Next Generation Radio Arrays

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SUMMARY

This document presents an approach to SKA, LOFAR, EVLA and ATA which places them all under the coordinating umbrella of a "new technology arrays", or NTA program. Such a program is presented as a unifying concept that can be used to strengthen proposals to the NSF, and as a guiding framework to help define design goals for multiple instruments. Some pitfalls of the current approaches are pointed out, constructive alternative suggestions are made, and the first steps toward implementation are briefly outlined.

Introduction

Soon after the Decadal Report for Astronomy and Astrophysics, we face what is arguably a defining moment in U.S. radio astronomy. Gratifyingly, three significant meter-centimeter radio astronomy projects were endorsed by the Report, specifically EVLA, LOFAR, and SKA technology development. EVLA was considered a "large" ground-based project, while the LOFAR and SKA initiatives fall into the "small" and "medium" categories, respectively. Endorsement by the Report, however, is far from a guarantee of funding, and as will be argued below, the current level of coordination and planning between our various efforts could be improved. As a result, the U.S. radio astronomy community risks squandering what should be a golden opportunity to reinvigorate the discipline. What we do now sets the tone for the rest of the decade. This document is an attempt to analyze the issues, and to suggest a course of action which will best serve our collective goals.

Background

SKA: The Square Kilometer Array (SKA) project is young in the U.S., having been born in a flurry of activity during the deliberations of the Decadal Report committees. It was a response to the gathering international momentum of the project, and was driven by the realization that very large improvements in radio telescope array sensitivity per dollar were within reach, thanks to cheap digital electronics and data transport technologies. In particular, the founding members of the U.S. SKA consortium embraced the concept of an array comprising a large number of stations, each of which was a phased array of small, cheap antennas. The many advantages of such an architecture have been extensively documented at meetings and in conference proceedings.

The challenges posed by the design of such an array are formidable. Achieving a full square kilometer of collecting area, particularly at high frequencies, may prove simply too costly. Many technical problems not faced by conventional arrays would need to be solved, and any hope of success would depend on harnessing of much of the best radio astronomy talent available.

EVLA: The EVLA project is a two-phase NRAO project, targeted for completion in this decade. The first phase replaces the VLA electronics, achieving broader frequency coverage and wider bandwidths, opening several new science areas and boosting continuum sensitivity by a factor of 10. The second phase, as currently conceived, involves the addition of up to 8 VLA/VLBA-style 25-meter dishes to fill the gap between VLA and VLBA baseline lengths. At present, both phases are planned to proceed concurrently, with completion late in the decade. The phase 1 proposal has already been submitted to NSF, and the phase 2 proposal is intended for submission later this year.

Few will argue with the scientific value of phase 1, which represents a major improvement of the world's premier radio array. The upgrade is long overdue, and effectively leverages a lot of existing, expensive hardware. The benefits of phase 2 are less obvious, for two reasons. First, the e-Merlin instrument in the U.K may provide much of the phase 2 capability. Second, at ~$6M per fully equipped dish, phase 2 collecting area is very costly, in excess of $10,000 per square meter. For comparison, the SKA target is $600 per square meter, and the ATA project will achieve something in the neighborhood of $2,000 per square meter, fully equipped.

Two other key points should be noted about EVLA. First, the higher frequencies are important to EVLA science, with pushes to 43 and 86 GHz for continuity with ALMA, and second, the extraordinary bandwidths to be supported by EVLA can be utilized only at the highest frequencies.

LOFAR: Finally, we have LOFAR, which is designed to open up a new spectral window to high resolution and sensitivity. The basic LOFAR antennas are dipoles, which are extremely cheap. The raw collecting area for LOFAR will cost less than $40 per square meter, even at the highest frequency of 240 MHz, a figure that precision paraboloids cannot approach. In addition, the dipoles are extremely wide field antennas, a fact LOFAR exploits by digitizing the signal from each dipole. Most of the cost of LOFAR is in the digital electronics that will pioneer new levels of array flexibility and versatility. This is an exciting, but far from risk-free, initiative. A particular area of redundancy with SKA as currently defined and justified is that LOFAR will be designed to be able to effectively observe the HI signature from the epoch of reionization (EOR). This science topic has been identified as the single most important science driver for the SKA.

