

Supplementary Document for NSF/ATI Proposal 0234128  
**Technology Development for the Square Kilometer Array. II**  
Responses to Reviewers Comments

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SKA Acronyms Used:

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ATA	Allen Telescope Array
EMT	Engineering Management Team
ISAC	International Science Advisory Committee
ISSC	International SKA Steering Committee
LNSD	Large-N, Small-Diameter
SESC	Site Evaluation and Selection Committee

**Document Plan:** Before responding to the review panel's explicit questions and criticisms, we discuss the proposed work in the context of the international time line (§1) and we discuss briefly in §2 how the proposed work areas fit into the full picture for development of the US design concept for the SKA.

# 1. GOALS AND TIMELINE FOR THE US SKA DEVELOPMENT EFFORT

## Context and Supporting Documents

The US SKA Consortium is the national organization concerned with promoting and realizing development of a design concept for the SKA, the Large-N, Small-D (LNSD) concept. In addition, the Consortium is preparing a proposal for hosting and siting the SKA in the southwestern United States. Members of the Consortium also are involved with defining science goals and developing and managing the SKA project on the international level through participation in appropriate committees that exist for these purposes.

The work we have proposed in both the funded NSF ATI grant (based on a proposal submitted in 2001) and the pending 2002 ATI proposal (the subject of this document) is intended to fulfill our broad goals of achieving a cost-effective design that meets science goals and allowing us to present a compelling proposal to the International SKA Steering Committee (ISSC).

Our work is in accord with the “roadmap” submitted by the Consortium to the NSF in April 2001, “A Roadmap for the United States’ Development Efforts on the Square Kilometer Array.”<sup>1</sup> However, we point out that milestone dates discussed in the Roadmap have since been altered by the ISSC, as we now discuss.

### Timelines Defined by the International SKA Steering Committee

The current timeline defined by the ISSC in conjunction with its advisory committees (EMT, ISAC and SESC) is given in Table 1.

TABLE 1: SKA DEVELOPMENT TIME FRAMES FOR THE US EFFORT IN THE INTERNATIONAL CONTEXT

Event	Date	Status
Concept Whitepapers, phase 1	15 June 2002	done
Evaluation of Concept Whitepapers	Aug-Nov 2002	done by EMT & ISAC
Revised Concept Whitepaper	31 May 2003	in progress
Initial Site Hosting Proposal	31 May 2003	in progress
SKA Science Retreat (Leiden)	Nov 2003	pending
SKA Science Case Book	May 2004	in preparation
New Concept Whitepaper	Mid 2004	in progress
Updated Site Hosting Proposal	Mid 2004	future
Final Site Hosting Proposal	Mid 2005	future
Revised Concept Whitepaper	Mid 2005	future
Concept downselection	2005	by ISSC
Site Selection	2005	by ISSC
Concept Selection	2007	by ISSC
Decadal Survey begins	2008	
Construction proposal	$\gtrsim$ 2010	

We note that the timeline was changed by the ISSC since we submitted the April 2001 Roadmap to the NSF. The milestones themselves have not changed, but the concept selection has been moved from 2005 to

<sup>1</sup>available at <http://www.astro.cornell.edu/~cordes/SKA>

2007. Considerable work needs to be done in optimizing SKA specifications against key science goals, while recognizing that the SKA's greatest contributions may be as a general purpose facility.

The current timeline is set by a balance between technology development time and important events in the presentation of the SKA to review panels and funding agencies in a timely fashion. Of particular importance is the need to present a detailed concept and science case to the next US Astronomy and Astrophysics Survey Committee ("Decadal Survey") expected to be convened in 2008.

### SKA Science as Currently Assessed by the ISAC

Table 2 lists the "Level 1" science goals identified by the ISAC for the SKA. The current SKA concepts have been evaluated (in August 2002) with respect to how well they meet the science goals of the 18 Level-1 science areas. This list, as defined by the ISAC and 9 working groups, is not a full list of SKA science, but rather represents the top two or three science areas identified by each working group. Currently, there are discussions taking place aimed at gleaning from the 18 science areas a short list ("Level 0") that "drives" the SKA project. We also emphasize that there is a near consensus that, though the Level 0 or Level 1 science areas are all interesting at present, the real value of the SKA will emerge for science areas that we have not yet thought of. An additional comment relevant to the particular configuration currently designated by the US SKA Consortium for the LNSD concept is that the *astrometry* capabilities of the SKA will be extremely powerful. Very weak sources can be used as calibrators so every program source will have nearby sources usable for phase calibration, thus allowing minimization of ionospheric and tropospheric perturbations. Astrometry doesn't appear in the table because it isn't a scientific category *per se* but it spans many of the categories and is a strong selling point for the SKA.

### Critical and Parallel Paths for the US SKA Development Effort

In broad brush, the LNSD concept pursued by the US SKA Consortium consists of:

1. An array of  $\sim 4400$  small-diameter paraboloids ( $\sim 12$  m) arranged in a scale-free configuration on baselines from 15 m to  $\gtrsim 5000$  km.
2. An operating frequency range from  $\sim 0.15$  to  $\gtrsim 25$  GHz.
3. Transmission of wideband signals from individual antennas (inner  $\gtrsim 50\%$  of array) and from antenna stations ( $\lesssim 50\%$ ) to a central processor for correlation and/or beam forming.
4. Sufficient modes of operation to achieve a wide range of imaging and non-imaging science, including high-dynamic range mapping on scales from arcminutes to submilliarcseconds and blind surveys for pulsars, transients and ETI.
5. Redundancy and fault-tolerance for minimizing maintenance and operations costs.

We point out that of the seven concepts presented to the ISSC in June 2002, the US concept is the highest ranked in terms of compliance with the 18 science areas listed in Table 2. We are thus at an advantage, for the moment, but if we cannot pursue key areas further, our concept will not be competitive at the time of the downselection process in 2005 and the final selection in 2007.

Great challenges are involved in realizing a compelling and realistic plan for building an instrument of the type just summarized. The work proposed to the ATI program for both our 2001 and 2002 proposals is intended to address these challenges.

