Report of the MeerKAT Large Project Review Panel – 2017

I. Background and Motivation for a Review

In 2009 the Square Kilometre Array (SKA) South Africa (SA) office released an open invitation to the international astronomical community, calling for key science projects to be carried out on the SKA precursor MeerKAT (Booth et al. astro-ph/0910.2935). A committee was formed in 2010 chaired by J. Lazio (JPL) to review these proposals and recommend a large science program for MeerKAT. The committee chose ten Large Survey Projects (LSPs), defined as requiring more than 1000 hours of telescope time over five years, and expected approximately 70% of the observing time to be allocated to LSPs during that span.

This strong approved MeerKAT science program had the effect of galvanizing early support for the Karoo array telescope. Equally important, the science requirements of the original Priority Group 1 science proposals guided some of the subsequent detailed technical specifications of the telescope. The final design choices for MeerKAT were markedly different than those in the original 2009 request for proposals. Important modifications included the number (64) and the size (13.5 m) of the antennas. Also, the maximum baseline changed from 60 km to 8 km. For receivers, the final L band design had much better noise performance than was assumed, while X band receivers are not currently planned. The currently planned suite of receivers include UHF (0.58–1.0 GHz), L band (0.9–1.67 GHz) and S band (1.75–3.5 GHz).

In mid-2016, recognizing these changes, and with the telescope in an advanced state of construction and commissioning, the SKA SA office requested updated science project plans from eight of the original ten LSPs. These eight LSPs are listed below. Two of the remaining LSPs (MESMER and MeerGAL) were not required to submit revised plans since they primarily required X-band, which is not currently planned.

1. *LADUMA: Looking at the Distant Universe with the MeerKAT Array*
2. *MALS: The MeerKAT Absorption Line Survey*
3. *MeerKAT Fornax Survey*
4. *MeerTime: The MeerKAT Key Science Project on Pulsar Timing*
5. *MHONGOOSE: MeerKAT HI Observations of Nearby Galactic Objects: Observing Southern Emitters*
6. *MIGHTEE: The MeerKAT International GHz Tuned Extragalactic Exploration Survey*
7. *ThunderKAT: The Hunt for Dynamic and Explosive Radio Transients with MeerKAT*
8. *TRAPUM: Transients and Pulsars with MeerKAT*

II. Panel Evaluation Process

A review Panel was assembled by the SKA SA Chief Scientist in conjunction with the SKA SA Science Committee consisting of seven astronomers with broad expertise. The members of the Panel were:

- Stefi Baum (University of Manitoba)

- H. Cynthia Chiang (University of KwaZulu-Natal)
- Jim Condon (National Radio Astronomy Observatory)
- Dale A. Frail (National Radio Astronomy Observatory; Chair)
- Roy Maartens (University of Western Cape)
- Naomi McClure-Griffiths (Australian National University)
- Paul Ray (Naval Research Laboratory)

Each member of the Panel received the following material in advance of the face to face meeting:

- Revised project plans for all eight LSPs
- Terms of reference describing the proposal process
- MeerKAT specifications and antenna release schedules
- A summary of technical issues for each LSP
- A list of those components in each LSP that should not be reviewed
- Detailed evaluations for each LSP from 6-7 anonymous subject experts, aka “Readers”; one set of Readers evaluated the time-domain proposals, another the line/continuum.

Panel members read all the material supplied. In addition, each LSP was assigned a Primary and Secondary reviewer from the Panel. Prior to the face to face meeting Primary and Secondary reviewers were responsible for producing an initial draft report for the LSP based on their own assessment and that of the Readers. At the face to face Panel meeting the Primary led the discussion of each LSP, and the Secondary kept notes on the Panel’s discussion of the proposal. The detailed reports for each LSP in Section IV are the consensus view of the Panel, written by the Primary and Secondary after this face to face discussion. They are listed in the order in which they were discussed by the Panel. Dr Chiang could not be present at the face to face meeting but sent in extensive reports for her primary and secondary LSPs and contributed to the review of the final report.

At the conclusion of these discussions, the Panel produced a rank-ordered list of science projects. Initially, the 20 components of all three time-domain proposals (MeerTime, TRAPUM, and ThunderKAT) were discussed and ranked separately from the 9 components of the five line/continuum survey proposals (MALS, Fornax, MHONGOOSE, LADUMA, and MIGHTEE). This separation ensured that the same Readers had reviewed each set of proposals under discussion and the Panel’s discussion was comparing broadly related science topics. Prior to voting the Panel merged the four MALS components into two, and merged three of the TRAPUM components into one (see Section IIIb for details). The final list had 18 time domain components and 7 line/continuum survey components.

Voting was done by each Panel member assigning a 1 through 18 to the time domain components, and separately assigning a 1 through 7 for the line/continuum components. The sum of the Panel scores provided a ranking. The outcome of this ranking was a list of A, B and C-ranked components for the time-domain and line/continuum surveys separately. These rankings are described in more detail in the next section, but they represent the three bands of recommendations by the Panel. The final merge of time domain LSPs with line/continuum survey LSPs was carried out by having each Panel member anonymously ordering all A-ranked components from top to bottom following the numerical scoring system as above. The same process was followed for the B- and C-ranked components separately. The original A, B and C rankings are preserved in the merge but within each rank is a prioritized list. The numbers of components in each rank and the total time requested within each rank for the time-domain and line/continuum surveys are given in Section IIIa.

III. Rank-Ordered MeerKAT Large Science Program

a. Explanation of Rankings

Table 1 provides a rank-ordered list of the recommended MeerKAT large science program along with the mean Readers' scores (where 4 indicates "excellent" scientific merit, 3 "very good", 2 "good", and 1 "fair"). Individual LSPs and components of LSPs are each given an A, B, or C ranking.

An A-ranked proposal or component represents high impact science that is uniquely well-matched to the capabilities of MeerKAT. Some LSP projects may still have some technical risk associated with them achieving their science goals. In these cases, the Panel has taken the approach of recommending that if possible SKA SA award the full time allocation provided the teams can meet certain milestones. The risks and requirements are given in more detail in the next section on individual LSP evaluations. Examples include demonstrating noise-limited dynamic range for deep continuum images (e.g. MIGHTEE), and achieving the required accuracy and stability for pulsar timing noise (MeerTime). Four time domain components and three line/continuum surveys are A-ranked, requesting a total of 4658 hrs and 5974 hrs, respectively. The total time request considered for all time-domain LSPs was 9426 hrs and for the line/continuum LSPs (where the MALS request is the adjusted version noted in Section IIIb) it was 11294 hrs, for a total of 20720 hrs.

A B-ranked proposal or component is still excellent science but the proposals were not ranked as high either by the Readers or the Panel. This may be due to weaknesses that were identified other than the scientific merit. This may include technical aspects, a perceived mismatch to the uniqueness of MeerKAT, or the level of resources required. In some cases, the Panel has recommended approval of these science programs if possible but with reduced telescope time. Note, MIGHTEE (S band) lies on the dividing line between B and C-ranked proposals. The Panel felt that the complementarity of the science with the higher-ranked MIGHTEE (L band) warranted including it in the project components recommended for telescope time. Seven time domain components and three line/continuum survey components are B-ranked, requesting a total of 2498 hrs and 4247 hrs, respectively.

If all A and B ranked projects receive their full allocation it seems likely that the allocation to LSPs will exceed the proposed 70% of science time in the first five years of MeerKAT. We discuss this topic further in Section V.

The Panel does not recommend awarding time to the C-ranked LSP proposal components. Some projects remain strong scientifically and are well-suited for a regular proposal call. Others have sub-optimal science cases because of changes which have occurred in either the baseline configurations or with the planned suite of receivers since the original 2009 request for proposals. Seven time domain components and one line/continuum survey component are C-ranked, requesting a total of 2270 hrs and 1073 hrs, respectively.

Table 1. Rank-ordered list of the recommended MeerKAT large science program.

