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The GATE Aircraft Program: A Personal View

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1. General comments

The outstanding feature of the GATE Experiment, in my opinion, was its success. The success exceeded my own and most scientists' anticipations.

The field phase of GATE, I believe, more than achieved its goals, and went beyond expectations in the performance of data systems, the cooperation and dedication of the diverse people, groups, and nations involved, and in the operation itself. More multi-aircraft missions were obtained, with apparently good data, than could reasonably have been anticipated. Furthermore, missions were well distributed among the pre-determined programs and objectives.

2. Hypothesized reasons for success

The reasons for the success of the GATE field phase were, I believe, as follows:

A. People—Enormous devotion and hard work of nearly everyone involved. High level of competence. Relative absence of visitors, operators, arm wavers, and boondogglers. Young and old complemented and stimulated each other. Famous scientists and graduate students together sweated 18–20 hour mosquito-infested days, broiled and froze on aircraft, wielded screwdrivers, tephigrams, calculators, typewriters, xeroxes, and performed good old manual labor, which no one fancied himself or herself too good to do, since it had to be done.

B. Advance planning and rehearsals. These paid off in providing clear-cut goals, priorities, plans for flight patterns and communications and, most of all, pre-acquaintance and teamwork among key participants.

C. Considerable on-site analysis and evaluation of data on all scales. Among the outstanding examples were: analysis relating convection code to dynamic variables; studies of African squall lines; documentation of diurnal convection cycles in the B-array; work on aircraft data and dropwindsonde data in the reconnaissance of several evolving and non-evolving Dakar disturbances.

D. Just plain good luck. Never in 30 years have I seen so many untried systems work on their first major field program. Noteworthy were the NOAA AWRS system and the NCAR dropwindsonde, which I believe may revolutionize some important aspects of meteorology if adequately exploited. In Phase 1, difficulties were experienced with the shipboard Beukers omegasonde windfinding equipment. By the end of Phase 3, however, a few intercomparisons suggested that at least some of these systems may have produced some reliable or at least correctable data.

3. Ship-aircraft relationships

By the time of Phase 3, I believe that ship-to-aircraft communications and cooperation had had the major bugs removed and were working very well. The ship reports were vital in both the pre-flight and in-flight aircraft mission planning. Furthermore, I believe the ships worked with better efficiency and morale when informed of what the aircraft were doing. The horrendous air traffic problems posed by the ship balloons were handled well, with compromises on both sides, with minimal requests to keep ship balloons down because of aircraft tracks. The reports of the vessel *Dallas* to the aircraft team at the beginning of and during missions concerning the boundary layer structure were particularly valuable to the Airborne Mission Scientists in setting flight altitudes.

4. Cooperation among scientists

I was amazed and delighted at the spirit of cooperation and exchange between groups and individuals of all organizations and nations. The less fortunate aspects of competitive spirit and abrasive interactions were minimal. Although sheer exhaustion and illness led to occasional temper flare-ups (including my own), the cause of these was generally recognized with toleration. Good feelings were soon restored, so that overall stimulating and friendly exchanges prevailed between scientists at nearly all times.

5. SMS-GOES satellite imagery—its value and limitations

The GOES satellite imagery was beautiful. The IR and visible pictures were essential to and superbly used in the weather forecasts, overall mission selection, and general planning. The satellite imagery was at its best in following ITCZ developments, in tracking Dakar waves and vortices, and also in following African squall lines.

Even the one-half mile resolution, however, failed to identify most active oceanic mesoscale cloud lines in their growing phases. Many convective systems were not recognized until the dying stages, when cirrus anvils predominated. Enhanced products (e.g., by densitometer) would have greatly helped in the field and will greatly aid in the analysis—if they are not so costly as to be unavailable to those of us working on the data.

One of the major lessons learned in GATE was the limitation of even the best current satellites to identify the early stages of active convection, particularly when short-lived and not organized into persistent systems larger than mesoscale. When many systems became recognizable as so-called "cloud clusters" on the GOES imagery, they in reality consisted only of orphan anvils and dead remnants of earlier convection.

A fact which I suspected before GATE and have now properly documented is that the term "cloud cluster" is unfortunate and should be abandoned. The reason is that it not only says nothing meaningful about the convection, but in fact often gives misleading information regarding the dynamics, physics, organization, and large-scale impact of the convective processes. Only once on my 13 missions in Phase 3 did I see anything resembling a proper "cloud cluster" and then I doubt that this real "cloud cluster" of clouds would have been detected on the satellite prior to its dying stages, if then.