Table 2: Level 1 Science Areas Defined by the ISAC and Working Groups (WG)

Item	WG	Description	Technical Capability or Issue
1	1	Galactic H I	Low-surface brightness sens. Size of core array; u-v coverage
2	1	Galactic Nonthermal+B	Off-axis polarization capability Size of core array
3	2	Transients	Blind surveys & response times
4	2	Pulsars	//
5	2	SETI	//
6	3	Epoch of Reionization	Low-frequency coverage Low-surface brightness sensitivity
7	4	H I surveys / Large-scale structure	Imaging dynamic range
8	4	Continuum surveys	//
9	4	CO surveys (high z)	High frequency coverage
10	5	High-z AGN	Low-frequency coverage
11	5	Inner AGN	
12	6	Protoplanetary Systems	High-frequency coverage, moderate spatial resolution
13	7	Coronal Mass Ejections	Low-frequency coverage
14	7	SS bodies (Kuiper belt objects)	Correlator bandwidth High frequency coverage
15	8	IGM (non thermal)	Low-surface brightness sens.
16	8	IGM (thermal)	
17	9	Spacecraft tracking	High-frequency coverage
18	9	Geodesy	(IF separation)

The goals that we have identified as important during the development phase in the US are shown in Table 3, “Critical Tasks and Activities for the US SKA Consortium.” The prime goals are:

- (a) Submitting a North American site proposal to the ISSC in 2005.
- (b) Developing the LNSD concept for presentation to the ISSC in 2005, with planned downselection of concepts by the ISSC in that year.
- (c) Presenting a final LNSD concept to the ISSC in 2007 for final concept selection.
- (d) Developing a full SKA design and science plan for presentation to the Decadal Survey (and to counterpart review processes in other countries).
- (e) Prototyping critical subsystems so that a realistic cost and performance plan may be submitted to funding agencies in the latter part of this decade or the beginning of the next.

Table 3 attributes critical items to the two ATI proposals we have submitted. It also links many of the tasks to parallel work being done by closely related projects, the ATA, EVLA, LOFAR and the DSN (Deep Space Network). In the table, the first two columns identify the element for the design and the critical tasks associated with that element. The next three columns indicate (with a bullet) whether the funding and activity associated with the element is from the first ATI grant (ATI2001), the current proposal (ATI2002),

or are anticipated from a future proposal to the NSF. The last column indicates whether the activity is associated with related projects or telescopes such as Arecibo, the ATA, DSN, EVLA, GBT, VLBA.

Table 3 shows the critical elements that need addressing in order to produce a compelling design that can be accurately costed and which meets the science requirements. The table also intends to tie together the multiple threads of the development effort into the coherent picture that underlies the effort. We emphasize that the two ATI proposals we have submitted (2001 and 2002) were written with this coherent picture in mind, a picture originally presented in the Roadmap document to the NSF.

As can be seen from Table 3, the column labelled “Future NSF Support” has many entries (bullets) that indicate the need for substantially more funding from the NSF in order to reach a detailed design for the SKA with the LNSD concept. When we wrote the Roadmap document in 2001, we suggested that a prototype array with, perhaps, 10% of the collecting area of the SKA, should be built later in this decade. Funding guidelines consistent with the recommendation in the last Decadal Survey are insufficient for such a prototype to be built. Ideally, the funding situation will change but for now we must anticipate developing the LNSD concept within the guideline. For this reason, we picture developing the concept through prototyping of *subsystems* of the SKA and to do so, we need to rely in part on the efforts now ongoing with the ATA, LOFAR, and the EVLA. For SKA-specific subsystems, including the antenna, new feeds, antenna optics, LNAs, digital samplers, and data transmission we will need to prototype a few individual elements in, for example, a small array. Because maintenance of  $> 4000$  antennas is costly if the mean time between failures is large, we need to operate a number of antennas for a few years in order to assess what the MTBF is.

## 2. Main Work Areas Proposed

In the second proposal to the ATI program (ATI2002) we requested support for five work areas that relate to critical tasks outlined in Table 3. After regrouping the work areas originally presented in the proposal somewhat, the five work areas are as described below. We note that the first three involve RFI mitigation:

- 1. Epoch of Reionization and Low-frequency Recombination Lines.** The value of this project for SKA development is that it (a) provides a new attempt at detecting the EoR signal that is cognizant of recent work by WMAP and which has been a prime science area for the SKA; (b) requires sophisticated RFI excision techniques, some of which are interferometric in nature and fall squarely within the set of techniques expected to be used with the SKA; (c) will aid in defining the lower frequency limit for the SKA, with attendant implications for feed and subreflector design. The doability of the EoR is a topic of vigorous discussion in both the LOFAR and the SKA communities and we consider it sensible to use available infrastructure (Arecibo) to make progress.
- 2. Using SKA prototype Broadband Feeds with a Large Collecting Area.** Radio astronomy bandwidths are typically much smaller than those anticipated for use with the LNSD concept. Following preliminary work for the ATA, whose feed frequency range is 0.5 to 11 GHz, the plan is to use two or more broadband feeds to cover the eventual frequency range of the SKA, which is nominally 0.15 to 22 GHz according to international management (the US SKA Consortium is considering higher frequencies as well). Experience with such feeds on radio telescopes is nil. Our plan is to use Arecibo to test such feeds and identify problems and solutions for using the feeds in an RFI-intense environment. Such experience will provide recommendations for how to contend with RFI on SKA antennas, whether notch filters are needed and how receiver linearity depends on the particular RFI environment. In addition, some of the test observations will be science motivated through studies of giant pulses from the Crab pulsar and searches for giant pulses from other objects, including M33. The main goal of this project is to test the ATA feed in a large-gain astronomical context. It is likely that additional feeds, such as the TRW feed and others not-yet designed, could also profitably be tested in this way.
- 3. Preparing a North American Site Proposal: Testing for RFI.** The US SKA Consortium is preparing a plan for a North American ‘site’ with an array center in New Mexico and a scale-free configuration extending to VLBI scales that are likely to reach Canada and Mexico. Part of the formal process for selecting an SKA site is the submission of initial site “proto-proposals” to the ISSC *this year* (2003). Over the next two years, considerably more effort is needed to characterize the sites for a core area of diameter  $\sim 35$  km (containing 50% of the collecting area) and for outlier single antennas and antenna stations to distances of several thousand km. The effort that we propose in support of the site proposal is the characterization and monitoring of RFI over frequencies of interest to the SKA and, for purposes of synergy with other, related developments, to LOFAR and the EVLA. RFI is of course a moving target, evolving rapidly in some but not all parts of the spectrum. However, we must characterize the RFI situation now in order to produce a compelling site proposal but also for the purpose of having hard data that will support our appeals to the FCC and other relevant institutions that can influence radio quietness.
- 4. Packet Switching Data Transport Feasibility Studies.**

Large instantaneous bandwidth is a hallmark of the SKA (in both the international specifications and in the LNSD concept). Combined with the Large-N nature of the concept, data rates from antennas and stations are enormous and must be brought to a central processing location in a cost-effective way. Current thinking is that dedicated fiber will be laid out to approximately 35 km from the array center, while commercial fiber may be used on larger scales. The boundary on which we change methods is a

moving target. Nonetheless, on some scale, we will want to use commercial fiber and it is worthwhile initiating experiments within the US to use fiber in test VLBI observations. This task is of interest to NRAO and to several member institutions of the US SKA consortium. Such work provides good student projects.

5. **Polarization Requirements for the SKA.** The LNSD, along with other competing concepts, will be carefully assessed by the ISAC to determine whether it can meet the specified science goals. Polarization capability underlies many of the key science areas for the SKA, but such capability is yet to be evaluated for the LNSD concept. High fidelity polarimetry over the SKA's large field of view is likely to place severe requirements on the polarization purity of the overall optics and also on post-processing calibration. These requirements in turn affect decisions on antenna design such as whether elements are symmetric or are off-axis paraboloids. Because the antennas are a large part of the overall cost of the SKA in the LNSD concept, quantification of the polarization requirements of the SKA and investigation into how the LNSD concept can meet these requirements are both needed at this early stage.