Large Survey Project (LSP) Components	*Requested Time (hrs)*	*Readers' Science Score*	*Panel Ranking*
MeerTime (binary)	1440	3.87	A
MHONGOOSE	1650	3.55	A
MeerTime (MSPs)	2160	3.58	A
LADUMA	3424	3.84	A
Fornax	900	3.41	A
TRAPUM (Fermi sources)	338	3.60	A
MeerTime (1000 PTA)	720	3.78	A/B
ThunderKAT (CVs)	250	3.42	B
MIGHTEE (L band)	979	3.23	B
ThunderKAT (GRBs)	330	3.42	B
MeerTime (GCs)	1080	3.38	B
MALS (UHF band)	2320	N/A	B
TRAPUM (nearby galaxies)	226	3.28	B
TRAPUM (GCs)	320	3.22	B
TRAPUM (SNR, PWN, TeV)	92	N/A	B
ThunderKAT (SNe Ia)	200	3.08	B
MIGHTEE (S band)	948	2.77	B/C
MeerTime (magnetars)	100	3.00	C
TRAPUM (Fly's Eye)	720	3.33	C
ThunderKAT (XRBs)	500	3.00	C
MALS (L band)	1073	N/A	C
MeerTime (young PSRs)	400	2.83	C
ThunderKAT (ULXs)	100	2.75	C
ThunderKAT (Novae)	150	2.75	C
ThunderKAT (CC SNe)	300	2.75	C

Notes to Table. See the individual LSP reports in the next section for more details.

1. For MALS (UHF) see recommendation for reduced observing time.
2. ThunderKAT GRB recommendation based on targeting only short hard bursts.
3. ThunderKAT SNIa recommendation based on limited distance D < 10 Mpc.

b. *Explanation of Key Panel Decisions*

Note that the Readers' scores in Table 1 do not have a one to one matching for MALS and for one part of TRAPUM. In these cases, in the place of Reader scores we have inserted "N/A".

For TRAPUM the Readers were asked to give a separate science ranking for pulsar searches in SNRs, PWNe and TeV sources. The Readers' scores for the three components of TRAPUM were: PWNe=3.08, TeV sources=3.08, and SNRs=2.53. The Panel decided to merge these three topics, voting on them as a single block of targeted searches for young pulsars. In any case the total amount of time is small (92 hrs). The consensus reviews in the next section follow the original science topics requested to be reviewed.

MALS was reviewed by the Readers by breaking up the request into four science topics. The Readers' science scores for the four components of MALS were: Cold gas=3.00, AGN=2.64, ISM/cosmology=2.14 and fundamental constants=1.82. However, since the source lists remain the same over all topics, the Panel felt that breaking up the review by frequency (L band and UHF) was a better way to compare the science returns. The L band science component has more competition from other facilities, while UHF remains a unique MeerKAT capability. The consensus reviews in the next section follow the original science topics.

The total requested times in Table 1 for the UHF and L band components of MALS were adjusted using the revised integration time per source as given in the "Findings of Technical Review of proposal done by SKA SA". At L-band the single source integration time was revised from 80 min to 56 min. Thus, for 1000 pointings, 56 min integration per source = 933 hrs + 15% overhead, for a total of 1073 hrs. At UHF the single-source integration time was revised from approximately 95 min to 121 min. Assuming 1000 pointings, 121 min integration per source = 2017 hrs + 15% overhead, for a total of 2320 hrs.

Among the A-ranked projects there is good correspondence between the Readers' scores and those of the Panel. This agreement simply reflects the strong science return from these projects. There are some small deviations. For the survey proposals, LADUMA was slightly down-rated below MHONGOOSE. While both are well matched to the capabilities of MeerKAT, it was the Panel's view that MHONGOOSE (and Fornax) will likely start to deliver science results in the form of quality images before LADUMA, which makes a single deep integration on the sky. For the time domain proposals the MeerTime 1000 pulsar array was also down-rated relative to the Readers' scores. While undoubtedly of great and lasting legacy value, much of the science return from this experiment really only occurs after monitoring has been going on for more than 5 years. In contrast, the MeerTime MSP and binary components can be generating high impact science returns almost immediately.

In the B- and C-ranked components, any deviations from the final Panel rankings and the Readers' science scores were due to the Panel considering additional risk-adjusted factors. The Panel's rankings are a consensus view based in large part on the scientific merit, but we also discussed and voted based on the other factors as listed in the consensus review (e.g., feasibility).

Among the B-ranked projects the two that have suffered the most from MeerKAT's design changes and receiver choices since 2010 are MIGHTEE and ThunderKAT. As we note in the next section, the lack of long spacings affects the original MIGHTEE science program, and much of the remaining ThunderKAT science case is best done above 3 GHz. MIGHTEE should be given the opportunity to demonstrate in early science that they can achieve the dynamic range they require. For ThunderKAT we have suggested some restrictions that should optimize the science return of slow transients at L or S band. There may be rare, individual slow transients that are bright and/or nearby that lend themselves to study at low frequency. For those that are C-ranked, we suggest that these cases be looked at on a case-by-case basis as part of a regular call or Director's Discretionary Time.

Among the C-ranked proposals the Panel's rankings largely track the Readers' science scores with one exception. The Panel downgraded the TRAPUM Fly's Eye component and we do not recommend that it be scheduled on the telescope. The field of fast radio bursts (FRBs) is important and fast moving. However, it is the Panel's view that relative to other world-wide efforts, the MeerKAT Fly's Eye effort will not be sufficiently competitive on a short enough timescale to warrant strong justification of this development over other enhanced MeerKAT capabilities (e.g. zoom/narrow band imaging mode for HI observing).

IV. The MeerKAT Large Survey Projects

a. MALS: The MeerKAT Absorption Line Survey

1. Scientific Merit

1.1. Evolution of cold gas in galaxies and relationship with SFR density

MALS proposes to observe intervening HI absorbers over the redshift range z~0–0.6 (L band, less 0.09–0.22 due to GNSS RFI) and z~0.4–1.45 (UHF) to constrain the evolution of neutral gas in galaxies at z<1.5. This will be done via measuring the number of 21cm absorbers with redshift, n21(z). With some assumptions about gas spin temperature and disk covering fraction, these will be converted to Omega_HI(z). At present, Omega_HI(z) measurements over the MeerKAT redshift range are limited to a handful of systems, with z>2 measurements obtained by other means (damped Lyman absorbers, Lyman limit systems, MgII absorbers, etc.). MALS will increase the number of intervening HI absorbers by factor of 5–10. The proposal states that if n21(z) scales with Omega_HI(z) MALS will measure the evolution of cold neutral medium (CNM) cross-section in galaxies at the 3-sigma level. Measuring how the evolution of the cold gas evolves compared to the well-determined evolution in star formation rate SFR(z) is an important objective, and since this evolution is strongest from z~0–1.4, the combination of MeerKAT L-band and UHF-band observations is well situated to probe the connection between the SFR and the available fuel.

An additional aspect of the proposal involves observations of OH absorbers to constrain the evolution of molecular gas with redshift. This is perhaps a unique contribution but not strongly developed in the proposal as little is known about the expected detectability rate of OH absorbers. It would have been interesting to see this developed further in the proposal.

If the ~4000 hr requested is approved, the authors predict that an order of magnitude more HI absorbers will be detected than are currently known, allowing the n21(z) to be measured with a 10% accuracy and therefore the CNM cross-section of galaxies to be detected at the 3 sigma level. If instead the program is reduced to target only S>400 mJy sources, at most half as many intervening absorbers will be detected. The reduced time allocation will reduce the significance per redshift bin of the Omega_HI(z) measurements.

There are some details that are missing in the proposal that may potentially limit the science return. Specifically, to measure Omega_HI(z) requires measurements of the HI column density which in turn require estimates, or at least assumptions, about spin temperature of the gas and the covering factor of the gas. The proposal contains very little information about the assumed covering factor of the foreground absorbers, relevant as many of the radio sources would be extended, on spatial scales of tens of kiloparsecs at the absorber redshift (e.g. Curran & Webb 2006, MNRAS, 371, 356; Curran 2012, ApJ, 748, L18) and the absorbers would only cover a fraction of the background radio emission. Additionally, the spin temperature can either be measured by Ly-alpha or assumed, but both will limit the survey's ability to measure Omega_HI(z) well.

1.2. Fueling of AGN, AGN feedback and dust-obscured AGNs

This science theme will be addressed by observations of intrinsic HI and OH absorbers associated with the radio continuum targets at z<2 or other radio continuum sources in the field, providing information on the relationship between the *in situ* gas and the host AGN. The proposed observations are expected to increase the number of associated absorbers by a factor of 10 (if using observing time dt~4000 hr as requested in the proposal) or 5 (dt~1500 hr, as per the alternative noted in Section 5). This science theme is presented as secondary to the evolution of Omega_HI(z), but has the potential to have an interesting impact on AGN studies by providing direct information of fueling and feedback. The blind nature of the survey may also provide some estimate of the number of dust-obscured AGNs.