Virtually all convection that I and the other Airborne Mission Scientists investigated and/or saw were *cloud lines*. The isolated penetrative cumulus or cumulonimbus was almost but not quite as rare as the bona-fide "cloud cluster." The point that the term "cloud cluster" is not meaningful, at least in the GATE area, has been made by several other GATE scientists.

Another vitally important limitation in satellite deductions, which can be turned around with further research into important revelations regarding the tropical atmosphere, was the frequent lack of relationship between spirals or rings of penetrative towers and corresponding closed circulations in the wind field. On numerous occasions, satellite stills and/or loops showed cloud features suggesting a closed circulation, which either did not exist in the wind field, or existed at a remote distance from the cloud "center" in a place where it never could have been located by any type of cloud imagery, from satellite, through radar, visual photography or eyeball. This observation does *not* imply a lack of relationship between wind convergence and penetrative convection, which should be a primary subject of study with the GATE data.

6. Radar in relation to mission planning and analysis

The performance of the ships' radars was unexpectedly fine. The reports from those ships which were regularly made available at GOCC (in Phase 3 primarily from the *Quadra* and the *Oceanographer*) were vital aids in mission planning, from 0400 hours when the scientists arrived at GOCC to the final establishment in the air of pattern coordinates, usually at about 1200 hours. The airborne radar on the NOAA C-130 was invaluable in the final decision process on days when missions involving convection were flown.

When acting as Airborne Mission Scientist, I personally used radar (mainly *Quadra* and NOAA C-130) and eyeballs entirely to set my patterns. On each of the occasions that we had a good lively convective day, my Mission Scientist radioed from GOCC that his satellite imagery told him that we were boxing around nothing (!), while I was seeing a beautiful line of building cb's from my aircraft. In each case, when the *Quadra* radar facsimile became available to him, he reported back that we were indeed flying in exactly the right place!

7. Scientific highlights and puzzles

The GATE data will surely provide a gold mine for the study of an atypical tropical region, which has received very little previous investigation. Ingenious investigations with these data may shed light on processes in other parts of the tropics, in part by revealing and explaining differences, as in a nature-performed experiment. However, great caution must be used in extrapolating GATE results to any other part of the tropics.

My pre-GATE contention that the GATE area is almost a "freak" region of the tropics is stronger than ever as a result of my participation. The GATE region differs from the central and western Atlantic and Pacific tropics in the following main ways:

A. Intense latitudinal gradients in both ocean and atmosphere, with a strong oceanic temperature decrease poleward and a very large increase in atmospheric static stability, particularly from about 850–400 mb.

B. The abrupt transition from continental Africa to ocean, resulting in systems in transition, many dying out and others rapidly changing their structure and dynamics.

C. The impact of African disturbances (on the synoptic scale: Dakar waves and vortices. On the mesoscale: squall lines) and *African dust*, which will prove to be a much greater factor than most everyone expected.

D. The abrupt vertical transition between lower monsoon and overlying drier continental air.

E. Much greater static stability than "mean tropical" in mid-troposphere.

F. Predominance of layer-type clouds, irregular cumulus bases, "incoherent" cumuli with poor bottoms, which die from the bottom upward.

G. Unexpectedly strong ocean currents and associated sea temperature gradients.

8. Probable impact of the GATE area meteorological oddities upon analysis objectives

Firstly, I suspect that the CISK concept will be found inapplicable in the GATE area, particularly with regard to "Dakar waves" after they leave the continent. Our dropwindsonde results suggest that the inflow and "action" in these disturbances were in the mid-troposphere and that the lower layers (often mistakenly called the "boundary layer") were apparently inert. The CISK concept may prove more viable in the ITCZ region of the B-array. Findings from this array, however, will not tell us much about disturbances and their development, but only about the extreme eastern tail of the ITCZ—possibly applicable to the corresponding zone in the Eastern Pacific. I am not at all confident that this circulation feature plays any key role in global dynamics, although I shall be happy to be proved wrong on this point.

I would also be highly suspicious of generalizing to the global tropics any parameterization schemes which may be developed and tested in the GATE area. I suspect that there may be as many as 4–5 types of tropical regions which must be differently treated: e.g., Atlantic (eastern and western); Pacific (eastern and western); and continents. Convection operates differently and so do disturbances between these regions, and hence it is hard to imagine that any one simple parameterization scheme will catch the essential energy transactions and large-scale impacts of all of them, although it may eventually be possible to isolate those features that cause the differences and feed these into models via a hierarchy of parameterization schemes.