### 3. Responses to Specific Questions and Comments of Reviewers of ATI2002

In the following we respond to particular comments made by reviewers and the review panel about our proposal. As a general statement, we note that many of the comments refer to a lack of coherence of the proposed work with respect to overall goals for SKA development. Part of this perception derives, we think, from the fact that while our first proposal (ATI2001) was a comprehensive, end-to-end proposal with work areas ranging from theoretical and simulation studies of scientific issues to technology development and outreach, the second proposal contained a minimal amount of background material and mainly presented justification for the particular projects included. The NSF provided excerpts of some of the reviews and it is these that we explicitly respond to. *Reviewer comments are in italics; our responses are in bold.*

*Reviewer 1:*

*This proposal is not written to be a stand alone document. Nearly everything in it references other prior proposals or white papers, and so it is impossible to assess the merits of the work based on the proposal alone. I would describe the proposal as more of a management plan for the work. In fairness to the project, I think it is much too complex to fit well within the 30 page limit, but this proposal is particularly poor. I think the proposed work may have considerable merit but I can not give this proposal a good rank.*

**In preparing the proposal, we were trying to be efficient in presenting the material by referring back to the first proposal (ATI2001), to the Strawman LNSD Concept Document, and to the Roadmap submitted to the NSF in April 2001 that, together, thread the subprojects we have proposed in the two NSF proposals.**

*My main concern is that there is no technical content at a level that is helpful in assessing the feasibility of the proposed work. The primary technical hurdle in making the SKA a viable project is RFI mitigation.*

**We couldn't agree more with this succinct statement by the reviewer about RFI being one of the potential show-stoppers for achieving SKA type sensitivities. In the following we give details about our technical approach for the catwalk feed project and for using broadband feeds and receivers at Arecibo.**

*Unless this really works there is no point in proceeding since the problem will only get worse. The proposal provides no information on how this can be done besides the use of a multiple feed system. This may provide a way to measure local RFI, but the reference antennas do not receive exactly the same signal as the*

*main antenna, and very sophisticated signal processing is required to subtract the RFI at a >40 dB level. Extraordinary linearity of the front end is also required, and even if the reference signals precisely match the main signal as received, weak saturation will cause them to differ after amplification. Any method of subtraction of RFI would appear to raise the noise level as well. Work may be ongoing to address these issues but there is no mention of any studies or results in the proposal.*

Again, we agree completely that RFI might prohibit success for the SKA and for meter/centimeter radio astronomy in general unless we do something about it. Most of the effort proposed in ATI2002 concerns RFI for this very reason, the aim being to attack RFI on several fronts. For the catwalk EoR project, where we propose using four feeds under the catwalk and an auxiliary feed on the carriage house, the plan is to explore several methods of RFI excision. These range from well-known methods such as installing notch filters in sub-bands with essentially continuous RFI, to editing of signals in the frequency-time plane, to the relatively sophisticated approach involving cross-correlations between the different feeds to determine the gain constants needed to subtract RFI signals. This last approach is based on the method proposed by Briggs, Bell & Kesteven (2000), which is interferometric in nature and thus transcends the single-dish nature of the particular platform we are using for RFI development. Some comments on this approach are discussed in the Appendix.

We assert that a set of plausible, attractive solutions to RFI has been articulated and shown (anecdotally) to be valid. It is now time to really exercise these simple approaches and see what the practical limits & pitfalls are. It is important to do this *before* detailed design effort begins on SKA, since RFI mitigation touches every aspect of the system. We also assert that it is incorrect to state that “Any method of subtraction of RFI would appear to raise the noise level as well.” The only method we know that really does this is the Barnbaum & Bradley least-mean-square canceler, which is a non-starter for us anyway. Blanking, parametric canceling, post-correlation, nullforming, anti-coincidence methods do not raise the noise level, though they lessen the net integration time. We are fully aware that RFI excision must be handled carefully in order to avoid introduction of systematic errors, such as non-uniform baselines in the time and frequency domains and other “toxicity.”

Co-I Ellingson has a long track record in investigating blanking and beam nulling approaches to RFI excision. PI Cordes is now investigating data mining of dynamic spectra that aims to classify all non-noise signals contained in a dynamic spectrum, including RFI of all kinds and astronomical signals, including dispersed pulses from pulsars.

*There is not even a very useful description of what the SKA is to be. While I realize that it is far from a set configuration, a straw-man type description would be very helpful to show at least one configuration which meets the specifications. There is no overall budget information, not even a goal, and no way to project whether we ever could afford such a huge project. If things are really so vague then how can we take this project seriously?*

The LNSD Concept Whitepaper, submitted to the ISSC in June 2002 and available at [www.astro.cornell/~cordes/SKA](http://www.astro.cornell/~cordes/SKA), contains detail that the reviewer is asking for (and much more). It contains a very detailed description of the LNSD concept that the US SKA Consortium is pursuing and includes a description of the scale-free configuration, the frequency ranges, the antennas and some aspects of the signal processing. There is also a cost estimate for the LNSD version of the SKA. To be sure, many details are vague and need to be addressed. That is the goal of the development effort over the course of this decade.

*I question the present work on site selection based on RFI measurements since there is no criteria for acceptable RFI, nor any way to determine what the RFI level will be in 10-20 years. Assuming an excellent set of locations is found, what can be done to protect these from cell phone towers and a host of other wireless services before funding for the SKA is available? Is it possible to get a commitment to maintain radio quiet at many sites for years before the project begins? There may be places in the world so far from population centers that RFI may be slowly changing but this does not apply to the southwest US. Admittedly there is a need to obtain typical data to guide future work on RFI mitigation, but a full site survey does not make sense.*

Clearly it is important for us to justify making RFI site characterization now as opposed to ten years from now. Formally, we need to develop a site proposal over the next two years for submission to the ISSC, as described previously. Other, competing sites (Australia and South Africa, for example) are being characterized in this way using substantial resources. We also contend that immediate site surveying is important because

- (a) Careful identification of RFI signals gives us some credibility in approaching relevant agencies (FCC, CORF) about misuse of the spectrum or for arguing for some degree of radio quietness for, say, the core array part of the SKA.
- (b) Usage of most of the spectrum is not fastly changing: the vast majority of the spectrum is used more or less the same way now as it has been for the last 40 years.
- (c) For spectral regions where usage is fastly changing, there is all the more impetus to characterize the current status to provide a baseline for future measurements.

