Minutes of OTC telecon, Thursday, May 10th 2007, 15:30 UTC.

Last revised 2007-05-11, DTE.

OTC members present: Walter Brisken, Bill Cotton, Darrel Emerson (chair), Rick Fisher, Tony Kerr, Rich Lacasse, Matt Morgan, Peter Napier, Roger Norrod, John Payne, Marian Pospieszalski, Art Symmes,, Dick Thompson & John Webber.

Ken Kellermann also attended. Apologies were received in advance from Rich Bradley.
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Agenda:

1. **How to package an R&D program**

2. **AOB**

1. How to package an R&D program that is attractive to the Director, to our scientists and to NSF.

Prior to the meeting, Rick Fisher had distributed the following to the OTC:

OTC topic for discussion from Rick Fisher:

I've been trying to figure out how to package an R&D program that is attractive to the Director, to our scientists and to the NSF. For all of it merits, the OTC Technology Development document didn't get much traction, I think, partly because it was a bit too much of a shopping list and partly because it never made a strong science connection.

Listening to the discussions on how to spend the Lockheed Martin reimbursement I heard the message that a lot of scientists want the benefits of a beam-forming array, but no one was willing to propose the development because it extended beyond a three-year horizon, and there was too much risk in terms of how long and how much money it would take. Nevertheless, I heard many times that this is what people really want. In the course of one discussion, John Webber threw out an off-the-cuff the estimate of 10 years and $10M to develop a low-noise BFA. This may be a little pessimistic, but it's only 2% of our operating budget. If BFA is truly revolutionary, it seems like a bargain and even a reasonable time frame. The trick, it seems to me, is to declare a realistic goal for this investment - something that's scientifically exciting. Maybe, 100 beams, 30% bandwidth, Tsys < 20K.

Related to this is my sense that an R&D program is going to tinker along very inefficiently until we establish a critical mass group *whose absolute highest priority is to do advanced development. $1M/yr would just about do it - 4 or 5 full-time research engineers, three techs and $200K M&S. The important elements are full-time engagement of the RE's, clear objectives, and a sense of urgency.*

One objective may not be enough, but another objective would cost more money. A second one that comes to mind is a single-beam, 1 to 30 GHz, Tsys < 20K receiver package (feeds through A/D's) that is small and light enough to fit on a 6-meter dish. This would be our part of SKA and a factor of 16 greater field of view compared to a 25-meter dish. Arguably, we have the best receiver engineers in the world so it seems to make sense that we adopt this objective. This, too, is in the 10 years and $10M category, which fits a realistic SKA development time scale. There is some overlap in technology with the BFA development, but I'd be very cautious about promising associated cost savings. When the synthesis array signal processing becomes affordable, one or more beam-forming arrays could be added to the smaller dishes for still greater field of view in a limited frequency range.

None of this addresses the need for mm-wave R&D, which clearly needs to be sold to the ALMA operations program. That's another front to be opened separately. There are also a lot of cm-wave politics to be dealt with in terms of university collaboration, international SKA, etc., but those are beyond the scope of an OTC discussion. Is there another objective that would be a big advance in cm-wave capability that we should consider?

Discussion:

Rick introduced the topic. The plan is to give Fred & Jim some specific theme for future R&D, which would also be relevant to the SKA and attractive to the AUI and NSF. What are we planning for long term development?

The idea is that we need a focus, not just a list of independent project. The frequency range of interest overlaps with the SKA. It should be saleable on the basis of science. What are the areas of technological parameter space? Bandwidth, frequency range, field of view, receiver sensitivity and collecting area. Collecting area equates directly with available funding. The only significant parameter that has not yet reached a practical limit is field of view.

There are two possible approaches to field of view: a large number of small dishes in a synthesis array, or focal plane arrays either of individual pixels (FPA), or beam forming arrays (BFA). The BFA allows closer (overlapping) beams, about 16 times as many in area as with a typical FPA, and also potentially allows for correction of aberrations, and so allows a large area of the focal plane to be used. An array of smaller dishes might cover 1-30 GHz.