Project Relationships

How do these projects relate to each other, and how coherent is the overall U.S. radio astronomy effort? Poorly, and not very, are the honest answers. Let us state clearly what problems we face in the current state of affairs.

The internationally agreed SKA timetable, embraced by the US SKA consortium, calls for site and concept selection by 2005. Outside the U.S., most groups are targeting late in this decade for the start of construction, with completion in the 2012-2015 timeframe. In the U.S., this schedule conflicts with that of the EVLA, in the sense that it is hard to see how NSF can countenance funding construction of the successor to EVLA before the latter is complete, or even for several years after it becomes operational. Since the EVLA is ready to proceed, aggressive early pushes for SKA funding, based on current SKA and EVLA concepts, appear optimistic.

LOFAR is intended, in part, to address the EOR science, with specific design modifications to that end. LOFAR is also ready to go, with imminent funding from ASTRON and NRL, and a great deal of momentum. Staking the SKA scientific case too heavily on EOR may thus be risky, given that NSF is aware of the EOR capabilities planned for LOFAR. At present, the US SKA consortium may not be putting its best foot forward in this regard, by providing NSF with reasons to downgrade SKA priority.

The combined effect of this type of perceived competition between SKA on the one hand, and EVLA and LOFAR on the other is potentially strongly negative. We run the real risk of being denied SKA capabilities for 20 years, which is bad for US radio astronomy, bad for global radio astronomy, and bad for astronomy as a whole.

A Different Approach

What is needed is a redefinition of the various projects in such a manner that they can be justifiably perceived as coordinated and complementary elements of a larger enterprise. That larger enterprise should be to **bring the benefits of innovative new technologies and techniques to radio astronomy in the shortest time possible, and with the greatest scientific impact possible, within a reasonable funding profile**. For convenience, let us label the umbrella enterprise the "new technology arrays", or **NTA**.

So, how do we put all these pieces together? The first and most important problem to address is that EVLA will push back major funding for SKA, probably to the point that construction would be hard to justify before 2015. Therefore, we should try to ensure that EVLA activity becomes **part of NTA**, and will help lead to the capabilities now touted for SKA. Second, we must think of new arrays, like LOFAR and ATA, as **integral parts of the NTA enterprise**, rather than as mere stepping stones to the SKA holy grail. Third, the US SKA Consortium position as presented to the NSF needs to acknowledge that the project endorsed by the Decadal Report is one of technology development, which need not be tightly coupled to current concepts of the SKA as an all-encompassing replacement for all existing arrays. Instead, it should embrace a **variety of possible scenarios** for deployment of the technology to be developed, including unique roles for LOFAR, ATA and EVLA, which can be complementary to any instrument we eventually call the "SKA".

Implications and Specific Suggestions

Let's take these three items in order, with some concrete, hopefully constructive, suggestions. First, the EVLA/SKA conflict:

Rather than replicating older technology for building collecting area, EVLA phase 2 offers the opportunity to spearhead the concept of phased array stations in a major imaging interferometer. Let us assume that collecting area can be built for EVLA phase 2 at a cost comparable with that of ATA, using small paraboloids. Continuing progress and economies of scale will likely permit operation of such array stations to target EVLA frequencies of 43 or 86 GHz without excessive cost escalation. For currently projected phase 2 costs, one could envisage a target of **doubling or tripling the VLA collecting area** using such technology. Array stations could be placed at multiple locations along already planned EVLA fiber runs, creating an imaging array of far greater power and versatility than that currently planned, though impacts on correlator design would have to be assessed. Such a phase 2 approach would lead to an instrument primed for further enhancement through the addition of more dishes to each station, and by the addition of more stations. The EVLA would carve out and maintain a niche at short centimeter and millimeter wavelengths, precisely the spectral region in which construction of a full square kilometer of collecting area appears somewhat optimistic.