In terms of synergy with NRAO and the EVLA II, discussions with NRAO indicate that it would be jointly beneficial to make measurements at some of the potential EVLA II sites, which may evolve into particular SKA sites or serve as proxies for those sites. Our plan includes comprehensive measurements on the Plains of San Augustin, where a core array could be built. NRAO makes some measurements at the VLA site, but they are not as comprehensive as we need for the SKA (both in frequency coverage and in temporal sampling). LOFAR's core array may also be sited there, so our measurements would be made in conjunction with the Southwest Consortium, which is proposing a southwestern site for LOFAR.

Specific actions would include constructing low-gain antennas to cover the frequency range of 0.15 to 22 GHz, purchasing a mast to hold antennas, a programmable spectrometer, and a laptop computer for controlling it, and acquiring software for the spectrometer. A vehicle is needed for transporting the hardware and we will look into rentals or a purchase, or perhaps borrowing a vehicle from NRAO. A technician (shared with the EoR project) would conduct the measurements along with some assistance from collaborating institutions. A few days would be needed for each site for an initial characterization. We would conduct lengthier measurements on the Plains of San Augustin in order to obtain seasonal and secular trend data.

*I also wonder why a present site survey must extend up to 50 GHz, given that there are virtually no ground based uses of the spectrum above 30 GHz at this time (although this is likely to change). It should be easy to locate those few users of the mm-wave spectrum, and space based sources are present at all sites. Expanding the frequency coverage has a significant cost and time impact, and will yield little or nothing, particularly since the SKA is supposed to cover < 30 GHz.*

We agree with the referee that measurements above  $\sim 25$  GHz are not needed at this point. We included the range from 25 to 50 GHz in the interests of covering bands relevant to the EVLA, rather than the SKA. Our site testing will be done in conjunction with NRAO, NRL and the Southwest Consortium, so our designated frequency range was for purposes of inclusiveness. We agree, as do our NRAO colleagues, that detailed characterization of the high-frequency bands is not needed.

*None of this is meant to say that the project lacks scientific merit, and I am sure that there will be considerable payoffs for exploring this important part of the spectrum. However, even the science case is quite weak in the proposal. The EoR description may be useful to the specialist but I can not determine what a HI line might look like from this epoch, and whether we might hope to really measure it.*

We definitely were thin on discussing the science case for the EoR. Our assumption was that the science case for the EoR was well known and thus concentrated on discussing in the proposal what we intended to do. For the record, the EoR has been discussed at length in many SKA venues, including “Science with the Square Kilometer Array,” by Braun & Taylor (1999) ([www.skatelescope.org/ska\\_science.shtml](http://www.skatelescope.org/ska_science.shtml)). At the time we wrote our proposal, the anticipated signature would be spectral and spatial structure, as opposed to a well-defined spectral line that varies spatially. In light of recent WMAP results on the cosmic microwave background (CMB), it is now suggested that the reionization is spread out over redshifts from about 20 to 6, perhaps with activity at two particular epochs. For us, this implies that the spectral signature in HI may extend from about 70 to 200 MHz. Current discussions in the LOFAR community are now addressing the question of whether LOFAR should operate *within* the FM band or not, for this reason. While we might consider operating the catwalk system to as low as 70 MHz, we do not think it feasible because RFI is too intense and we will not have the advantage of widely separated sites to help remove it. Thus our nominal range of 100 to 250 MHz specified in the proposal is likely to remain so that we can cover some of the EoR range and also detect recombination lines. Current SKA specifications include a lower frequency bound of 150 MHz. However, there are discussions about going to lower frequencies even with the 12m antennas currently contemplated for the LNSD concept, in spite of the fact that their diameters represent only four wavelengths at 100 MHz. Also, independent of what pans out for the SKA, the scientific goals of the EoR project demand that we cover as low a frequency as possible (most likely to the upper edge of the FM band). We point out that two of us are involved with the LOFAR effort. PI Cordes is on the Science Consortium Board for LOFAR, which assesses the science case for LOFAR and compliance of design specifications for the science. Co-I Ellingson is chair of the technical advisory committee for LOFAR, which serves as the review board for the design-review process.

Also relevant is a recent paper by Furlanetto, Sokasian & Hernquist (2003, astro-ph/0305065), “Observing the Reionization Epoch Through 21 Centimeter Radiation,” that discusses the EoR signature in light of the WMAP results. They predict a sharp drop in the spatial/spectral fluctuations at  $z \approx 8$  (158 MHz) that is squarely in the band we wish to consider.

*The proposed work at Arecibo will be a significant step toward answering some important questions about feasibility, but I can not determine what will be done. The proposed 10 channel spectrometer backend is never described, except for a budget, which seems extremely small.*

The catwalk feed system will consist of four, single-polarization feeds mounted under the catwalk and an auxiliary feed mounted on the carriage house, to serve as one of the RFI monitoring antennas. The feeds will be offset spatially so that different sky positions are sampled

instantaneously but a celestial source will drift through two of the feeds. The feeds must be designed so that they cover the desired frequency range, they illuminate the appropriate portion of the main reflector (about a 100m patch), and that cross-talk between the feeds be minimal. Weintroub et al. (e.g. 1998 Ph.D. Thesis) adopted two closely spaced helical feeds and discarded one of them because of cross talk, thus disallowing dual-beam approaches to excision of RFI. We are considering helical feeds with larger spacing and perhaps alternating polarization (RHCP adjacent to LHCP) in order to minimize cross talk. Yagis may provide a better alternative, but we also need to consider overall size of the framework that needs to hang off the catwalk. Receivers will be designed with particular attention paid to dynamic range in order that we can keep the receiver linear in the presence of most of the RFI. Signals will most likely be transported as analog RF to the control building, where they will be processed collectively via mixing and cross correlation. Dynamic range of the fibers is also a concern, which we are now investigating. Experience with existing fiber carrying signals from the gregorian dome and also new fibers installed for the 7-feed ALFA system (Arecibo L-band Feed Array) provide good experience for making a choice suitable for the catwalk system, which will require much narrower bandwidths than ALFA or other gregorian receiver systems. The spectrometers will be custom made by co-I Ellingson and build upon his experience with constructing FPGA-based FFT spectrometers. They will produce a large number of channels ( $\sim 256$ ) to allow excision of narrow lines, after which the resolution will be degraded for optimizing detection of the EoR signal. The correlator will thus be of FX type, the spectrometers providing the first, F, part and custom hardware the X part. The parts costs are relatively inexpensive; most of the cost is in the engineering.

*The overall budget is impressively large for the minimal amount of justification, and I think a proposal this big should present a much more organized case. Even budget pages are quite terse, and most of the SAO budget is in line "other" without any explanation.*

The catwalk EoR project has many components within the hardware, software development, and data analysis categories. Substantial help will be gotten from NAIC in designing and installing the feed array, receivers, and fiber. Some of this effort is costed as a portion of the one-half technician in the Cornell budget (the remainder for working on the ATA/TRW feed project). A breakdown of the costs is shown in Table 4, "Hardware Budget for the EoR Portion of the Project." The bulk of the costs for the project are for personnel, some of co-I Ellingson's time, an engineer at OSU, an engineer and technician at Cornell/NAIC, and graduate students at OSU and UMn. Given the level of effort needed by Weintroub et al. to build their spectrometer and conduct their single-pixel survey (two graduate students and engineering time at Harvard, and assistance from NAIC), this level of effort is appropriate for a more complex set of hardware and data analysis.