Of concern in estimating the number of absorbers is that some earlier absorption studies have found that the detection rate of 21cm absorption is much lower at z>1 than at low redshifts, z<<1. For example, it has been shown that the detection rate of associated 21cm absorption is lower at high redshifts and/or high ultraviolet/radio AGN luminosities (Aditya et al. 2016, MNRAS, 455, 4000) for flat-spectrum sources. Selection effects dictate that for a flux density limited sample of QSOs, those at large redshifts will be more luminous. The environment near a luminous z=1.5 QSO is going to be very different than a QSO at z=0.15, affecting the detectability of HI local to the QSO. The MALS proposal assumes a 30% detection rate at all redshifts, which ignores redshift evolution. The number of detections may vary enormously from what has been assumed, however this evolution could be scientifically interesting in itself. An important point, however, is that this science topic is a natural by-product of the main science goal and cannot be judged separately. It is good auxiliary science.

1.3. Constraining space- and time-variation of fundamental constants

The proposal states that it would be possible to use the 21cm and OH absorbers detected in the MALS survey along with other lines detected via spectroscopy on other telescopes (ALMA, VLA, NOEMA, etc.) to probe fractional changes in alpha and mu at the level of 1.0e-7, extending the sensitivity of current studies by an order of magnitude. This is a “value added” component to the main MALS science goal and is not a suitable goal in and of itself, nor is MeerKAT essential to do this science.

Unfortunately, this science case is not well-developed in the proposal. There are significant questions left open such as how these 21cm redshifts can be compared to the redshifts of low-ionization metal lines in optical spectra to derive constraints on changes in alpha and mu, or alternatively, how many OH absorbers will be detected in this survey, and whether the background radio sources will be bright enough at cm wavelengths for searches for the redshifted methanol or ammonia lines. Given that the current sensitivity of probes of fractional changes in mu is actually already at the level of 1.0e-7, from methanol lines at z=0.89 (Bagdonaite et al. 2013, Science, 339, 46; Bagdonaite et al. 2013, Phys Rev Lett 111, 231101), it is difficult to see how MALS will contribute significantly to this field.

1.4. Physical modeling of the ISM, astrochemistry and cosmology

There are several science goals embedded here that will make use of multi-wavelength ancillary data from VLBI, HST, SALT and VLT. The first goal, physical modeling of the ISM, to constrain the physical conditions of the absorbing gas on pc scales, is a laudable goal that hopes to use VLBI to map the small-scale structure and HST/SALT/VLT to infer the radiation field, metallicity, etc. for ISM models. There are some questions about how many sources can actually be observed with these telescopes and whether there will be enough to show any meaningful evolution with redshift.

The cosmology scientific goal is even less developed but relies on discovery of rare molecular species to measure the evolution of CMB temperature with redshift. This, as it was presented, seems unlikely to be successful.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

The broad observational strategy, of using bright flat-spectrum radio sources as primary targets, and then searching for absorption against all other sources in the field of each bright source, is a reasonable one. The time estimates (corrected from the Technical Reports) per source also appear reasonable.

There is considerable uncertainty in the actual number of 21cm and OH absorbers that will be detected.

The Panel estimates that the number of off-axis detections of suitable compact absorbers may be overestimated in part because the source density estimate in the proposal appears not to include the declining sensitivity of the primary beam area and the fraction of FSRQs drops rapidly below 15% in L-band flux-limited samples for sources stronger than 15 mJy (flux limit from MALS Table 3; see Owen et al. 1983, AJ, 88, 1 and Condon 1984, ApJ, 287, 461).

2.2. Resources

The team are skilled and extremely capable in the primary science objectives. They have the expertise in the observational and scientific analysis of radio absorption line spectroscopy.

The data volume for the survey is expected to be large (24 PB if all time awarded). The team appear to have many of the necessary computing resources through IUCAA (already funded through 2018) and plan to request CHPC and IDIA processing and archiving resources. If these resources are secured the project should be technically possible. We recommend securing IDIA or equivalent resources soon.

2.3. Management

The team organisation is clear and well-planned. They are well connected with the APERTIF and ASKAP absorption surveys, which will help not just in survey execution but also in scientific interpretation.

The data dissemination plan includes three data releases, basic data as well as value-added products, pipeline reprocessing, and an additional archive (to the MeerKAT archive) for serving products.

The continuum and polarization auxiliary science do not sit at the core of the MALS team and therefore we suggest these data be made publicly available immediately after calibration and verification.

2.4. Technical Aspects

The impact of RFI has been assessed and accounted for in the estimated results for the absorption aspect of the proposal, which is the primary objective.

It is not clear whether RFI (both solar and local) has been taken into account for the suggested HI emission component of the project. However, given that HI emission is a low priority science goal, there is no strong motivation to request night-time observations.

Obviously, GSM transmissions at ~925–960 MHz will be of concern. The MeerKAT specs suggest that these will decrease over time. If this does not happen quickly it will have a negative impact on the MALS observations.

2.5. Scientific Aspects

The MALS proposal makes some assumptions about the covering factor of HI and the redshift evolution of associated 21cm absorption that are important in estimating the predicted number of 21cm absorbers. In addition, assumptions about the gas spin temperature can affect both the predicted number of absorbers and reliability of Omega_HI (z) measurements. The predicted number of 21cm absorbers, both intervening and associated, may be overly optimistic. If so, this could imply a significant risk to the science returns from MALS.

There is enormous uncertainty in the predictions for the number of OH absorbers, but as this is secondary science it does not imply increased risk.

2.6. Other relevant aspects

Ancillary data are not required for the primary science objectives (cold gas evolution and AGN fueling). However, ancillary data are required to finalise the target source list and we encourage the team to continue in those efforts.

Two of the low priority science goals (evolution of fundamental constants; modeling of ISM) depend critically upon auxiliary multi-wavelength data. The team plans to perform follow-up observations with ALMA, NOEMA, VLT, VLA, and SALT, but there is no discussion of how this follow-up will be ensured. It is clear that members of the team are active in using several of these instruments, but the lack of a detailed plan for obtaining the auxiliary data is a weakness of the proposal.

There is some risk associated with some of the proposed follow-up observations. For example, the HST follow-up spectroscopy of intervening 21cm absorbers is critical to determine the HI column densities of the systems, and hence to obtain the cosmological gas mass density. However, this would only be possible for AGNs that are bright in the ultraviolet, which would not be the case for most of the AGNs in a dust-unbiased sample. Similarly, it is not clear whether most of the target AGNs would have sufficient flux density at cm and mm wavelengths for follow-up molecular line spectroscopy. The LSP mentions stacking of HI 21cm emission from galaxies in the field. However, this requires optical spectroscopy to get accurate redshifts for individual galaxies, which is not likely to be available for most of the pointings.

3. Suitability and Uniqueness

MeerKAT's sensitivity and frequency coverage makes it an excellent choice for this project. In particular, the UHF-band observations offer a deep, high-redshift survey that extends beyond work possible with any other telescope, except possibly uGMRT (but at lower sensitivity). The lower redshift space is covered partially with ASKAP and APERTIF, but given that those telescopes will be less sensitive, the MALS work on probing the low optical depth absorption will be complementary.

4. South African Capacity Development, and Outreach

About 25% of the team is based in South Africa at a variety of levels and a similar fraction in the advisory board and working group chairs are from RSA. IUCAA, SKA SA, and UKZN have already started student training programs in galaxy evolution and optical and radio astronomy techniques, which should contribute to capacity development in South Africa. The Indo-SA flagship program for collaborative visits and co-supervision of students are very positive indications of the level of commitment to South African development.

The MALS team plans to use the science popularization programs of the partner institutes (e.g. IUCAA, SKA SA, UKZN) to spread awareness about the MALS project. They also plan to use a combination of public lectures, press releases, and educator (in the form of text, animations and videos) and school/college projects, in addition to information on the MALS webpage, to describe MALS science and results to the general public. The outreach program is relatively generic but the inclusion of short courses and student projects is good.

5. Recommendations and Miscellaneous Items

The Panel recommendation is divided into two parts: UHF and L-band. The proposed UHF observations at z>1 are unique to MALS and will provide exciting results. The Panel felt that the UHF band observations should be given higher priority over the L-band observations, and if possible, awarded the full-time request. The L-band observations are a deeper complement to ASKAP and APERTIF surveys without providing the unique edge that is available at UHF.

