associated with tropical convection is less extensive when sea surface temperatures (SSTs) are higher. This conclusion is based on a negative correlation between the fractional area coverage by high clouds and the average SST under the clouds. LCH observe this correlation in about 20 months of Geostationary Meteorological Satellite (GMS) radiance data for the oceanic regions in the domain between 30°S–30°N and 130°E–170°W. LCH then used this correlation to hypothesize that increased SST leads to reduced area of anvil cloud associated with convective cores in the Tropics. They then concluded that reduced anvil cloud would give rise to a negative feedback through reduced upper-tropospheric water vapor and diminished greenhouse effect.

We have obtained the cloud data from LCH and have investigated the geographical patterns of cloud fraction variation that are associated with variations in the cloud-weighted SST. These patterns show that the changes in cloud-weighted SST for clouds with

tions in the deepest convective clouds (those with emission temperatures less than 220 K), which occur only near the equator where surface temperatures are highest. LCH assume that the coldest clouds are representative of the convective cores that feed anvil clouds. In this article we show that the deep convective cores are separated by more than 1000 km from the clouds that are associated with most of the variation in cloud-weighted SST. The explanation for the correspondence between cloud-weighted SST and cloud area is thus a shift in the latitude or longitude of the cloudiness and not a change in the relation between deep convective and associated tropical anvil cloud amounts. These latitude and longitude shifts are associated with meteorological forcing and not with SST forcing. Much of the meteorological forcing seems to originate in the extratropics and is probably unrelated to tropical SSTs. *If the SSTs and the near-equatorial clouds remain fixed and constant, any variation in subtropical cloud fraction will produce the negative correlation on which LCH base their negative feedback hypothesis.*

Hartmann, D. L., and M. L. Michelsen, 2002: No evidence for iris. *Bull. Amer. Meteor. Soc.*, **83**, 249–254.

Lindzen, R.S., M.-D. Chou, and A.Y. Hou (2002) Comments on "No evidence for iris." *Bull. Amer. Met. Soc.*, **83**, 1345–1348

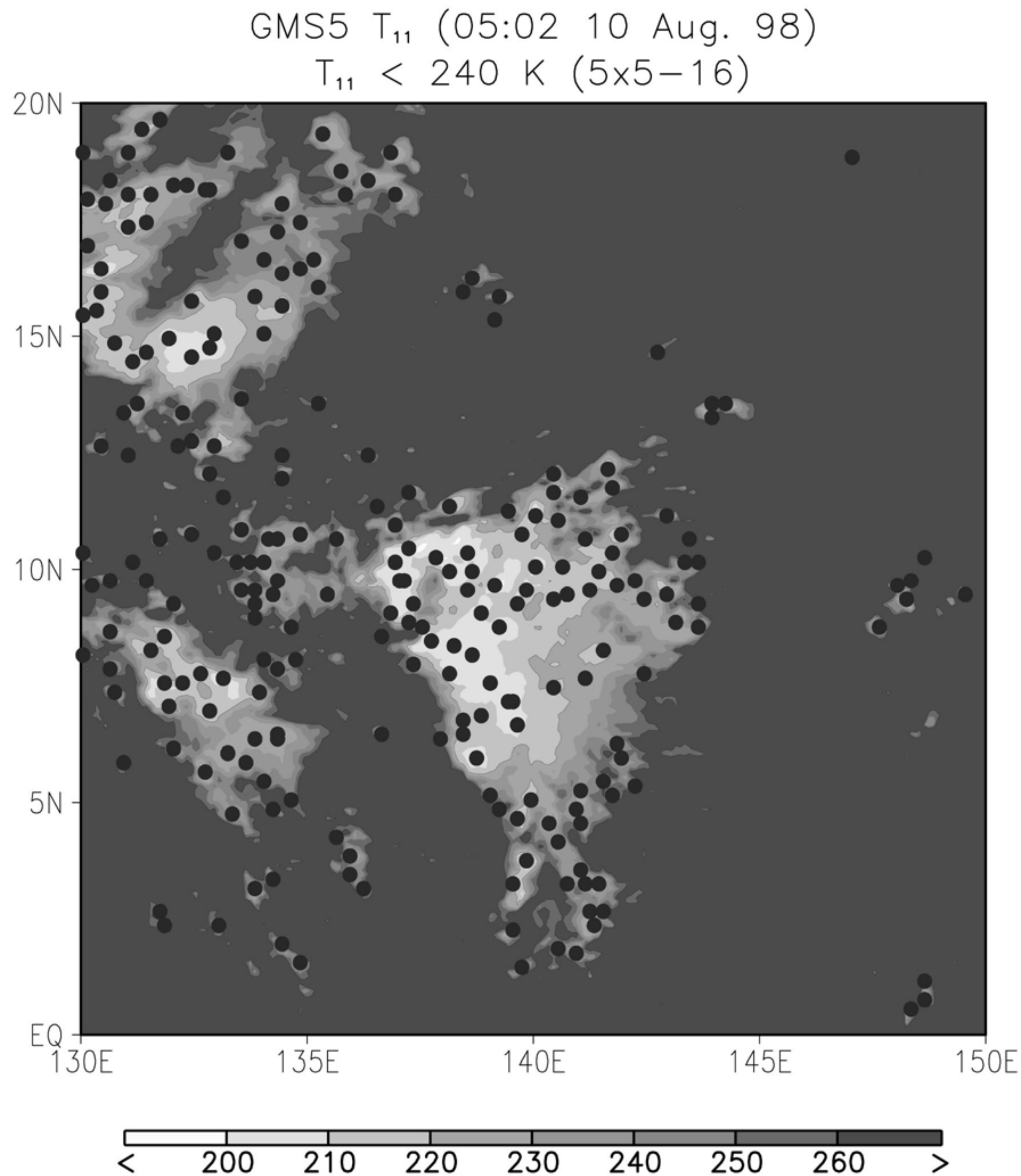
In contrast to the previous criticism, the editor of BAMS chose to publish this as a separate paper so that our response would be delayed by about 6 months. The editor also labeled the criticism as "Careful analysis of data reveals *no* shrinkage of tropical cloud anvil area with increasing SST."

