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REPORT ON THE 2ND GARP JOC STUDY CONFERENCE ON 4-d DATA ASSIMILATION, PARIS 17-21 NOVEMBER 1975

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1. Introduction

The purpose of the conference was to take stock of the progress being made in relation to the problem of assimilating a mixture of synoptic and asynoptic data, particularly in relation to the first GARP Global Experiment (FGGE).

It is a difficult matter to present a definitive account of a conference which consisted of a stream of highly technical papers dealing with the very frontiers of a difficult area, so I shall not attempt to do this. It matters little as the proceedings are to be published quickly, early in 1976 so I understand.

What follows is then a personal account. I relate impressions which have stuck in my mind and it should not be assumed that papers which do not get a mention are of no importance.

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2. The Papers

The conference opened with papers by Morel and Smith which were lengthy factual accounts of the observation system to be set up for the FGGE. The reader is referred to Met O 20 Technical Note II/57 for details. Morel stressed that it was vital that some workable real-time method of assimilating the multivariate synoptic and asynoptic data should be developed in time for the FGGE in 1978/79. The salient feature of Smith's presentation was his confidence that the Nimbus 6 and Tiros-N series would provide radiance soundings adequate for the resolution of the detailed structure of temperature fields. In particular he seemed to think that the 4.3 μm CO_2 channel would greatly reduce the effect of clouds on soundings.

First, I deal with a group of papers whose authors have tackled the main assimilation problem more or less comprehensively in the sense that they inserted data into realistically sophisticated models even though in some cases the studies were preliminary in nature. I feel that the papers by these authors afford the most reliable yard-stick of practical progress since the 1st Conference in Princetown, 1971. The preliminary studies of Lorenc and Cattle were well received. There were no serious criticisms. Their results were either in accordance with those of other workers or were felt to conform with intuitive expectations. I will say no more since the authors and their papers are available here in the Meteorological Office. Miyakoda described a very comprehensive set of experiments with a general circulation model. He had taken an NMC Hough function analysis, initialized and run forecasts. These forecasts were then used as a target to be beaten, as judged by comparison with a verifying analysis, by forecasts run with asynoptic data assimilation. It would take a document in itself to give an adequate account, but the outcome can be succinctly stated. He did not succeed in beating the target forecasts, and the variation which came closest (quite close in fact) was one which involved repeated insertion of the asynoptic data with a backward-forward assimilation process. In connection with the backward-forward assimilation, in one variation he had "reversed the Physics" where appropriate. Halem of GISS described in some detail experiments with a global model in which by using GISS retrieval (inversion) techniques a sufficient density of good temperature soundings was obtained even from VTPR data for them to be able to use a very crude analysis scheme in conjunction with forward marching assimilation. He stated flatly that the assimilation problem was as good as solved and presented results which appeared to support this view. This was all done in a flamboyant style. Some delegates were sceptical and some appeared not to take him seriously. However, see my further comments in section 3. Rutherford presented what appeared to be the most advanced piece of work in a practical sense. He described an actual operational system in which the assimilation of data is done in a 6 hour cycle using optimal interpolation of the wind, height, temperature and humidity data. The first three of these are coupled in the optimal interpolation by using modelled covariance functions determined either empirically on from the hydrostatic and geostrophic relationships. A striking feature of his system is that this optimal interpolation coupling is sufficient to render initialization unnecessary. However he does find it necessary to use a heavy damping to suppress gravity waves. McPherson described two sets of experiments with NMC's global model. Data was injected into the model every 6 hours. In the first set of experiments the data was assimilated by using the Hough function analysis. In the second a local interpolation procedure was used. McPherson indicated that the second method gave the better results and conjectured that this was because the injection of new and out of balance data locally in a global functional analysis scheme such as Flattery's Hough function analysis caused a very widespread inbalance over the whole domain.