*Reviewer 2:*

*This is a second attempt by this team to get relatively major funding to support efforts to create a U.S. design for the Square Kilometer Array (SKA). The team certainly has the credentials and experience to do the work they have set out in the proposal. What is less well justified in my view is the fairly specific research they lay out, for a project that is very ill defined and has an astronomical price tag assuming that it can be done at all. The study of moving data over switched networks, for example, seems prematurely detailed given the poorly defined nature of the project. The science showcased in the proposal (EoR) apparently only requires 15 km baselines, after all. Still, I suppose that any costing of the US design will require that this question be settled.*

The reviewer has certainly highlighted one of the main issues now being discussed in the SKA community, namely which of the myriad of science drivers should be highlighted in broad brush discussions of what the SKA will do. It would have helped if the reviewers could have seen some of the summary documents, produced at the international level, concerning SKA science. The range of science demands a design that covers many angular scales, ranging from arcminutes to milliarcseconds. Currently, discussions taking place are aimed at gleaning from the 18 science areas a short list that “drives” the SKA project, as mentioned earlier in §1. We also emphasize that there is a near consensus that, though the Level 0 or Level 1 science areas are all interesting at present, the real value of the SKA will emerge for science areas that we have not yet thought of.

Given that the SKA science discussions are quite detailed and ongoing, we would state that the goals for the SKA are very well defined in the sense that there is a wide range of science enabled by the SKA, especially for the LNSD concept. We also feel, however, that the SKA project needs to select from the science goals a few key projects that especially drive the design of the SKA and promote those within the astronomical community. It is almost certain that the ultimate design will involve tradeoffs that mean some of the science areas will either not be doable or will be de-emphasized. These tradeoffs will be the subject of several workshops over the next two years. We are convinced that the SKA will be doable technically, after a lot of hard work this decade. A big challenge is to build the SKA economically. The particular project proposed by us and alluded to by the reviewer (EoR) indeed requires only the central “core array” portion of the SKA. However, other science areas require the more extended parts of the array. Our goals for the EoR are described above and will influence the design of the SKA (in the LNSD concept) with regard to frequency coverage, feed design and perhaps even antenna type appropriate for low frequencies.

*I am aware of the synergies with current projects of some of the other aspects of the proposal, such as RFI mitigation and site surveys. But both of these activities are already being done for real instruments, LOFAR and EVLA (New Mexico Array), and it would seem more appropriate, if more effort is needed, to propose to do it under these projects. I assume that the previous proposal was partially funded in recognition of the importance of RFI mitigation research, but it was not made clear why a second grant is needed after only a year.*

We agree that significant work is contemplated for LOFAR and the EVLA that at some level overlaps with what we are doing, but we respectfully point out that neither of these exist yet! The SKA project certainly has some commonality with these instruments but it is also true that it will be an instrument quite distinct. Therefore, additional funds for LOFAR and the EVLA would not necessarily advance SKA development. The partially funded work in ATI2001 included a piece of RFI mitigation research involving, primarily, dual site measurements. We view this as one piece of the overall effort needed for developing RFI mitigation techniques that attack the RFI problem from a number of directions.

*The catwalk project at Arecibo sounds interesting, and probably worth doing, but does it really have anything to do with SKA? Only the RFI part of the study is clearly relevant. I rather like the idea of using a dedicated broadband instrument to detect both a strong (OMC) and weak (galactic recombination line) component amid strong RFI, and it may make an ideal dataset for some aspects of RFI mitigation. But the subtler effects will be seen in interferometric data, which is not tested by this research plan. The big science results hoped for regarding the EoR seem unlikely to be achieved with this system, according to the assessment of the proposers. Putting the ATA feed at Arecibo seems to have even less to do with SKA, but it is not a cost driver of this proposal.*

As discussed previously in §§2-3, we see the use of broadband feeds and the catwalk project as providing important expertise that will feed into the design and the definition of operating modes for the SKA. At this point, no-one can really say if the EoR signal will be detected with the catwalk system but one may be pessimistic about its detection with LOFAR and the SKA as well. We view the situation as one of the glass being half full rather than half empty, in the sense that Arecibo provides a unique opportunity for a pilot study of the EoR that will contribute to the overarching science goals for LOFAR and the SKA. Whether we detect the EoR signal or not, achieving useful upper bounds requires a very serious effort in RFI mitigation which will benefit the design of the SKA. Thus we agree completely with the reviewer in stating that "...the RFI part of the study is clearly relevant."

*Summary:*

*I am somewhat supportive of the catwalk experiment, mainly for the RFI mitigation study rather than the science. The other proposed activities, however, seem somewhat contrived. There is already so much funding support from many sources for EVLA, ALMA, ATA, and LOFAR, where the real technology development is being done, that adding more funds for this work seems unnecessary. Those interferometer arrays are more germane to SKA than the proposed single-dish experiments anyway.*

As we described in §1 and Tables 1 and 3, the projects we have proposed are intended to complement the activities now ongoing for the EVLA, ATA and LOFAR that pertain to the SKA. We also emphasize that though a single dish (Arecibo) provides the collecting area, one of our RFI excision techniques is interferometric in nature. In addition we point out that, unlike any of the other projects, save LOFAR (which is not yet built), the SKA will be designed so that it can be used in effectively a single dish mode. For example, the core array, of size to be determined, will be used at times for blind searching of the full field of view (FOV) of the primary antenna beams. Targets for blind searching include pulsars, transient sources, and ETI. This capability is challenging because it requires constructing all synthesized beams needed to cover the FOV, a daunting capability if one looks at the necessary number of calculations. The number of such beams  $\sim (b_{\max}/D)^2$ , where  $b_{\max}$  is the largest baseline of the core array and  $D$  is the antenna diameter. Even for an inner core of size 1 km, which contains  $\sim 25\%$  of the collecting area in the LNSD concept (cf. 2002 Whitepaper), the number of beams needed  $\sim 10^4$  and each of these must be searched for (e.g.) periodic pulsar emission. Experience at Arecibo is directly relevant to this mode of operation, particularly with multiple beam feed arrays. Currently, none of the projects mentioned by the reviewer will have this capability, except possibly for LOFAR. So, we argue that using the largest available collecting area — Arecibo — is germane to some important aspects of the SKA.

*Reviewer 3:*

*This umbrella proposal covers a broad range of work that can be related to support of the US consortium's SKA participation. It is a large and costly proposal that covers a number of astronomical and technical questions without a clear sense of how the component pieces fit into the larger project. Some of the subprojects, for instance the use of Arecibo to make pilot studies for the era of recombination observations, are scientifically very interesting; others, for instance the associated RFI mitigation work, are technically very useful for this and other projects; but others, for instance data transmission tests, are not well-justified in effort or personnel.*

The component pieces do fit into the larger project, the SKA. As stated before, our first proposal (ATI2001) presented a much more comprehensive picture of how the pieces fit together.