The basic assertion of Hartmann and Michelsen was that the stratiform clouds we were looking at did not originate in cumulonimbus convection. Rather, they were clouds associated with extratropical systems that intruded into the tropics. Such clouds, they argued, were associated with colder SST, and hence would produce an increase in cloud cover associated with a decrease in cloud weighted SST. We had, in fact, explicitly tested for this in our paper.

It is easy to dismiss the Hartmann-Michelsen assertion on several counts:

1. The Iris Effect would diminish as we restricted ourselves to regions closer to the equator if HM were correct. If anything, the effect increased.

2. Close examination of GMS5 data permitted us to identify cumulonimbus cores associated with cirrus in all parts of the domain.



GMS5 T_{11} (05:02 10 Aug. 98)
T_{11} < 240 K (5x5−16)
20N
15N
10N
5N
EQ
130E
135E
140E
145E
150E
<
200
210
220
230
240
250
260
>

There is also Del Genio, A.D., and W. Kovari, 2002 Climatic properties of tropical precipitating convection under varying environmental conditions. *J. Climate*, **15**, 2597-2615.

These authors note that when they looked at a warmer region and a cooler region in the tropics, they found more upper level cloud cover in the warmer region in contrast to what the Iris Effect would suggest. Unfortunately, they failed to distinguish cloud cover from cloud cover per unit cumulus. The Iris Effect refers to the latter.

Fu, Q., Baker, M., and Hartman, D. L.: Tropical cirrus and water vapor: an effective Earth infrared iris feedback? *Atmos. Chem. Phys.*, **2**, 31–37, 2002.

Chou, M.-D., R.S. Lindzen, and A.Y. Hou (2002a) Impact of Albedo Contrast between Cirrus and Boundary-Layer Clouds on Climate Sensitivity. *Atmospheric Chemistry and Physics*, **2**, 99-101.

Lin, B., B. Wielicki, L. Chambers, Y. Hu, and K.-M. Xu, 2002: The iris hypothesis: A negative or positive cloud feedback? *J. Climate*, **15**, 3–7.

Chou, M.-D., R.S. Lindzen, and A.Y. Hou (2002b) Comments on “The Iris hypothesis: A negative or positive cloud feedback?” *J. Climate*, **15**, 2713-2715.

Both these criticisms argued that LCH had misrepresented the radiative properties of upper level cirrus by overemphasizing their ir effect and understating their visible effect. Lin et al went so far as to claim that the main impact of upper level clouds was in the visible.

They essentially arrived at these conclusions by ignoring the fact that we were taking Ac(260) as a surrogate for all tropical upper level cirrus, and instead asserting that the clouds defined by Ac(260) were essentially the only clouds we were considering. (N.B. Virtually all documents dealing with the impact of clouds on climate agree that for upper level cirrus, the primary radiative impact is in the ir.) Thus, they focused on the clouds in the immediate vicinity of the cumulus cores which are atypically thick and do have a high albedo. We have already noted that separating thick clouds from thinner clouds did not change the magnitude of the effect. Neither did changing the reference brightness temperature.

