

# Report to the Astronomy and Astrophysics Advisory Committee on Theory and Computation Networks

Mordecai- Mark Mac Low and Gregory Laughlin

## I. Introduction

*New Worlds, New Horizons* made a recommendation that NSF, NASA, and the DOE cooperate to fund networks in theoretical and computational astrophysics. We were requested by the AAAC at their meeting of 13-14 October 2011 to consult with the community to recommend the desirable parameters of such a program.

Our interlocutors at NSF, Tom Statler, and NASA, Linda Sparke, informed us that DOE was uninterested in participation, as they already provide substantial resources (through their INCITE program) in support of large computational astrophysics collaborations. It was further suggested that a likely level of support for this program was on the order of \$2M per year.

Specific questions that we were requested to respond to were:

- What constitutes a network?
- What distinguishes interdisciplinary activities?
- Should the program be restricted to certain key questions?
- What are the needs for a TCN program, e.g. with respect to theory/computation balance and workforce initiatives?

## II. Consultations

Initially, the idea of holding a workshop to enable effective consultation with the community was discussed. However, after due consideration, it was decided that the time and expense could not be justified, and instead individual consultations in person and by electronic means, combined with a group telecon that included designated Federal officials Statler and Sparke, were substituted. The Appendix lists everyone consulted.

## III. Existing Models

1. Germany: *Schwerpunktprogramm (SPP; Special Priority Program)* These DFG funded programs were referred to in the original *NWNH* recommendation

as one model of a successful network that enables a large, coordinated theory effort. They provide coherent funding for a field of research across Germany at an annual level of roughly €2-2.5M before overhead through a coordinated program of relatively small grants (€50-100K/yr).

The intent is to identify research topics that are emerging and have high potential, but are nevertheless fragmented within a larger research community. Collaboration is encouraged through pre-proposal workshops that draw together workers in a field to come up with a plan that is expressed in the framework SPP proposal, and substantial funding (> €100K/yr) for workshops, visits, outside visitors, etc. Participation in the process is further encouraged because the DFG will not make grants in the field of the SPP outside of the SPP review.

SPPs run for six years, with two or three requests for proposals during that period. Proposal reviews are performed by an international panel suggested by the framework PIs, and chosen by the program officer. The review includes a poster session with the proposers and the panel. Typically the DFG only supports one SPP at a time in astronomy. Currently it is supporting "Physics of the Interstellar Medium".

This model has the advantage, but also the disadvantage, that a great deal of structure is built in. The process can become bureaucratic, with many planning meetings, extended proposal reviews and such. These can push collaborators together, or just impede everyone's work.

DOE INCITE and old NSF Grand Challenge grants are or were of similar scale, but more structured, with a focus on a small number of PIs leading a large effort, rather than a set of coordinated but independent research projects proceeding in parallel. NSF Physics Frontier Centers and Science & Technology Centers are also same scale; sometimes involve so many people that most participants don't get significant funding.

2. *NASA Astrophysical Theory Centers* These were smaller, more focused groups with PIs at perhaps three institutions, and roughly 0.5M/yr funding (in 2012 dollars). Perhaps 3 grad students, 1-2 postdocs, and some salary support for extended visits by more senior people, as well as funding for frequent meetings.

Monthly meetings encouraged new collaborations. Geographical proximity allowed in-person meetings. These had the big advantage over telecons that side conversations in the corridors occurred naturally. Such

spontaneous interactions are where collaborations are initiated and move forward.

The Theory Centers were originally chosen by NASA HQ staff. They did not fare well when forced into direct competition against more focused individual investigator grants. In retrospect, however, they did foster valuable interactions.

## IV. Discussion

### A. Scale

*NWNH* discusses the two models that are described in Section III as paradigms for the recommended program. Both are held up as successful examples of research networks. At the original recommendation of a total budget of roughly \$9.5M per year in this program, either would have potentially been a viable model. However, under current budget constraints, the larger scale networks analogous to an SPP or other major European networks would require making only one award per funding period, which would defeat the purpose of offering strategic support to the range of top priority topics identified by the Science Frontier Panels. Community input has further revealed substantial enthusiasm for the model of a medium-scale, multi-institutional network that expands collaborations beyond the bounds of a single institution or a single field, but does not grow so large as to generate overwhelming administrative overhead. Therefore we recommend awards of size comparable (in current dollars) to NASA Theory Centers, that is of order \$1.5M over three years, allowing multiple awards per cycle.