Second, there was a group of papers which dealt with some aspect of the main problem in relation to the use of sophisticated operational or research models. In our operational practice if the height field produced by the analysis is non-elliptic in some locality then we make a local adjustment to render it elliptic. An alternative procedure described by Paegle involves altering the local divergence field instead, without modifying the height field. Paegle showed that the alteration required can be significant in both amount and area on occasions and that these occasions can be shown to be dynamically genuine and not due to analysis error. Not unnaturally the changed divergence field effects the subsequent forecasts, beneficially in the examples shown, although the benefit is lost after 24 hours. However this loss is quite likely to be due to the inability of models to generate or maintain a non-elliptic field. The NCAR model used by Paegle certainly cannot, so he states. Jones reported on his GATE analysis experiments using optimal interpolation plus a variational adjustment using the balance equation as a weak constraint. Haltiner provided a complementary paper which described a variational adjustment with the balance equation used as a strong constraint in such a manner as to take account of the greater reliability of height data relative to wind data in middle and high latitudes and vice versa in the tropics. He dealt at some length with problems which may arise if a global model then has to be run from fields obtained by interpolation from these variationally balanced fields or with a finite difference scheme which is not compatible with the one used in balancing. Hayden described experiments with the NMC global "spectral" model in which VTPR data were referred to different reference levels, in some runs the reference level being varied from scan to scan of the analysis process. He demonstrated that the choice of reference level effects the heights in the analysis quite markedly but that it makes little difference to the analysed winds. Schlatter described an analysis scheme which provides a simultaneous optimal interpolation of the three variables u, v, and h. The formal effect of this is to replace most scalar quantities in a standard optimal interpolation routine by 3x3 matrices. He claimed that doing this consistently improved the forecasts obtained from the NCAR global model, but he ran out of time and we did not see any results. Simmonds described data assimilation techniques with the GFDL spectral global model. The assimilation process seems to involve jumping from coefficient space to physical space and back again much as in the way nonlinear effects are handled in the spectral approach. The paper attracted some fire as many present seemed to think that data near the pole would inevitably be assimilated to a different degree to data near the equator. Simmonds denied this. In any event his results seemed neither more nor less convincing than most other authors'.

Third, there were a group of papers which in my mind are linked by the fact that they dealt with special effects or problems demonstrated in relation to simple models and/or simple sets of equations. Anthes presented work on a simple meso-scale model with assimilation done by repeated insertion of data. His results seemed to me to provide clear cut support for Cattle's finding that the wind is the most effective agent of adjustment. In addition Anthes showed that it is the rotational part of the wind which is particularly effective, the divergent part slowing down adjustment even if correct. Blumen presented a paper which began on a high philosophical note and developed into a demonstration with respect to the theory of a very simple model based on the conservation of potential vorticity that analysis by optimization with respect to a norm induced on the forecast error fields instead of the traditional l_p(p whatsoever) norms for the analysis error field itself could be shown to give better forecasts. His thunderous denunciation of those of us who have spent the years sinning in this respect was greatly appreciated. Sasaki presented a "noise freezing" technique in relation to a shallow water equations model. The technique consists of "modifying the values of the constants which appear in the primitive equations so that the phase velocity of the inertia-gravity waves is reduced down to the characteristic speed of the meteorological waves". His results were not very convincing and I have some comment to make on the theory in section 3. Talagrand presented some theoretical work on the question of dynamical redundancy amongst the variables in a backward-forward process, and Sundquist appears to have found a way of mitigating the unbalancing effects of the erroneous gradients which result from any simple interpolation from pressure to sigma coordinates. In terms of a global shallow water equations model Williamson presented a method of initialization by expanding the data into the normal modes of the linearized model. As I understand it, a judgment then has to be exercized to decide which modes correspond to acceptable dynamical entities and which correspond to gravity waves and noise with unacceptable amplitudes. The unacceptable modes are then eliminated and the data field reconstituted. In its reconstituted form it can serve as an initial field.