In brief, these criticisms managed to change the results obtained by LCH by claiming that LCH had followed an irrational procedure different from the one actually employed.

So, what was the result of these criticisms that were clearly made in the absence of any serious attempt to understand LCH?

In the environmental literature as well as in papers by scientists like Jim Hansen and Steve Schneider, LCH is accompanied by the adjective *discredited*.

discredit (from American Heritage Dictionary 4th Edition)

TRANSITIVE VERB: Inflected forms: dis-cred-it-ed, dis-cred--it-ing, dis-cred-its
1. To damage in reputation; disgrace. 2. To cause to be doubted or distrusted. 3. To refuse to believe.
NOUN: 1. Loss of or damage to one's reputation. 2. Lack or loss of trust or belief; doubt. 3. Something damaging to one's reputation or stature.

I would suggest that this is an unusual word to use in connection with a serious paper.

A possible explanation may be implicit in the following paper:

Lin, B., T. Wong, B. Wielicki, and Y. Hu, 2004: Examination of the decadal tropical mean *ERBS* nonscanner radiation data for the iris hypothesis. *J. Climate*, **17**, 1239-1246.

Chou, M.-D., and R.S. Lindzen, 2004: Comments on "Examination of the decadal tropical mean *ERBS* nonscanner radiation data for the iris hypothesis." submitted to *J. Climate*. (in press)

For example, Lin et al assumed differences from LCH imply the absence of negative feedbacks, but the negative feedbacks implied by their results are larger than those in LCH. Similarly, they assumed that the discrepancies between their specification of cloud radiative properties and the observations is due to their use of the Iris result that cloud cover diminishes 22% per degree. What they failed to note was that to bring their values into line with observations, they would need much more than 22%.

In brief, Lin et al failed to recognize how conservative LCH was. They assumed (without ever checking) that LCH had attempted to maximize negative feedbacks, and that any deviation from LCH would eliminate negative feedbacks. This is clearly not the case.

Finally, there are a number of papers which simply report on satellite results for the radiation balance in the tropics over the 1990’s.

Chen, J., B.E. Carlson, and A.D. Del Genio, 2002: Evidence for strengthening of the tropical general circulation in the 1990s. *Science*, **295**, 838-841.

Wielicki, B.A., T. Wong, ..., 2002: Evidence for large decadal variability in the tropical mean radiative energy budget. *Science*, **295**, 841-844.

Cess, R.D. and P.M. Udelhofen, 2003: Climate change during 1985–1999: Cloud interactions determined from satellite measurements. *Geophys. Res. Ltrs.*, **30**, No. 1, 1019, doi:10.1029/2002GL016128.

These papers all report anomalously increased OLR associated with tropical surface warming. Models, by contrast, fail to show such an increase. Presumably that is why it is referred to as anomalous. One can (though the authors don't) infer a negative feedback factor from the observations, and it is at the high end of what LCH estimate. Moreover, the 'anomaly' is associated with reduced upper level cirrus. However, the same papers insist that what they see is not the Iris Effect, but rather the result of a change in the large scale circulation.

However, there is something seriously wrong with this attribution.

The point is that circulation acts primarily to organize convection that is fundamentally due to evaporation. Thus, changes in circulation may act to concentrate convection in one region while suppressing it in another. However, integrated over the tropics, the effect of circulation drops out. The most appropriate measure of convection is the cumulus mass flux, M_c. Averaged over the tropics, $M_c = E/q$, where, E, the evaporation, is generally taken to be proportional to $q_s - q$, where q_s is the saturation humidity, while $q = rh \times q_s$ is the specific humidity near the surface, and rh is the relative humidity. We thus see that the average $M_c = (1-rh) / rh$; ie, it depends only on rh. As long as rh does not change with changes in mean temperature, then M_c remains unchanged as well. This is, in fact, why normalization by M_c is needed to obtain the climate feedback.

In brief, these papers are most likely displaying the Iris Effect.

I would suggest that the Iris Effect, even if ‘discredited,’ is also alive and well.

This leads to a very important question: Namely, what will happen to the exponentially growing climate change community if the sensitivity of climate to global forcing is small?

Could the wish to avoid this question be the reason why our ‘official’ estimates of climate sensitivity have not improved since the Charney Report of 1979?