The Panel would also support an alternative experiment suggested by the SKA SA office in which only the brightest (>400 mJy) flat spectrum radio quasars are observed: 740 at L band and 370 UHF sources. With 15% overhead, we estimate this experiment to take 1655 hrs.

There are considerable uncertainties regarding the predicted number of absorbers to be detected, which could affect the science outcomes. It would be useful to monitor the progress of the survey, targeting the brightest sources first. This point is particularly applicable to the lower priority L-band observations, for which we felt at best only the brightest targets (>400 mJy) should be targeted (see above two paragraphs), with additional time contingent on the results. The brightest targets will give the best sensitivity to small optical depths.

The one-year proprietary period for the main data products is reasonable, especially for protecting young scientists in the program.

The updated case now includes continuum and polarization science, which overlap with other projects and are not at the core expertise of the proposers. For these specific data products, it would be advisable to eliminate the proprietary period and make the products available immediately.

The proposal tries to describe too much at the expense of adequate detail on some of the key points, specifically selection effects and detailed strategy for the calculation of Omega HI(z). The inclusion of ancillary data products (continuum and polarization) is fine, but text on this detracted from the proposal.

b. MHONGHOOSE: MeerKAT HI Observations of Nearby Galactic Objects: Observing Southern Emitters

1. Scientific Merit

The MHONGOOSE LSP aims to study the flow of gas into galaxies and its conversion to stars, as well as feedback effects of galaxies on their surroundings by carrying out deep HI 21cm studies of 30 nearby galaxies down to a 3-sigma detection limit of 7.5e18 per cm^2 at an angular resolution of 30'' (or 5.5e17 per cm^2 at an angular resolution of 90''). The broad aims are to study gas flows in and out of galaxies, to examine the connection between gas and star formation, to detect the cosmic web, and to relate the dark and baryonic matter content of the galaxies. The scientific objectives of MHONGOOSE are all critically important in the context of galaxy formation and evolution, and, indeed, are key science questions for the SKA. Earlier studies have been hampered by either a lack of high angular resolution (when done with single dish telescopes) or a lack of column density sensitivity (when done with interferometers). MHONGOOSE will be the first study that combines high column density sensitivity with excellent angular resolution, and thus is likely to take the first large step in studying accretion onto galaxies in the nearby Universe. The sensitivity of MHONGOOSE is about a factor of 50 higher than that of the previous best study, HALOGAS (which found that the cold gas accretion rate in a similar-sized sample contributes only ~ 10% of the star formation rate in the sample galaxies); this suggests that the bulk of the accretion arises at lower HI column densities, to which MHONGOOSE will be sensitive. The improvements that MHONGOOSE will provide in reaching lower column density and higher resolution are clearly illustrated in Figure 5 of the proposal.

Overall the science case is well written, and the proposal makes a convincing argument that the observing program can deliver qualitatively new HI images of a very carefully selected sample of nearby galaxies. Furthermore, MHONGOOSE complements the Fornax proposal, as it avoids dense regions where the determining physics is different. The proposed work would provide valuable new insights into the baryon cycle in galaxies and would be an important step forward beyond existing surveys by combining high angular and mass resolution studies of HI in nearby galaxies.

MHONGOOSE also has the potential to make the first detection of the cosmic web around field galaxies, and new insights into this connection would certainly be fascinating. However, there is some uncertainty in whether or not extended HI can be unambiguously associated with cold accretion. There are systems with perplexing HI extensions at higher column densities whose origin is not fully understood, and the proposers have not explained how high spatial resolution observations of such HI will establish this connection. In addition, the sample size increases the required observing time linearly, but the need for 30 systems (as opposed to 20, 25, etc.) to achieve the stated science goals is not justified in detail in the text. There is no clear discussion of how the connection to the cosmic web should behave for the different mass bins.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

The MHONGOOSE LSP aims to carry out deep studies of the HI 21cm emission from 30 field galaxies, selected from the SINGG sample, in six uniform HI mass bins between 10^6 and 10^{11} solar masses. A large fraction of the galaxies have gas mass significantly lower than 3e9 solar masses, where cold gas accretion is expected to be a dominant process. Strongly interacting and group/cluster galaxies have been excluded from the sample. All galaxies lie within a distance of 30 Mpc, implying that the MeerKat L-band beam would yield a spatial resolution of ~ 1 kpc at the distance of the targets. The galaxies have also been chosen to be uniformly distributed into edge-on, face-on and intermediate-inclination systems in each mass bin. The sample is well argued for the required observations and is justified by the column density limit required. There is some uncertainty in the justification of the sample size for the specific goal of testing the connection of galaxy assembly to the cosmic web; there isn't a clear discussion of what differences are expected between the 6 mass bins for a sample size of 30 galaxies.

The revised time request per galaxy appears reasonable to achieve the desired sensitivity goals. The observations of 30 systems would also provide a good sampling across various galaxy parameters, such as HI mass, star formation rate, rotational velocity, inclination, etc. Overall, the revised time request of 1650 hours (including 15% overhead) seems appropriate for the stated sensitivity and objects.

2.2. Resources

The LSP team has all the required expertise in a wide range of relevant areas, including HI 21cm imaging, multi-wavelength coverage, radio interferometry, and magnetic field estimates. The data volumes per galaxy are not very large (10 TB per galaxy) and the LSP team already have funding from ASTRON for the first two years to support the analysis. Funding has also been obtained from the US NSF and the ERC to support this project, and a Precursor Science Data Centre is being set up via a collaboration between the Netherlands, South Africa and IBM, that will help the processing of MHONGOOSE data. Overall, the LSP team appears to have the required physical resources to handle the data analysis, and there are good indications of institutional support.

2.3. Management

The project management plan appears reasonable. The early science goals involve testing different observing strategies as well as the temporal stability of the bandpass and polarization calibration, which will help the LSP team to refine the survey strategy. The approach appears quite realistic to the requirements and problems of a new telescope. The data analysis will be done using standard methods, using an initial HI pipeline calibration, followed by detailed imaging (most probably in CASA). The MHONGOOSE team are also collaborating with the Fornax, MIGHTEE and LADUMA teams for the data analysis, which is likely to result in a uniform data analysis strategy, which would benefit all the LSPs and MeerKAT. The collaborative work isn't described in much detail, although the plans can be inferred from the other proposals. There appear to be reasonable resources and expectations for data volume and handling.

2.4. Technical Aspects

There should not be any serious problems with the HI data, especially since the LSP team are being careful to test out both bandpass and polarization calibration in early science, and have also requested night-time observations for the project to avoid solar interference. Night-time observations are likely to be important for the project. The analysis should be similar to what was done for HALOGAS, so the work will be challenging but of little technical risk. The zoom mode of MeerKAT will be required for this project.

For the continuum imaging, the rms noise estimate of 0.75 microJy/beam is a factor of ~ 2.5 below the confusion noise for Stokes I; the Stokes I continuum images will hence be limited by source confusion but this will not be an issue for the polarization images. RFI contamination should be limited, although there isn't much discussion of how information loss in the band will affect the determination of rotation measures.

2.5. Scientific Aspects

The proposed survey spans regimes probed by earlier surveys, so the predictions for this work seem relatively robust. There are no significant assumptions about the source populations made in the LSP that are likely to incur risk. There is some uncertainty in how well the cosmic web might be detected and resolved, but this uncertainty does not pose a major risk to the project. There may also be some uncertainty associated with the magnetic fields project, as pulsar data in the Galaxy suggest significant field changes on kiloparsec scales.

2.6. Other relevant aspects

The 30 target galaxies are part of the SINGG sample and already have excellent multi-wavelength coverage (UV, optical and IR imaging, and H-alpha spectroscopy). The LSP team includes the PI of SINGG and members who will provide the WISE IR data and the S4G data. Overall, the plan for obtaining multi-wavelength data appears fine.