In this document, we have tried to present information or pointers to documents with that show a more complete picture. Table 3 in the present document lists key work areas and how they are being approached under the first grant received under ATI2001, under the second proposal (ATI2002), and under anticipated future requests to the NSF. The end product of the development effort will be a detailed presentation to the next decadal survey, followed by a proposal to the NSF. Data transmission is specifically important for the project. Usage of dedicated fiber has already taken place at NRAO and with the EVN. The EVLA will also comprise a dedicated network. For the long distances to the outer antennas (or stations of antennas) in the LNSD concept, we don't know what the most-cost effective method for data transmission will be. For this reason, we consider it important to experiment with packet-switched networks and assess performance in reconstructing data sets from packets. Existing infrastructure, the VLBA and other antennas, provides a good testbench for this purpose.

*Adding an ATA wideband feed at the telescope's focus is a very nice project, but it should be funded as an Arecibo project. It is somewhat surprising that the project management has left a considerable amount of site characterization, white papers and RFI studies, to this proposal. A new RFI survey at some level in the southwest is useful, but it is unclear how the electromagnetic environment would change over the lifetime of an instrument like the SKA. Tests are a necessary part of a site survey, but have little purpose unless they are linked to legislative plans to keep the site's environment static or predictable. The proposal does not discuss the use of the site survey in future negotiations. Quick tests are justified, and it is surprising that these are not a very high priority in the current project.*

While using the ATA feed at Arecibo could be of interest from an Arecibo-only point of view, there are no funds to install such a feed nor is there currently a scientific proposal requesting such a feed. We see the use of the ATA feed at Arecibo as a stringent field test of a crucial element of the basic building block for the SKA, along with the companion broadband LNA. Nonetheless, we think that gaining some scientific spinoff from the feed is sensible, and study of the Crab pulsar's giant pulses is a clear example.

We emphasize that the site testing components *were* part of our original ATI2001 proposal and were cut because we were funded at less than one third of our request. The point of the ATI2002 proposal was to attempt to get these funds for a crucial part of the development effort, which the reviewer endorses. In §§1-2 of this document, we elaborate on why site testing is warranted and needed now, as part of the site proposal process. While we agree that efforts must be made to guarantee fair usage of the radio spectrum, we need informational ammunition in the form of site characterization in order to make informed requests for radio quiet zones. However, many people perceive that the real solution to RFI is to learn how to excise it using sophisticated techniques, if possible, and if not, to choose a quieter site.

*Summary:*

*A loose collection of projects that, while certainly supporting the SKA effort, could also be considered as individual projects. This proposal is too expensive to be this diffuse.*

The proposal was perhaps diffuse, but we feel that its context is not. By context we mean the array of documents alluded to in §1 (and in the ATI2002 proposal) that describe the overall development effort.

*Panel Summary:*

*Concerns:*

*The proposal presents a poorly focused program for achieving its goals. It lacks coherence. Several loosely affiliated projects appear to have been bundled together. The relationship of the projects to the SKA are made only tenuously at best. Typically they are related to a technology that the SKA must address, but that is the only linkage given. The panel wanted to see why these were the right projects and how specifically the US SKA concept was being developed or tested. How did they fit in to other existing efforts in the US and elsewhere? How do the projects move us toward the final US concept?*

**The subprojects were not simply bundled together. Rather, they emerged from a two-year process of discussions about the development effort for the SKA and, in particular, for the Large-N-Small-D concept being pursued by the US SKA Consortium. Table 3 presents a list of critical tasks and activities that are needed for compelling proposals that will be submitted to the International group (the ISSC) over the next few years. The two proposals are for the SKA concept and for a North American site. All the efforts we have proposed for feed into the development plan.**

*Given the US SKA technical goals and priorities, what has already been done? What remains?*

*What is needed first is the US SKA technical goals and priorities. Then a coherent plan with priorities of how specifically to address them. Certainly the first step should be an evaluation of the existing state-of-the-art, of ongoing efforts, and of the new projects to either bolster ongoing efforts or start new ones. For example what are LOFAR and ATA doing about RFI? Why is this proposal starting a new effort at Arecibo? How does it all fit together?*

**Table 3 designates a list of technical elements (column 1), the critical tasks associated with those elements in column 2, and an indication of which of those tasks are addressed by the work being funded in ATI2001 and which would be addressed by funding under ATI2002 and by future requests to the NSF. We also indicate in the last column those facilities that pertain to the development effort. For example, the first entry, Configuration, is being addressed at a modest level in ATI2001 through funded efforts by Colin Lonsdale at Haystack/MIT. He is also involved with the LOFAR project and receives input from Douglas Bock at UC Berkeley, who is involved with the ATA project. Issues being investigated for the SKA include finding distributions of antennas that best satisfy the science goals that have been identified for the SKA. These studies would not happen automatically just because the ATA and LOFAR are arrays. RFI studies are indeed a large part of the ATA and LOFAR projects. But the anticipated frequency ranges of the SKA (in the LNSD concept) and the other two instruments are quite different. As stated earlier, the SKA will be used in single dish modes for some applications which are high on the list of science goals (e.g. blind searching for transients, pulsars and ETI). We need to investigate RFI excision for such modes of operation and Arecibo is an ideal platform for doing so.**

*In addition to the lack of specifics regarding the project's role in the development of the US concept, very few details were given about the projects themselves.*

*Some examples:*

*- RFI Mitigation: How does this relate to the efforts at the ATA and LOFAR? Why are we not building on those efforts or supporting them directly? The proposal should at least review them. How do the single dish efforts sketched here apply to SKA? What are the algorithms? An astronomical context is good, but its relationship to SKA goals must be made clear.*

Hopefully, our comments elsewhere in this document will answer these questions. We believe that we are building on the ATA and LOFAR efforts. One institution (Haystack/MIT) receives funding from ATI2001 to bridge expertise relevant for the SKA from the LOFAR project. UC Berkeley originally was to have received funding under ATI2001 for the purpose of extending the feed design for the ATA to higher frequencies. This work was cut under the reduced funding level and we felt that it could be deferred. Currently, feed designs are being considered by ATI2001-funded work at Cornell (Germań Cortes) and at JPL (Sandy Weinreb). The effort so far is at a minimal level and will need to be increased as time goes on. We didn't request funds for feed efforts in ATI2002 because work was already ongoing and we felt that we could wait for the ATA feed to mature. In a year or two, we may need to request additional funds for feed development should the two existing candidates (the ATA feed and the TRW feed) prove inadequate. The ATA feed will, of course, be tested on the ATA but the TRW feed will not. For this reason (in part) we wish to test the feeds at Arecibo while, if possible, getting some scientific payoff at the same time. But please note that the scientific payoff is not the main motivation for using the feeds at Arecibo.