Timing is another critical issue. Five-year grants were recommended in *NWNH*. If no further funding were available in this program, however, this would lead to only two rounds of grants per decade, which seems insufficiently flexible to meet the rapid pace of development of the field. However, even if this program cannot maintain strategic programs over the long term because of its constrained funding, it can still fill a unique role by providing startup support for new collaborative networks by funding them for three-year grant periods.

### B. Workforce

Community input emphasizes that participation in research networks is particularly valuable for graduate students, as it introduces them quickly to a broad range of ideas and a larger group of potential collaborators. Another major issue identified is the relatively limited amount of support for the development of computational methods and tools, in comparison to the support available for applying them. This is true at both junior and senior levels. Support for both groups should be encouraged, but not required, as the design of each program will inevitably lead to different needs.

### C. Fields of Study

As described in NWNH, this program is intended to focus on the areas of greatest potential for progress, such as those identified by the Decadal Survey. However, we should not limit ourselves just to those subjects identified as high priority questions by the Science Review Panels, as that will not give the necessary balance of focus and flexibility.

### V. Evaluation Criteria

In order to maximize the return from this program, the effectiveness of implementation of proposed networks will need to be explicitly evaluated. This means an extension beyond the usual criteria for evaluation used by the NSF of scientific merit and broader impact; and by NASA of scientific merit and programmatic relevance. Specific aspects that should be called out include:

- *Justification for importance of collaboration.* There are many different configurations that could drive important advances. These include supporting the interplay of analytic and numerical modeling; support of theoretical studies by observation or experiment (or both); work combining elements of multiple disciplines; and multi-institutional development and deployment of a widely useful software platform.
- *The structures of collaboration.* The mechanisms for promotion of collaboration both among members of the group and with others in the field. The opportunity for exposure to the broader field through such a program can extend student horizons, and provide new research opportunities for senior members. Some possible mechanisms include internal workshops, funding for inter-institutional visits and outside visitors, and technical support such as code dissemination and documentation.
- *Impact on the field.* The case must be made that the collaborative activity will lead to a solution of a problem central to answering questions of priority equivalent to those identified in New Worlds New Horizons (Table A.1 gives the summary of these questions.) This will ensure focused effort on questions identified by the community as highest priority.

### VI. Queries

In response to the specific queries posed:

- *What constitutes a network?* A group of at least three PIs from three institutions working on a common problem with a variety of approaches.
- *What distinguishes interdisciplinary activities?* Interdisciplinary activities were not called out in the *NWNH* description of this program. A compelling network proposal will likely be composed of work in multiple disciplines, but interdisciplinarity as such need not be explicitly evaluated.
- *Should the program be restricted to certain key questions?* It should not be limited to the priority questions identified by the Science Review Panels. These are examples that do cover a broad range but restricting ourselves to them do not give required flexibility. They should be treated as examples that can help ensure focus on high-priority items.
- *What are the needs for a TCN program, e.g. with respect to theory/computation balance and workforce initiatives?* The balance between theory and computation can and should be left to the proposers to set as necessary to design a compelling proposal. Encouragement of support for students and numericists engaged in code development will contribute to the community's ability to address questions requiring computation and data analysis expertise.

## Appendix. List of Individuals Consulted

\* participants in the group telecon

+ people who both participated and spoke with us individually

Dr. Brandon Allgood (Numerate, Inc.)

Prof. John Blondin (N. Carolina State U.)

Prof. Eric Blackman (U. Rochester)

Asst. Prof. Sukanya Chakrabarti+ (Florida Atlantic U.)

Prof. Shu-Ichiro Inutsuka (Nagoya U., Japan)

Assoc. Prof. Kathryn Johnston (Columbia U.)

Prof. Ralf Klessen (Dir., Inst. Theor. Astroph., Uni. Heidelberg)

Asst. Prof. Mark Krumholz (UCSC)

Dr. Dieter Krückeberg (Program Officer, DFG)

Prof. Christopher McKee (U. California, Berkeley)

Prof. Steven MacMillan (Drexel U.)

Prof. Michael Norman\* (Dir., San Diego Supercomp. Ctr., UCSD)

Dr. Jeffrey Oishi+ (Postdoc, Stanford U.)

Dr. Edward Seidel (Asst. Director, MPS, NSF)

Prof. David Spergel\* (Princeton U.)

Prof. Frank Timmes (Arizona State U.)

Dr. Stephanie Tonnesen (Postdoc, Princeton U.)

Asst. Prof. John Wise+ (Georgia Tech)

Prof. Ellen Zweibel+ (U. Wisconsin, Madison)