3. Suitability and Uniqueness

MeerKAT is the undoubtedly the most suitable instrument for this project, as MHONGOOSE science requires a combination of high angular resolution and high column density sensitivity. The best earlier studies in this field, based on the GBT and WSRT (as single dishes) and the WSRT interferometer, each had very different limitations: the GBT and WSRT single-dish study had a high column density sensitivity (1e17–6e17 per cm^2) but at a coarse angular resolution (9'–49'), while the WSRT HALOGAS study had a relatively low sensitivity, ~ 1.0e19 per cm^2, but at a good angular resolution of 90''. The MHONGOOSE survey will achieve the HI column density sensitivity of the single dish studies, but with the angular resolution of the best interferometric studies, and thus achieve an overall sensitivity improvement of about a factor of 50 compared to existing surveys. It is likely to be the best such survey until the advent of SKA1-MID, and the increase in sensitivity will allow access to low column density gas, which will in turn shed light on the baryon cycle.

4. South African Capacity Development, and Outreach

One-quarter of the LSP team members are from South Africa, including three South African research chairs and other university faculty members. The PI himself is partly affiliated to a South African university. This makes it very likely that the MHONGOOSE project will attract a number of South African graduate students and post-doctoral fellows. A number of South African PhD and Master's students have already been involved in the science and technical preparations for MHONGOOSE, including the commissioning of the spectral line, mosaicking and polarization modes of the KAT-7 array, as well as studies of HI in nearby galaxies. Overall, the MHONGOOSE project is well positioned to train the next generation of South African radio astronomers.

The plans for outreach are relatively standard: producing images, using social media, and writing articles. That being said, the images produced from the combination of deep HI and multiwavelength data are likely to be spectacular, given the nearness of the individual galaxies and the depth of the survey. One of the MHONGOOSE team members is an expert at such image-making, and the images are likely to have a large impact on MeerKAT public outreach. The Teaching 2-element Interferometer is commendable and an interesting idea.

5. Recommendations and Miscellaneous Items

The Panel was unanimously supportive of this proposal, which makes excellent use of MeerKAT's high surface brightness sensitivity and angular resolution. MeerKAT is clearly ideally designed for studies of faint HI designed to reveal gas accretion and the cosmic web. As the proposal notes, observations of this quality will not be surpassed until well into SKA1-MID.

The requested sample size of 30 galaxies is sensible to ensure multiple galaxies in each mass bin. Furthermore, the sample size is consistent with other earlier surveys of nearby galaxies and well matched by the optical data available from the SINGG project.

Given the anticipated scientific returns of this projects and the relatively small time request compared to many LSPs, we do not recommend a reduction in time, if possible.

c. The MeerKAT Fornax Survey

1. Scientific Merit

Our understanding of cluster formation and evolution, as well as gas accretion from filaments should be considerably augmented as a result of the proposed study of HI in the Fornax cluster. Fornax has important differences from other nearby well-studied clusters (Virgo and Coma), particularly in the hot IGM, and there is evidence that gas-rich galaxies and galaxy groups are falling into this early-type dominated cluster.

The proposed observations are expected to improve HI mass sensitivity in Fornax over ATCA by roughly a factor of 20 at 10 kpc resolution (at the distance of Fornax), and go as deep as ATCA at 6 times higher resolution. By mapping a 12 deg^2 area (out to a radius of 2Rvir) in HI, this survey will probe the HI mass function down to low limits (~5x10^5 Msun) and allow the characterization of gaseous fueling or removal processes via the detection of low N_{HI} features (7x10^19 per cm^2 at 1 kpc resolution, 8x10^17 per cm^2 at 10 kpc resolution). Resolving the HI to small angular scales and masses will allow the study of ram stripping of HI from galaxies as a function of position within the cluster. This should advance our understanding of how HI is lost from galaxies as they move within the cluster environment. The planned HI column density sensitivity of 10^18 per cm^2 at 10 kpc resolution is just at the level where one might anticipate a detection of the HI from the cosmic web, allowing direct comparison with cosmological simulations. While detecting neutral hydrogen from the web is a little speculative, it is of great importance.

The ancillary data (OmegaCAM on VST, Herschel far-IR imaging, SAMI and MUSE IFU spectroscopy, and ALMA CO observations) should contribute significantly to the science benefits from the survey.

The proposal would have benefited from more analytic detail (e.g. how many galaxies are needed to achieve specific objectives; e.g. what will detected features say about models of galaxy evolution).

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

This is a straightforward and well-designed survey. The survey strategy involves mosaicking of the Fornax cluster and nearby sites of interest. The original survey outline has been modified to include recent ATCA detections and exclude regions where no HI has to date been detected. The mosaicking and sensitivity choices appear appropriate to the science. Time request and calibration overhead both appear appropriate. Reducing area depth sensitivity would compromise the ability to see the lower-column depth HI gas or the ability to cover the substructure of the cluster.

The proposal correctly requests night-time scheduling to mitigate solar RFI on the short baselines. Care will need to be taken to ensure that all pointings get similar (u,v) coverage, so that the sensitivity to (especially) extended emission remains the same across the field.

Good bandpass stability will be important to achieve the required spectral dynamic range, especially in the pointings towards and around Fornax A. The team may have to revise their current plan for a single 10-minute pointing for bandpass calibration for each run. In any event, they should demonstrate that they can achieve what they simulated during commissioning.

2.2. Resources

The team has extensive experience with HI observations of galaxies from the field to dense clusters. It is a relatively small team, which lends itself to higher responsibility for individual members who are themselves experts in the field, with a good spread over the required areas (HI, continuum, simulations, radio interferometry, multi-wavelength data).

They have secured funding for a significant research group and archive storage. The data volumes are large (~ 5 PB) but the existing European Research Council funding should allow the visibilities to be stored indefinitely, which significantly reduces the risk for such projects. As with the other HI LSPs, they will make use of the IDIA infrastructure, and store the data on tapes.

2.3. Management

The management plan is appropriate for a relatively small group. Science teams are defined, along with a team policy document. An ERC grant to build a research group dedicated to this survey has been secured and though the proposal lacks a clear data release plan, the intention to release enhanced science-quality data products is stated. The Fornax survey team are involved (along with the MIGHTEE, MHONGOOSE and LADUMA teams) in the development of a pipeline for line surveys, which should ensure the reliability of the science products.

2.4. Technical Aspects

The proposers have done a good job of demonstrating the importance of night-time scheduling for their science. They point out that the presence of Fornax A in the field will be a challenge, and have modelled the impact through simulation. They should demonstrate that they can achieve what they simulated during commissioning. Calibrators and time allocation to calibration appear appropriate. The zoom mode of MeerKAT will be required for this project.

2.5. Scientific Aspects

The main science here is insensitive to model assumptions. For plausible models of the HI mass function, the survey will detect a sizable number of objects (estimated at 60–150 in the proposal). The one part of the science case at some risk of failure is the detectability of HI 21cm emission from the cosmic web, which is at the limit of the proposed sensitivity, but is also well worth attempting due to the importance of the science.

2.6. Other relevant aspects

The supporting multi-wavelength observations are well described and present no risk. Large amounts of multi-wavelength data are already in hand and plans for obtaining additional ancillary data are well developed.

3. Suitability and Uniqueness

MeerKAT is the ideal instrument for the Fornax survey, owing to its large field of view, high raw sensitivity, outstanding surface brightness sensitivity (due to the relatively compact configuration) at a reasonable angular resolution, and excellent bandpass stability (due to the off-axis feeds).

4. South African Capacity Development, and Outreach

About 30% of the current Fornax team (senior level) is based in South Africa, with quite a few at universities, and the fraction has grown significantly since the original proposal. This is the basis for doing more to involve South African students and postdoctoral researchers. The PI's ERC grant can further assist in this.

The LSP team plans to combine their deep HI and optical data to produce images for the purpose of public outreach. These are very likely to be spectacular, given the nearness of Fornax and the depth and width of the survey. They are also considering animations to provide a visual perspective on cluster life, which would be quite interesting. The outreach program is currently at a very nascent stage, and should be further enacted and developed.

5. Recommendations and Miscellaneous Items

The Panel recommends the full-time request, if possible, in order to achieve the required sensitivity to meet the science goals.

Good bandpass stability will be important to achieve the required spectral dynamic range, and the team should demonstrate that they can achieve what they simulated during commissioning.

d. LADUMA: Looking at the Distant Universe with the MeerKAT Array

1. Scientific Merit

The role of HI in galaxy evolution as far back as $z \approx 1.4$ is a critical scientific problem that is directly addressed by LADUMA. The MeerKAT array configuration and UHF receivers may allow LADUMA to detect statistically useful samples of galaxies with $M_{HI} > 10^{10}$ Msun in redshift bins of width $\Delta z = 0.1$, and thereby constrain the role of HI in star formation since the "cosmic noon" when most stars in the universe were formed.