Note a few points regarding this idea. The relatively small-N of the EVLA is less of a limitation at high frequencies, as the sky is less heavily populated and required dynamic ranges are smaller. The idea of concentrating on higher frequencies maximally leverages the extremely wide bandwidth specification of EVLA. NRAO is well positioned to address the challenging problems of imaging using phased array stations, and will become directly engaged in solving key NTA data handling and analysis problems. This may be the long-awaited "killer-app" for AIPS++ and the measurement equation formalism. The current EVLA phase 2 plan would require major modification, and an increase in risk, but with a major increase in scientific payback. The project would align NRAO with the national and international SKA community, something that would hopefully not be lost on NSF – in their eyes, EVLA phase 2 money would be leveraged with SKA.

It may also be worth noting that such a phase 2 would be more complex, more challenging, and hopefully more exciting for NRAO staff to work on than "more of the same" represented by the addition of a few more "Pietowns" to the VLA.

Next, consider the relationship of LOFAR and ATA to the NTA enterprise. LOFAR will have a square kilometer at 15 MHz, and thanks to the high frequency antenna design will have a significant fraction of a square kilometer even at 150 MHz. This collecting area will be much cheaper than anything contemplated for SKA, so there is little point compromising designs to reach such frequencies with, for example, small paraboloids. The currently stated SKA frequency range is a stretch for any single antenna technology. The bald fact is that dipole arrays are a superior technology for the EOR science problem, offering more capability at lower cost. This could be interpreted as LOFAR simply appropriating the EOR science driver, to the detriment of SKA. A more enlightened view, however, would be that LOFAR will bring us an early opportunity to attack this most exciting topic **as an integral part of the NTA enterprise**, in which the entire community is involved.

ATA and its successors or enhancements can and should be regarded in a similarly constructive way, in terms of a complementary component of the NTA enterprise.

Finally, we consider the SKA project, specifically within the US. The goal of building a square kilometer of collecting area is quite arbitrary, though it has proven to be an effective national and international rallying point. In the context of a NTA enterprise, however, it makes sense to seek this goal only for mid and long wavelengths, given the remarks above concerning EVLA phase 2. This renders cost estimates below $1B more credible. It also allows SKA to concentrate on frequencies and techniques where a large-N design offers the most compelling advantages. It strongly leverages existing ATA and LOFAR efforts.

So what does our currently conceived SKA telescope morph into under this NTA enterprise picture? A likely outcome is that "SKA" will become at least 3 instruments, only one of which will have a full square kilometer, perhaps from 500 MHz to 5 or 10 GHz. We would have an ultra-flexible LOFAR-like array based on dipoles or Dutch tiles at lower frequencies (perhaps even as an extension of LOFAR). A massive centimeter-wave array featuring raw sensitivity and spectacular imaging would be built as a distinct entity separate from the VLA. The EVLA itself would become an expandable and expanding high frequency array, dominating wavelengths shorter than several centimeters (call it EVLA++), tightly integrated with the VLBA and providing a smooth transition between SKA and ALMA capabilities.

Where would such arrays be located? This approach may be the light at the end of the tunnel of what may become a very knotty political problem for SKA, since multiple locations would host key, indispensable components born of the NTA enterprise. Viewed in this NTA perspective, both LOFAR and EVLA phase 2 are central components of the larger enterprise (that is currently labeled "SKA"), and as such represent strong US (and in the case of LOFAR, Dutch) activity **this decade**, thereby meshing sensibly with the international SKA timetable. We would thus be correctly perceived as coordinating effectively with our international colleagues.

Implementation

What would need to be done to move effectively in the above directions? First, the US SKA consortium plan articulated to the NSF would have to be modified to reflect a broader NTA enterprise approach. NRAO would need to re-think the design of EVLA phase 2, with more aggressive exploitation of new techniques to drive down collecting area costs, thus positioning EVLA into a central role in the national and international NTA enterprise. This central EVLA role would need to be reflected in the position of the US SKA consortium as presented to the NSF. The US SKA consortium, in collaboration with our international colleagues, will need to carefully redefine SKA in terms of NTA ideas. Careful coordination between any US SKA consortium NSF proposal and the EVLA phase 2 proposal will be needed, if NSF is to justifiably conclude that the US radio astronomy community as a whole, both NRAO and the universities, is unified in its goals, and is thus worthy of strong, directed support. The NTA should be the unifying concept which guides the design of these proposals.