Finally, I mention a small group of papers which does not fit readily into one of the above groups. I presented my own paper which was mainly a terse account of the theory and practice of our operational orthogonal polynomial analysis system. It also contains a suggestion for achieving an optimal interpolation via a method different from and cheaper than Gandin's, and two other proposals which connect strongly with papers by Petersen and Flattery. In the middle of my paper there is a proposal for research into the possibility of arranging the analysis/forecast suite as a negative feedback system. Petersen presented a paper entirely devoted to this possibility. The terminology and notation of Petersen's paper are very different to mine but both are proposals for the implementation of some variant of a Kalman filter, a point specifically confirmed by Petersen in response to a question from the floor of the hall. Flattery proposed a unified system of inversion and analysis utilizing the vectors of a Karhunen-Loeve expansion of radiance data. It is clearly of the same genre as the possibility raised in the very last paragraph of my paper, following the condensed account of the unified approach to the inversion and analysis problem. Flattery's approach involved "back-inverting" sonde data to radiances, utilizing where necessary the K-L vectors from an appropriately large area of a forecast field as a background to help with the cloud problem, performing a spatial analysis in the radiance domain and then transforming back to the temperature domain.

3. Comments

From the point of view of being ready for FGGE I suppose that it is Rutherford's operational scheme that impresses. However there must be some reservations. In spite of being allowed about twice the presentation time of anybody else he still did not reach the end of his paper and we saw no results. Even if we had, the traditional specimen case result "chosen at random" and shown at the end of a paper would not really enable anybody to assess a matter as complex as this. Inevitably his use of heavy smoothing to control the gravity waves arouses misgivings. When his paper has been received here and studied, if the Office has a serious interest in it I would suggest that we send a couple of younger members of the staff to Canada for a week to make a detailed in situ assessment. Rutherford was not too certain but it appears to take about seven times as long as our present analysis scheme. A point arises here. It is evident that optimal interpolation is enjoying a considerable current vogue. It was used by many authors besides Rutherford. It is also clear to me that optimal interpolation has precious little to do with the assimilation problem, and it is getting used mainly because of the readily available formalism and software and because it is fashionable. I am not of the opinion that optimal interpolation is genuinely optimal even when it is used in circumstances where its necessary premises obtain. If the premises do not obtain then the case for using such a cumbersome and sensitive procedure disappears. Several authors, including Rutherford, showed a nervous apprehension on this point, for having announced their use of optimal interpolation they promptly disclaimed any optimality. This is an issue which should be squarely faced. Many scientists made remarks which implied a belief that a spin-off from their FGGE researches would be sophisticated operational analysis/forecast systems for the 1980s. Morel stressed that FGGE research must be directed towards real-time systems. However I have a feeling that the term "real-time" may mean different things to different people. It seems probable to me that workers who are mainly interested in the research aspects of FGGE may use the term simply to imply that one cycle of analysis and forecasting must be completed before the next main wave of data starts to arrive. On the other hand, an operationally orientated worker will be aware of the very close-knit and interlocking system of programs which make up an operational suite and in using the term "real-time" will have in mind the possibly unacceptably disruptive effect on commitments and schedules which a new and longer running module may have. The cost of being fashionable should be counted early to avoid trouble later.

Probably the most disturbing aspect of Miyakoda's work was that the backward-forward method gave the best result, and again it is the time-of-running implication that is worrying. Many present were sceptical about his "reversed Physics" but on this I would take the pragmatic view.

At Princeton in 1971 an intense rivalry between GISS and the American meteorologists was noticeable and it was still evident in Paris. Halem's paper got off to the accompaniment of a certain amount of clowning by the American meteorologists who did their best to convey to the rest of us that this was a comic interlude. Halem's extraordinary style of presentation did little to dispel this impression at first, but it was very noticeable that the longer he went on the more silent and serious the audience became. We have to sort out style from content. His opinion that GISS retrieval methods will virtually enable us to dispense with analysis as traditionally understood is something which only practice can prove or disprove. His contention that retrieval must, in any given system, be related to the model's forecast fields is a view which I favour despite the very obvious inertial pitfalls lurking in such a scheme. More on this later.