LADUMA should yield a significant advance owing to its unique combination of redshift coverage, sensitivity, and field-of-view. MeerKAT and LADUMA were made for each other.

LADUMA aims to probe the properties and evolution of HI galaxies via an extremely deep single-pointing integration (333 hours in L band and 3091 hours in the UHF band extending down to $\nu \approx$ 0.58 GHz) of the ν = 1.42 GHz HI emission line in the CDFS (Chandra Deep Field South) field, covering the redshift range $0 < z < 1.4$ (minus some gaps caused by RFI). Key science goals are: measuring the

HI mass function and its dependence on redshift and environment; measuring the cosmological mass density Ω_{HI} as a function of z; and studying the Tully-Fisher relation. In addition, other galaxy properties related to HI, such as the specific star-formation rate sSFR, will be investigated; and LADUMA will search for OH megamasers, 21cm absorption systems, and strongly lensed HI galaxies.

The proposal estimates that hundreds of individual galaxies with HI masses $\leq M_{HI}^*$ will be detected in each $\Delta z = 0.1$ bin over the range $0.05 < z < 0.65$, plus tens of higher-mass galaxies per bin to $z \approx 1.2$. While damped Ly-α absorbers in SDSS have yielded Ω_{HI} at $z > 2.2$, the highest-redshift detection of HI emission from the competing survey CHILES (on the JVLA) is only $z \approx 0.38$. Stacking is proposed for LADUMA to make statistical detections of lower-mass galaxies at lower redshifts and higher-mass galaxies at higher redshifts.

This is vital for our understanding of HI in galaxies, and it is core SKA science. Successful high-z results would be both high-impact and unique, and should dominate the field until the SKA is in operation.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

The observational strategy is sound, but subject to two technical risks that may prevent LADUMA reaching the theoretical noise limits calculated for its long integration times (333 hours at L band, 3091 hours at UHF) – especially because the data are to be combined in the image plane, where systematics may already be "frozen in".

- RFI from GNSS, GSM and the Sun is expected to degrade sensitivity in one or more frequency bands (redshift ranges). RFI may be reduced but not eliminated by observing at night.

- The strong ($S_{1.4\,\mathrm{GHz}} = 1.4$ Jy) radio continuum source PKS 0326-288 is 1 deg from the LADUMA pointing center in the CDFS. If the MeerKAT/ LADUMA spectral dynamic range is $< 3 \times 10^4$, it will prevent the UHF observations from reaching the thermal noise goal $\sigma \approx 26\ \mu\mathrm{Jy\ beam}^{-1}$ per $\Delta\nu = 16.6$ kHz spectral channel. The proposed $\tau = 3091$ hours of integration in the UHF band is unprecedented, and there is a significant technical risk that systematics will set a noise floor that prevents the sensitivity from improving as $\tau^{1/2}$ long before $\tau = 3091$ hours is reached.

2.2. Resources

The team, along with its APERTIF and MIGHTEE collaborators, has the required expertise and access to resources (mainly in data science). They have been active and successful in securing funding in support of the project.

2.3. Management

Management structure, membership and activities are all well planned and executed. This includes a strong data management plan, international collaboration and links to MIGHTEE (developing a unified pipeline).

2.4. Technical Aspects

By far the biggest risk for LADUMA is the unknown dynamic-range limit in the scientifically critical UHF band, especially if the data from individual observing days are combined in the image plane.

See 2.1 for risks associated with RFI (especially from GSM signals in the 925–960 MHz band and in the overlap between the L and UHF bands, where the highest number of expected detections are) and with the bright source PKS 0326-288.

2.5. Scientific Aspects

The assumptions about the source populations are realistic. Even if they prove to be inaccurate, the proposed LADUMA observations probe a sufficiently large solid angle and redshift range to detect departures from the assumed source populations and derive good science from those departures.

2.6. Other relevant aspects

The CDFS region covered by LADUMA has the best multi-wavelength coverage in the southern hemisphere. The plan for acquiring existing and new multi-wavelength observations of the CDFS is sound. Future surveys (e.g., MOONS, LSST) will further enhance the coverage.

3. Suitability and Uniqueness

MeerKAT is the only existing instrument suitable for the high redshift ($z > 0.4$) component of LADUMA. The closest competitor is the deep JVLA CHILES HI survey, but the JVLA field-of-view is only about 1/3 as big, and the JVLA frequency coverage limits the maximum CHILES redshift to only $z \approx 0.4$. Westerbork's APERTIF and ASKAP's WALLABY HI surveys will cover much larger solid angles but with lower sensitivity, complementing but not replacing LADUMA by detecting rare galaxies with exceptionally high HI masses. Almost 90% of LADUMA's observing time will be devoted to UHF observations of HI at the scientifically important higher redshifts, where the main competition will be the upgraded uGMRT, whose field-of-view is about 10 times smaller than MeerKAT's and whose spectral dynamic range is likely to be worse.

4. South African Capacity Development, and Outreach

The South African engagement (about 1/3 of the team members are from South Africa) and outreach plans are generally rated as very good to excellent. A number of students have been trained, including through the innovative JEDI workshops. Outreach for schools, including videos, is innovative.

5. Recommendations and Miscellaneous Items

LADUMA was ranked as one of the top LSPs by the Panel, with a caveat about UHF spectral dynamic range. The Panel recommends the full-time allocation, if possible, provided that the systematic errors can be controlled (see Sections 2.1, 2.4 and below).

The time request of τ = 333 hr for the L band deep integration is reasonable.

For the UHF band, which is essential for the highest priority science, the time request may be reasonable – but only if systematic errors do not limit sensitivity before the proposed τ = 3091 hours have been used up.

As a requirement for the full 3091-hour time allocation in the UHF band, the HI image noise levels should be monitored, and if they stop decreasing as $\tau^{-1/2}$, the survey observations should be suspended until the systematic errors can be reduced. Demonstration that image noise continues to decrease as $\tau^{-1/2}$ should be an essential part of the annual reviews of LADUMA's progress.

The proposers are invited to consider alternatives for mitigating the dynamic range problem:

- Choose another field with no strong source nearby.
- Improve dynamic range by editing and calibration in the (*u,v*) plane, not in the image plane, where many errors are "frozen in" and can no longer be removed.

e. MIGHTEE: The MeerKAT International GHz Tuned Extragalactic Exploration Survey

1. Scientific Merit

MIGHTEE lists nine science topics to study with its simultaneous continuum and HI surveys covering ~17 deg^2 at L band and ~6 deg^2 at S band in four southern fields that already have extensive multi-wavelength data (XMM-LSS, E-CDFS, and ELAIS-S1, and COSMOS):

1. Evolution of the cosmological star-formation rate density SFRD (via continuum, HI, polarization);
2. Galaxy mergers in dusty AGNs (via OH megamasers);
3. Cosmic evolution of HI to $z \approx 0.5$ over 20 deg^2, to complement LADUMA's smaller but deeper HI survey;
4. Quenching of star formation via HI stripping;
5. Polarization of radio AGNs as a function of environment, physical size, etc.;

6. AGN feedback studied via evolution of the AGN continuum luminosity function to high redshifts plus HI emission (to $z\approx0.2$) and absorption (to $z\approx0.58$);
7. Large-scale structure (clustering, bias, weak lensing via polarization PA), construction of a RM grid to probe the intergalactic medium (IGM), dark matter (via HI rotation curves of galaxies with $M_{HI} > 10^{10}$ Msun and $0.07< z < 0.12$, plus a few nearby lower-mass galaxies);
8. Clusters of galaxies (via relic and halo radio sources and cluster magnetic fields);
9. Evolution of magnetic fields in high-redshift galaxies (via combined L- and S-band polarization).