*- EoR: It is nice to have a definite scientific target to test the RFI development. As stated above, however, the relationship to the technical US SKA development was not clear.*

The main tie to SKA technical development is with respect to RFI mitigation algorithms, particularly the cross correlation technique, which is interferometric in nature and scales to antenna arrays.

*- ATA feed on Arecibo: The plan is to put the ATA feed on Arecibo, and maybe also try another feed from Weinreb. How does this apply to moving the SKA forward?*

As stated earlier, Arecibo provides a test bed for both feeds (ATA and TRW). It is possible that additional broadband feeds will be designed over the next few years. To date the performance of the ATA feed has been assessed with computer modeling and field tests on an antenna test range. Astronomical tests are more demanding and provide truly bottom line performance assessments. E.g. what are the resonances vs. frequency, if any, and how does the polarization performance evolve with frequency. Experience at Arecibo shows that relatively narrowband feeds show resonances and that cross polarization is significantly worse at some frequencies than others. Polarized broadband sources such as pulsars and AGNs provide an excellent opportunity to diagnose feed performance. To observe these requires a large collecting area.

*- Fiber optic network: It is proposed to survey the state of the field as well as test some of the leading technologies. A survey of the state of the art seems sufficient, including the ongoing developments at NRAO on E-VLA. The communications and computer industries will develop this field rapidly.*

We agree that a survey of the field is important, but we consider field tests using commercial packet-switched networks to be an important activity. Fiber costs are a non-negligible portion of the overall cost of the LNSD concept. To reach a realistic cost estimate we need to understand the technical tradeoffs of data transport.

*- Polarization specifications: The SKA should have a justified polarization specification. They request funds for 15% of a postdoc for three years to do simulations. What are the guidelines for these simulations? That is, what are the scientific and RFI needs that will lead to the polarization specification?*

We agree that specific scientific and RFI needs are essential before one can develop a polar-

ization specification. We plan to address this concern through close interaction and iteration with the ISAC (of which Co-I Gaensler is a member). Specifically, we will begin by considering those scientific goals formulated by the ISAC which require polarimetric capability, and solicit from ISAC members information on the corresponding polarized properties of the sources under study, and the measurements which need to be made. We will then develop software to simulate the appropriate polarized sources and the propagation of their signals through the interstellar medium. Once we have generated synthetic data-sets, we will simulate the detection and analysis of these data with a LNSD array — this will incorporate the effects of instrumental polarization, beam/bandwidth depolarization, RFI, incomplete  $u - v$  sampling and calibration. We anticipate that this work will be carried out in close collaboration with the MIT/Haystack group, who are building a comprehensive software package to simulate potential SKA observations.

Using such simulations, and through iteration and consultation with the ISAC, we can determine whether the angular resolution, frequency resolution, dynamic range, surface-brightness sensitivity and polarization purity provided by a particular set of telescope and array parameters will be sufficient to make the proposed measurements.

Calibration and deconvolution strategies for analysis of polarimetric data are usually different from those for total intensity observations, and it is expected that a similar situation will apply for the SKA. A variety of new polarimetric analysis routines have been recently made available as part of the `imagepol` package within AIPS++, while a task to apply maximum entropy deconvolution to polarimetric data is now available within MIRIAD. We plan to conclude our proposed research effort by testing algorithms such as these on the synthetic data-sets as described above. Such efforts will establish whether there will be serious limitations in applying current calibration and deconvolution techniques to polarization data, paving the way for more detailed algorithm development once a specific SKA concept has been chosen.

We propose to carry out this research in close conjunction with other, separately-funded, efforts, in which we are studying the distribution of polarized emission in the Milky Way and Magellanic Clouds and the subsequent propagation of polarized radiation through intervening material. The data obtained as part of these on-going polarization experiments will be invaluable for developing and testing realistic simulations.

*- Site survey and selection: RFI survey: This can be done now. However, what will the RFI environment be like in 20 years? Population projections also an more important consideration in the site selection and should be addressed. What is being done for LOFAR for site selection? How does that fit into the SKA plans?*

Field tests are beginning for LOFAR and our plan is to jointly make such tests. LOFAR-only tests will cover only up to 240 MHz. SKA-relevant tests require up to 22 GHz or slightly higher to 25 GHz. In terms of logistics, personnel are needed to conduct these measurements. Finally, not mentioned in our ATI2002 proposal is the prospect of contributing to the international effort of assessing sites. It is entirely possible that the LNSD design concept for the SKA will be implemented but at a non-North-American site. In the short term we of course will promote the NA site. But hardware we purchase and methodology we develop can represent a contribution to a near-future international site-testing effort as well.

*Summary:*

*While the SKA is an extremely exciting and ambitious project, it is not clear how the research proposed here will move the US SKA collaboration closer to their goal of defining the US SKA concept design by 2007.*

**Our comments and answers to specific questions are intended to address the primary concern of the panel about lack of clarity on how the proposed work fits into the larger scheme of a US SKA effort and the international plan for decision making on the SKA.**

#### 4. Summary of Work Plan

The work plan for the ATI proposal submitted in 2002 includes several pieces of a much larger plan for evolving a very specific design concept for the SKA, one involving a large number of small-diameter antennas. The larger effort also includes site characterization in support of a site proposal to the International SKA Steering Committee. Table 3 shows most of the key areas needed for realizing a detailed plan for the SKA. As can be seen, the first ATI proposal and the current ATI proposal cover only a fraction of the tasks. What will be needed well in advance of concept selection (2007) is prototyping of SKA-specific 12m (nominal) antennas, with feeds and receivers as needed to cover the anticipated frequency range. For the time being, antenna designs and feeds and receivers are being pursued at a low level through some of the effort funded under ATI2001 and also as spinoff from the ATA and from investigations for using arrays in the upgrade of the Deep Space Network. Those efforts are of great importance to our SKA work plan, but eventually we will need funding for SKA-specific hardware. We also point out that the ATA, itself being a Large-N-Small-D concept, can serve as an extremely important test bed for some aspects of our SKA concept. In order for this to be so, it must be outfitted with a correlator in order to allow astronomical imaging applications.

## Appendix: Postcorrelation RFI Excision

The key concept in the “post-correlation” approach to RFI mitigation is that the RFI contribution to measured astronomical spectra can be accurately estimated using the integrated cross-spectra between a reference antenna and the primary (original astronomical) feeds. It also turns out (as will be pointed out below) that a combination of the integrated cross-spectra from the *primary* antennas can be used in lieu of a reference antenna, eliminating the need to point an antenna at the RFI.