McPherson's assimilation experiments with the NMC Hough-function analysis were done with a black box left behind by Flattery when he returned to the USAF. The black box situation is not a sensible basis for a set of experiments of this kind, and the outcome has to be considered with this in mind.

Paegles work on non-elliptic analysis areas strikes a chord here in the Met Office. When developing and adjusting the current operational analysis scheme I noticed that the non-elliptic areas of an orthogonal polynomial analysis were often larger and more intense than those of the grid point analysis run on the same data. This seemed then to be a "disadvantage" of the orthogonal polynomial analysis scheme, as it triggered off an undue amount of activity in the following initialization program, and the adjustment of the analysis scheme was shaded accordingly with a view to reducing these areas. In the light of Paegles paper perhaps Met O 11 or Met O 2b should reconsider this aspect of the analysis.

It is noticeable that truly spectral models seem to have dropped out of the running somewhat, at any rate in relation to the assimilation problem. This may be more due to a prior commitment on the part of assimilators to finite-difference techniques rather than to any final scientific judgment. It is my view that choosing between spectral methods and finite difference technology is a matter of choosing one set of difficulties rather than another and that what happens next depends on the amount of ingenuity you can bring to bear. Even if the main objection to Simmonds' paper were valid, a remedy is available. It would be a matter of choosing base functions with an appropriate distribution of roots. I would say that those institutions which have the resources should keep up the spectral attack as an insurance premium.

Some of the papers in my third group in Section 2 above were also linked in my mind by a common difficulty. It is that I find it hard to translate from simple models and equations to the sort of sophisticated models met in operational practice. To give two examples - Sasaki's description of his noise-freezing technique, quoted above, could with some candour equally well be described as inserting fudge-factors into the equations and choosing them to give the best result. If it works who can complain. One of his tactics appeared to me to be equivalent to putting a fudge-factor into the hydrostatic equation. In the shallow water model you can see what happens, but what would happen if this was done in the 10-level model? Quite likely the model atmosphere would quietly and gradually disappear. Again, I would have no enthusiasm for challenging Blumen's basic philosophy as it sounds reasonable and he may well be right, but could it be done for the 10-level model?

Flattery's work was in a preliminary state. Characteristically, he made no great attempt to overcome the audience's scepticism. I think his justification for the use of Karhunen-Loeve vectors is full of holes, but I did not say so, probably because I favour his approach overall. The main criticism, pressed rather strongly by some, was based on a belief that the use of "back-inverted" sonde-date in a spatial analysis of radiance data would result effectively in the degradation of the sonde data rather than the improvement of the radiance field. I feel that Flattery could have done more than he did to allay misgivings on this point, but it is likely that he intends to press on and do it and leave the results to speak for themselves.

4. Conclusions

In terms of a real and genuine understanding of the problems involved in assimilation I think the general conclusion was that we still have most of the way to go and that it will be a long haul. As for the FGGE, it is fearfully imminent in relation to our state of readiness. I think the meteorological world will be in a position where it will have to accept gratefully whatever is available even if it is not really good enough. At the moment, subject to verification of his results, it looks like being Rutherford's system that will be ready for use.

Looking beyond FGGE to the 1980s I think that the inversion/assimilation/analysis/ forecast system we should aim for is a very unified and self-contained one, tailored around whatever model we are using. The model forecast fields should be used in those parts of the system which deal with inversion, assimilation and analysis. The inertia problems thus engendered should be tackled and overcome and as this is done the whole system will tighten up. In this connection I make a plea for some relaxation of the rigidly deterministic approach which has prevailed so far. The tightening up process would be greatly assisted by the implementation of a feed-back process which partially swept out the systematic errors which will quite unavoidably arise from imperfections in the inversion, assimilation, analysis and model formulations. I stress that this last paragraph is a personal view.