The L-band survey design is driven primarily by the deep continuum, polarization, and HI surveys intended to constrain the cosmological evolution of star formation and AGNs. The S-band part of MIGHTEE is new. The Readers were instructed to group all nine science topics and grade the total scientific merits of the L-band and S-band observations separately. They slightly favored L band over S band. The Panel concurs, with a caveat about the dynamic range risk discussed in Section 2.4 (Technical Aspects) below. We assigned the highest scientific weight to constraining the cosmic evolution of the SFRD, a topic of fundamental importance to which MIGHTEE could make a significant contribution. Indeed, this topic is the highest-ranked by the SKA for its continuum observations. Some of the other topics (e.g., large-scale structure from weak lensing) are also important scientifically but only marginally feasible with the limited sensitivity, resolution, and sky coverage of MIGHTEE; they are better matched to the full SKA.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

After the 2009 MIGHTEE medium-deep Tier 2 continuum survey proposal (primarily intended to determine the evolution of AGN and star-formation activity as a function of time and environment) was written, the planned extent of the MeerKAT array was reduced. The longest baseline shrank from 60 km to 8 km. This change hurts deep L-band continuum surveys because it broadens the synthesized beam and increases the total-intensity confusion limit. Some of the lost resolution can be recovered by heavily down-weighting the shorter baselines in the core, albeit at the cost of sensitivity.

The 2016 MIGHTEE proposal has adapted by relaxing the rms noise plus confusion requirement to $\sigma\geq2$ μJy/beam, reducing the sky area covered from 35 deg^2 to 17 deg^2, and splitting the observing time request between L band and S band, where the size in wavelengths of the new MeerKAT is closer to that of the old MeerKAT at L band. The smaller (6 deg^2) S-band survey should not be confusion limited, need not suffer from significantly “unnatural” weighting, and improves the RM coverage of polarization surveys. On the other hand, the typical source is only 0.6 times as strong at S band and the statistical uncertainties are doubled by the reduced sky coverage.

The thermal noise calculation seems to be accurate, but the calculated confusion limit remains uncertain to within a factor of two. Proposal Figure 1 shows the rms “effective noise” as a function of the Robust parameter *r.* For $r\approx-0.4$, the FWHM beamwidth is $\theta\approx7$ arcsec. The flux density of the weakest source reliably detected in the presence of confusion was estimated in terms of the number m of beam areas $\Omega = \pi \theta^2 / (4 \ln 2)$ per source. The mean number μ of confusing sources within the FWHM angle θ (the Rayleigh resolution criterion) of any source is $\mu = 4 \ln 2 / m$ and the Poisson probability of finding no unresolved confusing source stronger than the weakest detectable source is $P(0) = \exp(-\mu)$. For m = 5, 10, and 20, this is P(0) =0.57, 0.76, and 0.87, respectively. Thus, both m = 5 and m = 10 (proposal Figure 1) yield unrealistically low confusion limits. Using θ=7 arcsec and m = 20, the cumulative 1.3 GHz source density is $N(>S) \sim (20\,\Omega)^{-1} \sim 4 \times 10^7$ sr^{-1} $\approx 10^4$ deg^{-2} at $S \approx 12$ μJy/beam, which is probably the minimum L-band continuum detection limit for the current MeerKAT configuration. It is also about 5x the highest rms “effective noise” values shown in the purple bands of Figure 1.

To reach $\theta \approx 7$ arcsec at L band requires greatly down-weighting data from baselines within the $D \approx 1$ km central core by setting the Robust parameter to $r \approx -0.4$. This multiplies the required integration time by factors $t_f \approx 3.5$ to reach the noise level of a naturally weighted image (lower panels in Figure 1). Unfortunately, this large t_f factor cancels the FoV advantage factor $\Omega_f \approx (25/13.5)^2 \sim 3.4$ in survey speed that MeerKAT’s 13.5 m dishes bring over arrays of 25 m dishes such as the VLA.

The MIGHTEE proposal did not state that night-time observing is required. The L band continuum observations if done in conjunction with HI will probably have to be made at night. The S-band observations can almost certainly be made day or night if scheduled to avoid pointing within a few degrees of the Sun.

Section 4.11 of the proposal suggests enhancing MIGHTEE science by making matched-sensitivity VLBI survey observations across a significant fraction of the selected fields covering $\Omega \approx 17$ deg^2. The high VLBI resolution can discriminate between compact AGN sources and more extended star-forming galaxies. No numbers were given for the required observing time. However, even with its most sensitive receiver (C band), the VLBA needs ≈500 hours to reach $\sigma = 1.5$ μJy/beam in a *single pointing*, so the total observing time required for the proposed VLBI survey appears to be in the tens of thousands of hours.

2.2. Resources

The observational strategy is straightforward, and the MIGHTEE team contains leading experts with the required expertise and resources.

2.3. Management

The management plan seems to be realistic and appropriate.

The requested 18-month data proprietary period “after a field is finished” is unclear because “finished” is unspecified. It is likely that calibration and data-processing improvements will result in multiple data releases spread over several years. SKA SA management should monitor this (and all other LSPs) to ensure reasonably prompt (12 or 18 months) data release after each observing season, and not allow each team to unilaterally define "finished."

2.4. Technical Aspects

We believe that the largest technical risk will be achieving the required total-intensity dynamic range, a risk which was neither mentioned in the MIGHTEE proposal nor called out by the Readers. Dynamic range is usually defined as the peak flux density in an image divided by the rms noise. For example, a 200 mJy/beam source dominating a σ = 2 uJy/beam image implies a dynamic range of 10^5:1. This dynamic range is not difficult to reach in a field dominated by a strong unresolved source at the pointing center, but it is difficult in deep survey fields containing numerous sources on the sloping sides of the primary beam because the relevant flux density becomes the quadratic sum of all source flux densities in the beam, and pointing errors plus parallactic rotation of noncircular primary beams acting on sources away from the pointing center will also limit dynamic range (see SKA memo 114). The required dynamic range grows rapidly with wavelength. Thus, μJy JVLA L-band surveys with $\theta \approx 7$ arcsec are limited by dynamic range over most of the sky, and even μJy JVLA S-band surveys can reach the theoretical noise limit only in carefully selected primary beam areas. The MeerKAT L-band FoV is nearly 4X that of the JVLA L-band FoV and ~16X that of the JVLA S-band FoV, so the MeerKAT required dynamic ranges are proportionately higher.

Sidelobes of the strong ($S_{1.4\,\mathrm{GHz}}$ = 1.45 Jy) double radio source PKS B0326-288 = ATLAS S145 will almost certainly limit MIGHTEE sensitivity in the CDFS field shared with LADUMA. As Norris et al. 2006 (AJ, 132, 2409) learned, "*The CDF-S/ATLAS field contains an unusually strong source (S145 = ATCDFS J032836.53-284156.0) in pointing center 1 which presents a challenge to our calibration procedures, as it is present in the sidelobes of several other pointings. Calibration errors from this source significantly increase the rms noise of the images in this region of the ATLAS field. We have found that a significant contributor to these calibration errors is the noncircularity of the primary beam response of the antennas. While the primary beam response can be measured accurately, current radio-astronomy imaging packages do not enable the data to be corrected for this. Work is in progress both to characterize the primary beam response (using holographic antenna measurements) and to write new calibration software that can apply this information.*"

Other MIGHTEE fields contain moderately strong sources and may not reach the noise/confusion limit. Non-uniform sensitivity will not seriously affect some MIGHTEE goals, such as detecting distant star-forming galaxies and AGN, but it will probably harm others, such as measuring the clustering of radio sources and the evolution of bias.

A consequence of reaching the dynamic range limit is that the image noise will no longer integrate down as $\sigma \propto \tau^{-1/2}$. The Panel recommends that MIGHTEE tentatively be granted its requested L-band observing time, subject to the requirement that it can demonstrate to SKA SA management oversight that σ continues to integrate down. In return, SKA SA management should allow the MIGHTEE LSP team considerable leeway to revise their observing and imaging strategies as the data start coming in. The MIGHTEE LSP team has the required astronomical expertise and observing experience to optimize their results. MIGHTEE has so many science goals that the risk of some not being achieved is high, but the risk that all will fail is low.

2.5. Scientific Aspects

The MIGHTEE survey areas were appropriately chosen for already having the most extensive multiwavelength data.

The scientific risks are difficult to judge because the MIGHTEE proposal doesn't provide sufficient quantitative information. Many of the science requirements on the MIGHTEE survey sensitivity and sky coverage are soft, so their degradation by factors of two are not fatal. However, the Panel agrees with the Reader who pointed out that the MIGHTEE proposal "is long on qualitative statements about solving a host of problems in astronomy and cosmology, and seriously short on quantitative estimates of how much progress we can expect in each of the many areas it covers." The proposal needs more equations and/or numbers for the required sensitivity, angular resolution, sky coverage, source numbers, etc. For example, it doesn't say how many star-forming galaxies in each redshift range are needed to constrain the evolution of the SFRD with small statistical errors, how many MIGHTEE will actually detect, and what fraction of the total SFRD is in galaxies that will be missed because they are fainter than the MIGHTEE detection limit at any particular redshift. The quantitative simulation of the two-point correlation function in the middle panel of Figure 3 looks much better than the JVLA result based on a sample complete down to $S_{1.4}$ = 20 uJy, but that comparison uses the old MIGHTEE sample depth of $S_{1.4}$ = 5 uJy, which is no longer realistic.

