WCRP Community-wide Consultation on Model Evaluation and Improvement

Please complete the following template by writing your answers into the box below the questions, and then submit your response electronically by ***15 September 2009*** to Anna Pirani at apirani@princeton.edu.

Q1: Please state your particular area of interest, e.g. global and regional NWP, seasonal prediction, sea-ice feedbacks, monsoons, troposphere-stratosphere exchanges, etc. Given your interest, what would you consider/identify as the KEY uncertainties/deficiencies/problems of current models? (Give references and/or one key figure where possible)

MPI-M pursues research in Earth system dynamics. Most studies are related to timescales of the past century to millennium and climate projections for the 21st century. Consequently many studies make use of coupled atmosphere land ocean models. Current research is devoted to process modelling, internal variability, and climate sensitivity and feedbacks.

Systematic biases observed in single components (e.g. insufficient marine stratocumulus in AMIP simulations) or in the coupled model system (e.g. double ITCZ) are major concerns. Such errors typically result from an insufficient representation of unresolved processes, and may lead to biased feedbacks (e.g.: systematic bias in marine stratus → bias in aerosol cloud interaction). Systematic errors in ESMs are also an obstacle for decadal climate prediction.

Beside difficulties in modelling the mean state of the atmosphere, ocean or coupled system, also the natural variability is often misrepresented or underestimated, starting from the diurnal cycle in convection. Sometimes it seems nearly impossible to model the mean and the variability "correctly" at the same time. Phenomena that depend directly or indirectly on waves are hard to model (MJO, QBO, El Nino).

On longer timescales, or in a forced climate system, processes may be important that are not yet described with sufficient detail or are missing in our Earth system model. Examples are the thermodynamics and chemistry of soils in high latitudes (permafrost, CH_4), or the dynamics of shelf ice or ice shields.

Q2: What do you think should be evaluated/improved as a priority in models in terms of parameterization and/or interactions among processes? (The answer to this question may differ from the previous one)

It might be very beneficial to set a focus on cloud processes and the dynamics of tropical convectively coupled waves. This should result in an improved ability to simulate a realistic tropical wave spectrum, from small scale gravity waves near the truncation limit of a model to the largest scale equatorial waves and the MJO.

Q3: For this purpose, do you see a particular gap (in knowledge, in observations or in practice) that would need to be filled, or a particular connection between the different modeling communities (or between modeling, process studies and observations) that should be made a priority?

At the process level, better quantification of critical variables like cloud liquid and ice paths would be important to constrain better the cloud parameterizations and hence the radiation budget.

Climate model evaluation can enormously profit from extended analyses of the 20th century climate. ERA-75 will make an important contribution to the atmospheric side.

Ocean analyses are less developed, and this is very likely an area where more developments will have an important impact. (And coupled analyses, for atmosphere ocean and land are still to be developed.) Concerning ocean analysis, it will be important to further develop the ARGO system until it becomes an operational observing system.

Q4: Do you see any particular resource or opportunity within WCRP and CAS (e.g. data from an available campaign or satellite, a particular initiative already launched or to be launched) that would be particularly useful and should be exploited to tackle this problem? Do you have suggestions for new initiatives?

The WCRP/IGBP initiative on aerosol, clouds, precipitation and climate, with its regime-based emphasis is a good start as it provides a vertically integrated project that connects key climate ideas. Quite likely climate model development could profit from a better integration of tools and models with NWP (e.g. AMIP-transpose, seasonal forecasting exercises).

Q5: What would best accelerate progress on the topics raised in questions 1-4?

See below

Q6: Any other suggestions/issues to be raised?

There is a tendency to think that climate research has moved from fundamental to applications. This is a misconception. Climate modeling relies disproportionately on our fundamental understanding as unlike whether forecasting we can not rest on a record of making past forecasts. WCRP can do a great service to the community by emphasizing the degree to which climate modeling rests on fundamental understanding, and in the end advances no faster than the understanding itself. This again (and for the same reason) is different than NWP. Given that most climate models still grossly misrepresent energy balances on sub-planetary scales, the more so the smaller the scale, WCRP should emphasize an evaluation of regional energy biases and their implications for predictions on sub-global scales. I would further note that most of these biases are unrelated to processes that introduce complexity into the system on longer-timescales. The key issues are old ones: clouds and ocean circulation. There is no way around these difficult problems.