A simple example goes as follows: Consider a “primary” voltage signal  $s_1(t)$ , consisting of the desired astronomy and the corrupting RFI. Consider also a reference antenna which has approximately isotropic (or less) gain toward the astronomical signal and isotropic (or better) gain toward the RFI source. Let the voltage signals from two orthogonal (but not necessarily perfectly-so) polarizations of this reference antenna be  $s_3(t)$  and  $s_4(t)$ . The desired product is normally the integrated power spectrum:

$$P_{11}(\nu) = \langle S_1(\nu)S_1^*(\nu) \rangle \quad (1)$$

which could be, for example, the output of an FFT spectrometer, where  $S_1(\nu)$  is the FFT of  $s_1(t)$ . Briggs, Bell, & Kesteven (2000, AJ, 120, 3351) showed that under relatively weak (i.e., easy to satisfy) assumptions, the RFI contribution to  $P_{11}(\nu)$  expressed in terms of integrated *cross-spectra* is given by

$$P_{13}(\nu)P_{14}^*(\nu)/P_{34}(\nu) \quad (2)$$

where  $P_{13}(\nu)$  (for example) is defined as  $\langle S_1(\nu)S_3^*(\nu) \rangle$ . Thus, RFI mitigation using this strategy consists of computing the integrated cross-spectra  $P_{13}(\nu)$ ,  $P_{14}(\nu)$ , and  $P_{34}(\nu)$  in parallel with  $P_{11}(\nu)$ , and (either in real time or as a post-processing operation) computing the term shown in (2) and subtracting it.

The simplicity and flexibility (in the sense that it works the same way somewhat independently of RFI waveforms) make this strategy very compelling. Initial experiments with this strategy, performed mainly by ATNF, have yielded very encouraging results. At the same time, the strategy has some known weaknesses which must be addressed. Key among these is that it is assumed that neither the desired astronomy nor some other source of correlated noise appears simultaneously in the primary and reference signals. To the extent that this assumption is violated, the performance will be degraded; thus, it is important to understand if and how this comes to be in actual observations. Another issue is that the performance of the technique is limited by the interference-to-noise ratio in the reference signals; for example, much better results are expected for a high-gain reference antenna pointed at the RFI source than for an isotropic horizon-monitoring antenna.

Once the impact (if any) of these limitations is quantified, part of our proposed effort is to develop and implement improvements to the basic technique which overcome these problems. A promising approach involves an eigenanalysis of the cross-spectra (also known as *principal components* or *Karhunen-Loève decomposition*) with the goal of *retroactively* improving RFI estimates (just as if a higher-gain antenna had been pointed at the RFI source(s)) and *retroactively* nulling problematic correlations between elements (e.g., nulling the astronomy contribution in the RFI reference signals to mitigate potential self-cancelling). Relevant theory (albeit presented in a slightly different context) is articulated in Ellingson & Hampson (2002), *IEEE T. Ant. & Prop.*, 50, 25. PI Cordes also has experience using principal component methods in data analysis of pulsar signals.

TABLE 3: CRITICAL TASKS AND ACTIVITIES FOR THE US SKA CONSORTIUM

Element	Critical Task	ATI2001	ATI2002	Future NSF Request <sup>†</sup>	Other/Comments	
Antenna Design	Size as minimum in cost curve			•	DSN work	
	Symmetric vs. off-axis paraboloids	•			DSN = symmetric antenna	
	Accommodation of feed(s)	•			DSN work	
	Mount			•		
	Manufacturing & transportation	•		•		
	MTBF & maintenance costs	•		•	DSN	
	Frequency range from science drivers		•	•		
Feeds & Optics	Spillover	•		•		
	Mechanisms for multiple feeds				ATA, DSN	
	Spectral & polarization performance		•			
	LNA connections & cooling			•	ATA, DSN	
	Number needed for frequency range	•		•		
	Noise performance vs. wide bandwidths	•		•	Arecibo, ATA, DSN	
	Noise performance vs. cooling costs			•	ATA, DSN	
Receivers (LNAs)	Linearity vs. RFI			•	ATA, DSN, VLA	
	Number needed for frequency range			•		
	RFI Mitigation	Achieving noise-limited sensitivity	•	•	•	
		Site characterization		•	•	EVLA, LOFAR
		Single site, multibeam context		•		Arecibo
		Dual site context	•			Arecibo + GBT
		Small N interferometry				VLA
Large N beaming & interferometry				ATA + correlator		
Small N with SKA antennas/feeds/Rx			•	Small SKA prototype		

<sup>†</sup> Future requests may be within the ATI program or other programs, as appropriate, and refer to the development phase of the project.

TABLE 3: CRITICAL TASKS AND ACTIVITIES FOR THE US SKA CONSORTIUM (CONTINUED)

Element	Critical Task	ATI2001	ATI2002	Future NSF Request	Other/Comments
Configuration	Optimize mapping on different scales	•		•	ATA, LOFAR
	Blind survey capability over full FOV	•		•	LOFAR
	Land costs			•	EVLA
	Maintenance costs	•		•	
Data transmission	Dedicated fiber costs (short baselines)				EVLA
	Packet switched network (long baselines)		•		VLA
Digital Signal Processing	Operating modes for imaging science			•	VLA, ATA?, LOFAR
	Operating modes for non-imaging science	•		•	ATA, LOFAR
Systems Analysis	Correlators & beam formers			•	ATA, LOFAR, prototype
	Maintenance costs	•		•	ATA, LOFAR, VLA
	Cost equation	•		•	DSN
	Data management			•	Arecibo, ATA, LOFAR
Project Analysis	Multiplexing/operating modes	•		•	ATA, LOFAR
	Science goals		•	•	ISAC
	Synergies with other $\lambda\lambda$ & non-EM science			•	ISAC
Prototyping	Large N technology and performance				ATA, LOFAR
	12m antenna manufacturing			•	DSN
	Correlators			•	ATA, EVLA, LOFAR
	Widefield transient searches				LOFAR
	RFI excision techniques	•	•	•	Arecibo, GBT, ATA, LOFAR, EVLA
	SKA-specific paraboloids			•	In small-N array
	SKA-specific feeds			•	In small-N array
SKA-specific LNAs			•	In small-N array	
North American Site Proposal	RFI Monitoring + other characterization		•		w/ EVLA, LOFAR
Outreach	TBD			•	cut from ATI2001

Table 4: Hardware Budget for the EoR Portion of the Project

Item	Comment	Qty	Unit Cost	Cost
Antennas	Built at Arecibo	5	1k	5k
Catwalk mount & design		1	5k	5k
Amplifiers	Two per receiver	10	0.25k	2.5k
Filters	RF Bandpass	5	0.3k	1.5k
Fiber Optic Links		10	1k	10k
Fiber	1000 ft + 2000 ft		2k/1000ft	6k
IF/LO system	mixers, oscillators	1	5k	5k
Digital sampling system (for input to backend)				5k
Miscellaneous parts (connectors, housings, spares, etc.)				10k
				$\Sigma = 50k$
Digital backends	FFT Spectrometers	5	2k	10k
	Correlator (5 'stations')	1	10k	10k
Prototyping costs				7k
Data acquisition & analysis	Computers	3	2k	6k
				$\Sigma = 33k$