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Some ideas for future work-items in preparing for GOCE.

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The end of the E2M project raises the question of which new work must be done and how it should be organized. Below are some of the items I have noted when going through the work-packages of Team 2 (the space-wise approach).

Scientific and computational issues.

In general the work to be done is to assure that the whole processing chain will function. This can be done in advance using data from the GOCE End-to-End Simulator (I hope). However also other data sources are available such as SST data from Ørsted and GPS-Met, and soon CHAMP. We also have airborne gravity and gradiometry, which can be used to verify the software and the procedures. (I have done this in a number of papers with Rene Forsberg and Demitris Arabelos over the last years).

But here comes some of the specific items I have noted:

WP 1:

Several times it has been mentioned that a good DTM is used. Meanwhile I have become very doubtfull of the quality of these DTM's, and I am afraid that there use will create more trouble than something positive. The intention was to use the DTM in a remove-restore procedure to enhance local gravity field variations. Then we could easier identify if a gross-error really was a strong local gravity field variation. This is still a possibility.

But for the remove-restore procedure I think there exclusively should be used the best possible gravity field model. Such models should "contineously" be produced during the lifetime of GOCE. Only on Antarctica, at DTM might be useful, but then it should include depth-to-rock information. And it is here we should use GOCE to get this information ...

Another item is the noise-modelling. We may come up with noise models by asking the instrument people. But who knows how the instruments behave in space ? I have used a procedure sucesfully where gradiometer data were upward-continued to flight altitude, and the differences were then used to estimate a noise-covariance function. This procedure should be tried on simulated GOCE data - i.e. can we in this way recover the simulated noise we have put in.

WP 2:

In theory and in some realistic examples it has been shown that using the non-isotropic spatial signal covariance function we get an improved result. But is it worthwhile to try this ? It is a rather complicated story, still with some theoretical problems. Is it a better strategy to homogenize everything, by subtracting out all the time everything we know ? At least the experience with using LSC for coefficient prediction indicates that this is a good idea. Furthermore, if we work with residuals, the stochastic model changes all the time. (Hopefully the variances get smaller and smaller all the time !).

If we use isotropic covariance functions, should we then try other models than the T/R model. (I hope everybody will stop using "Kaula", at least). There are models by Jekeli and Moritz which should be tested.

In the space-wise approaches some types of "normal" values should be used to reduce the number of equations/integrations used. But which values should be used ? Point or mean values ? Gravity disturbances or vertical gravity gradients ?

Prototypes exist implementing the integration approaches (harmonic and Slepian). They should be finalized. (I hope I am right here, Alberta ?).

Having worked with Ørsted data, I have come in doubt how easy it is to work with double-differenced "accelerations" derived from GPS/GLONAS phase data. Do we need a higher data rate that the 1 s specified now. (10 HZ is readily available). Could we directly get better velocities ? This should be studied.

With all the different data types, observations may be so strongly correlated that numerical singularities could occur. If the data are converted to "normal values" the problem is solved, but then we do not extract all information in an optimal manner. This must be investigated.

Then use of sparse preconditioners is a way out of ~~the problem of~~ the difficult problem of solving a very large amount of equations. But does the sparseness have some consequences for the recovery of long-wavelength information. I do not believe that, but I am not sure. This could also be tested by simulations, if no theoretical proof can be made.

WP 3:

The CGM/SP has given very promising results. But will numerical problems occur with very large datasets ?

In many cases spherical approximation is used. If we only work with space data, this is not needed. But if we also include data at the Earth's surface spherical approximation is generally used, because the statistical properties of isotropy are most easily represented this way. Should ellipsoidal harmonics be used ? If we cut the covariance functions to some high degree, then there should be no problem here (the problem being that no closed expressions have been found for covariances expressed as infinite series in ellipsoidal harmonics).

The problem of using SST phase data has been touched upon above. It is possible to avoid double-differences but then covariances must be integrated twice along track. This is not a difficult procedure, but probably rather time consuming. But it should be tried.

I hope somebody will go through the software standard proposed. It requires certainly more discussion.

WP 4:

Several different procedures have been proposed for "data-screening" or outlier detection. Should we use downweighting or removal of bad data ? Which downweight function should be used. The different strategies should be tested.

Some simple covariance models have been proposed to be used in outlier detection. They seem to be very efficient and fast. But they should be compared with detection procedures using other covariance functions.

Software capable of detecting outliers using several components of the measurements should be developed. The program **geogridx** must be upgraded.

The identification of systematic errors is still a difficult problem. A successful test has been made by D.Arabelos and myself using a smooth area in Canada. This area might not be the best, and we should look for other areas. (The problem is that mountains were quite close).

WP 5:

The collection of gravity in the Arctic will be finished by the end of this year. But Antarctica is a big problem. Should we ask IAG to initiate a stronger coordinated effort ?

Organisatorial issues.

It has been proposed that two data-centers should be established, which should be working independently with the data. One could think of a team working with the time-wise approach and another with the space-wise approach, for example.

There is however no doubt that the "space-wise" team is much better prepared, and already have come a long way. They have also very carefully looked into data preprocessing and error-modelling.

Furthermore I have been lacking a stronger interaction between the "teams". There has virtually been no discussion. But with the very comprehensive E2M report finished, there should be an excellent basis for discussions. A number of workshops should be organized around GOCE. The GRACE and CHAMP people might also be useful here, but we must be careful that this does not become a big show as when E. comes with all hes admirers. There is really a need for that we - the real experts - comes to a better understranding of what is required.

We also need new people to be educated.. PhD-schools could be attached to the above proposed workshops. Again the E2M report is a fantastic teaching material.

Finally there is the question of wether we should bring other people in. There are capable groups around like Fredens group, and groups in Eastern-Europe (maybe). I must say that do not think it is necessary. (But I might be very wrong here).

Another type of people are those who should prepare for applications of GOCE data. Here the MAG must work on getting preparations going by advertising GOCE. A number of fine studies of GOCE applications already made will help here.