Finally, I believe that cumulus modelers may be in for rough going in their attempts to simulate the GATE cumuli. The cumuli in the GATE area were more transient than those in Florida or the Caribbean, had poor, uneven and variable bottoms, and frequently cumulus bases existed at four or more levels. "Hot towers" originating at the altocumulus level were not uncommon! It would appear that "entity" models may be useless as may be any isolated cloud model, of the one, two or three dimensional variety. At the present time I believe that the most promising model avenues for the GATE cumuli be in either 1) the field of motion multicloud model of the type developed by Hill (1974), or 2) adaptations of Pielke-type (1974) mesoscale models introducing phase changes and key cloud processes, perhaps highly parameterized.

9. Support and management

A. Forecasts: The forecasts for convection in the B-array made in Phase 3 were the best job of tropical forecasting (omitting severe disturbances) that I have ever had the pleasure to observe. I still cannot decide whether remarkable new skills and tools were responsible, whether the GATE area may be not quite as difficult to forecast as the Caribbean and/or Florida, or what role luck may have played, if any. In any case, the forecasts were both terrific and well communicated.

B. GOCC Operations and Traffic Control: A good job was done. Many scientists felt from time-to-time that the rulebook was too rigidly applied. My view is that with so many aircraft not used to working together and in the midst of ship balloons, as much flexibility was usually allowed as common sense dictated.

C. Aircraft Crews and Maintenance: Excellent.

D. Management: Excellent. A difficult job well handled. Somehow I did not get the impression as I have in smaller examples of "big science" that politics was dominating nor that there were too many cooks in the kitchen. If all "big science" went like the field phase of GATE, most of us would find our reservations about it greatly allayed. The final test will lie in the follow-up and availability of GATE data, discussed in Section 10.

E. Services: Supporting services were sometimes less than adequate. Demands for transportation, clerical support, and technical assistance often overtaxed the available resources. I believe that planning for future field programs should place greater weight on these essential support functions.

10. Conclusion, outlook and major concern

On the whole, the plusses in GATE exceeded the minuses by an order of magnitude. Virtually everything was done either well, or the best it could be under the circumstances. Dozens of groups dedicated themselves unselfishlessly to the achievement of the GATE goals. These goals were, in fact, more than achieved in the field phase.

Whether or not the GATE program really achieves its goals and contributes its potential to meteorology now depends upon the data analysis, which will require large funding, cooperation and dedication, over at least the next five years.

Unfortunately, my experience has been that where most field experiments fail is in their follow-up, namely analysis and publication. The fault in the past has lain with both management, who finds other pressures on funds, and scientists, who find more glamour in running out to the field again, than in gluing their bottoms to a desk chair to carry through the painful and laborious scrutiny, corrections, analysis, reanalysis of data.

In GATE I have still another concern. How are younger, less known, less well-funded meteorologists going to obtain access to the data? Satellite and radar imagery is costly to copy, as are cloud pictures.

How is distribution going to be provided? How is a balance going to be maintained between giving some data rights to those whose ingenuity, dedication, and suffering obtained particular parts of the data and accessibility to the general meteorological public who paid for them in many ways? How is the problem of premature publication of half-baked papers generalizing from fragments of the data to be avoided?

In my opinion, the last question is less important than the first—the main task is for management to fund the data distribution and analysis and for participants and non-participants alike to get to work on them. I do not believe that anyone or any group should "own" any data and that any GATE data must be supplied to anyone on request, requiring at most the cost of duplication. Clearly an honor system must be tried to deal with the possibility of some persons running into print with someone else's data. Alternatively, a review board may have to be established for all publications based on GATE data.

A special effort should be made to disseminate the GATE data throughout the university community and not to confine most of the analysis to government laboratories or to a few well-funded university groups. My experience has been that proper field program analysis requires 5–10 times the financial expenditures as the actual operation and 25–50 times the time expended. The expenditure in money cannot be reduced much, but can be minimized, and the benefits for the future maximized as more students become involved per senior employee. The actual time duration of the analysis can be greatly reduced by wide dissemination of data so that very many people will be motivated to work on them.

Finally, I believe the GATE data, if followed up and analyzed properly, should advance tropical meteorology by a quantum jump, despite the "oddity" of the region studied. If tropical meteorology is advanced by a quantum jump, global meteorology should move forward by a significant amount.

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