The purpose of the theme is to make it saleable to NSF, Fred and to AUI. What are the technical issues to be solved. This is a 10-year plan. Note that a noise temperature of 50

K would not be acceptable, each synthesized feed should have 20 K or lower noise temperature in order to be competitive.

Over 10 years, we might need 10 M$ for development. The BFA project would be independent of that for a large number of smaller dishes. What is the critical mass of manpower? Would 4 full-time research engineers over 10 years be sufficient?

Ideally both projects, the BFA and a large array of small dishes, would be presented and funded.

Peter commented that this is a very good goal. We need BFA for the SKA. But, it is difficult and expensive. In today's climate big construction projects may now *require* international collaboration. For example, the AT is now on its 3rd generation of FPA. The key thing is that it's well funded, with lots of people, and includes international (Canadian) collaboration. Peter wonders if we really should try to go it alone, or should we seek a partner. The Australians are concentrating on lower frequencies, 700-1700 MHz. Collaboration could involve NRAO adding cryogenics to the AT design, so improving sensitivity.

Tony asked if the AT effort was for a BFA? Peter confirmed, yes. They fully sample the aperture, with what they call "Smart Feeds."

Darrel asked about the relevance to the VLA, or would this just be in the context of the GBT? Rick replied that it would be an open-ended project, starting with the GBT. Peter reminded us that there was an early BFA proposal as part of the Phase II proposal for expanding the VLA, but which was removed from the final proposal.

Bill Cotton commented on the problem of parallactic angle rotation, which might require a physical field rotator. Ken commented that that was an issue now with the VLA, given the sidelobe patterns.

John Webber asked about the specific scientific motivation for a particular field of view. For a specific science goal, might a small array be more practical. We need a tradeoff of science needs versus cost for different approaches. Maybe the concept of a general purpose instrument is no longer appropriate. A mission-oriented approach might lead us to a small cheap array in some cases.

Peter added that we want new technology development that could then be applied to many different projects. We're developing a technology, not a particular instrument.

John commented that a long-term project is very unlikely to get funding if it is not specific to a particular goal. Bill remarked that the SKA is an umbrella for funding of projects of this sought.

John repeated that the funding prospects are very grim. This project is unlikely to come out of the regular NRAO operations budget.

Dick Thompson made the point that an array of small dishes may well not be funded sufficiently well to allow an interesting number of dishes, while a single BFA specifically for the existing GBT may have more chance.

Marian reminded us that a project of many small dishes would essentially be the ATA. For us to continue without treating the existing ATA as a prototype would be ridiculous. For planning, we have to make a good case based on the existing, real example. For an FPA, as opposed to a BFA, the technology is already there. An analog BFA would not be feasible, it needs to be a digital approach to a BFA.

John added that we need a scientific test case, such as in the 700-1700 MHz band, where we could take advantage of work already undertaken elsewhere. It is however very hard to estimate cost from what we know now.

Rick thought that we should take the independent pixel (FPA) concept out of the argument. We already know how to do that. John commented that the technology for a single-pixel array at 10-30 cm might be very different from that for Q-band array.

Marian asked what are the technological barriers? For feeds, there is a huge amount of work needed. We should learn from past experiences in looking to the future.

Peter commented that the Radio-Astronomy-specific FPA or BFA problems may be quite different from interests of industry. For example, mutual coupling of signal and noise between feeds. John commented that it might be quite difficult to model a system, limited by computer power; he gave the example of earlier correlator simulations.

Rick said that the current aim is to take thoughts to Fred and to Jim, to see what resistance or support there is for these concepts.

2. AOB:

John Webber asked for advice on his last viewgraph of a presentation to the Visiting Committee, on long term R&D plans. Relatively little feedback had been received from the scientists on earlier OTC proposals, although scientist comment on the recent request for LM fund proposals had produced useful results. Rick suggested that lower noise and greater fields of view should be the main emphasis. Sub-mm development in the context of ALMA, and FPGA developments should be included. John thanks the OTC for the comments and suggestions.

DTE, 2007-05-11