One Reader was concerned about how many polarized sources would be detected. According to Section 4.7.3 of the proposal, the use of galaxy polarization position angles to detect the magnetic cosmic web requires "several 100 to 1000 polarised sources per square degree, and with RM precision of ~1 rad m^{-2}." Figure 6 in Stil et al. (2009, ApJ, 693, 1392) shows median integrated polarizations ~1% at 1.4 GHz. Thus less than half of the galaxies will have $\Pi_0 > 1\%$ and a 5 uJy polarized flux implies total flux $\geq$500 uJy. There are about 500,000 galaxies sr^{-1} so strong, so there might be 150 deg^{-2} polarization detections. There will be less depolarization at S band, but the sources will also be weaker and the sky coverage smaller. Thus, the Panel believes this concern is justified.

2.6. Other relevant aspects

Excellent multi-wavelength data are already in hand for all four proposed MIGHTEE fields.

3. Suitability and Uniqueness

MIGHTEE is tuned to MeerKAT's "sweet spot" in the "medium deep" survey domain, bounded by the much larger but less sensitive EMU and APERTIF surveys on one side and smaller but ultimately more sensitive JVLA surveys on the other. The reconfigurable, multi-wavelength JVLA could in principle duplicate most of the proposed MIGHTEE observations and has already done smaller subsets of some (e.g., the COSMOS continuum survey, the CHILES HI survey), but proposal pressure on JVLA time is high enough that only MeerKAT is likely to make the proposed MIGHTEE observations.

The current configuration of MeerKAT is optimized for HI, pulsars, and transients; it is too compact and centrally concentrated for making very deep L-band total-intensity surveys in the presence of confusion. The configuration is “bigger” in wavelengths at S band. The MIGHTEE continuum survey will provide some improvement over existing facilities such as ASKAP or the JVLA for programs similar to MIGHTEE, but not a large one. The extragalactic sky is so isotropic that deep MIGHTEE surveys do not benefit significantly from MeerKAT being in the southern hemisphere.

4. South African Capacity Development, and Outreach

The current level of South African engagement of this LSP team is strong, especially at South African universities. Several Readers criticized the lack of detail in the outreach and popularization plans. The Panel assigned a low weight to that criticism, which was also made for a number of the competing proposals. Rather, we recommend that SKA SA itself coordinate and support the outreach efforts of all LSP teams.

5. Recommendations and Miscellaneous Items

The Panel recommends allocating most of the requested time allocation, if possible, for the team to achieve its science goals without significant compromise. Furthermore, the MIGHTEE LSP team should be allowed to update their observing strategy in light of actual experience with MeerKAT (see Section 2.4 above). In particular, if the L band image becomes dynamic range limited, some of the remaining L band time might be rescheduled at S band if the MIGHTEE team submits a proposal demonstrating its scientific value to SKA SA management.

On the question of whether the calibration overhead should be 10% or 20% (noted under “Findings of Technical Review of proposal done by SKA SA”), the time needed for routine, periodic calibration (e.g., observe a phase calibrator every 30 minutes, a polarization calibrator near transit every day, and a bandpass calibrator every day) should not be more than 10%. However, a lot of time-consuming fundamental instrumental calibration will be needed by MIGHTEE, particularly mapping out the full primary beam by observing a calibrator over a regular grid of pointing offsets. Responsibility for such observations should be shared among the individual LSPs (each with its own unique requirements) and MeerKAT’s overall commissioning team.

We note that a “free” commensal byproduct of the MALS LSP could be L-band continuum images of up to 1000 pointings with 56 minutes integration each centered on compact flat-spectrum radio quasars stronger than 200 mJy. The images may be confusion or noise limited at higher levels than MIGHTEE, but not hugely higher. Compact radio sources at the pointing centers can be excellent in-beam phase calibrators to yield high dynamic range. MALS Table 2 indicates only ~2 uJy/beam rms noise with reasonably natural weighting. Would such continuum images or the *(u,v)* data for them be useful to the MIGHTEE team? They aren’t in selected deep fields, but they would usefully constrain populations of relatively low-redshift AGNs and star-forming galaxies detected in widefield multi-wavelength surveys (e.g., SDSS, 2MASS, WISE, IRAS,...). They might better constrain the SFRD at relatively low redshifts, for example. No continuum collaboration was listed in MALS Section 12 or the MIGHTEE proposal.

f. MeerTime: The MeerKAT Key Science Project on Pulsar Timing

1. Scientific Merit

MeerTime is a collection of pulsar timing components that will have high scientific value and legacy impact for MeerKAT. The telescope is excellent for pulsar timing, particularly for southern pulsars, where the only previous competition is Parkes, which is about a factor of 6 less sensitive. This LSP team has led the development of a pulsar timing backend and S-band receivers for MeerKAT, which will benefit the entire user community. While some of the science may take years to yield major breakthroughs, the binary pulsar project should provide some high-profile results, such as neutron star mass measurements, in fairly short order.

Generally, the young pulsar and magnetar components are less compelling as legacy timing projects for MeerKAT, and it would be acceptable to relegate them to the open time competition.

1.1. Millisecond Pulsars

The sensitivity of MeerKAT will provide dramatic breakthroughs in the ability to detect a background of low-frequency gravitational waves via MSP timing. MeerKAT's emergence into the IPTA should allow it to make a truly transformational contribution in this area, and could be the difference between detecting gravitational waves and not. Particularly, new MSPs discovered in Fermi sources tend to be <1 mJy and could be great targets, and by beginning to time them now and continuing in the SKA era, they will make valuable contributions to future pulsar timing arrays. Expanding the number of pulsars with residuals <1 μs is critical to improving the sensitivity of the IPTA and MeerKAT is poised to do this for a number of southern MSPs.

Given the importance of MSP timing to the SKA science case, this project will be very valuable if it can demonstrate very high precision (<100 ns) timing using an interferometer.

1.2. Binary Pulsars

Binary pulsar timing accesses a range of important science and sensitivity increases lead directly to new measurables such as Shapiro delay and post-Keplerian parameters. The exquisite precision with which binary parameters will be derived is also a fundamental tool to understand the astrophysics and evolution of massive binaries. The large number of recent MSP discoveries, combined with those sure to come out of TRAPUM, provide many good targets. However, is would be good to see which systems the authors actually want to target, and what overlap there will be with sensitive northern hemisphere telescopes.

1.3. Globular Cluster Pulsars

The unique environments in which pulsars in globular clusters exist provide opportunities for studying their evolution that are not found in the Galactic MSP systems. Again, the sensitivity of MeeKAT promises to be transformational in our understanding. This will allow some southern cluster pulsars to be studied in exquisite detail that has not been possible in the past. However, the authors don't spell out clearly what goals they can address with the proposed time. Many of the pulsars discovered in clusters are quite faint (log N/log S ~ 1/S), and even with MeerKAT's sensitivity, TOA precision won't be amazing. The authors don't say specifically, but for a 10 μJy pulsar the expected S/N is about 15, which at a monthly cadence is fairly limiting in terms of what can be measured. Extracting proper motions will then rely on the favorable time scaling ($t^{-5/2}$) but the program only last five years. What will actually be detectable?

There are also concerns about the availability of sufficient tied-array beams with coherent de-dispersion capability. It would have been good to see more specificity of the clusters/pulsars to be targeted and what measurements will be expected to be achieved. There are some technical concerns about the number of tied array beams that can be formed and coherently de-dispersed (some clusters require ~30, but only 4 are planned to be implemented), which may force some of the timing to be done with just the "core beam" or by observing some pulsars in the same cluster sequentially, reducing the efficiency of the allocated time. This will be even more of a problem for clusters requiring S band. We rank this lower than the previous two components.

1.4. Thousand Pulsar Array

This is a really interesting aspect of the proposal. The long timing baselines of hundreds of pulsars by Jodrell Bank have continued to turn up surprises that challenge our understanding of pulsars, e.g., the quasi-periodic oscillation (precession?) of B1828-11, the later and related discovery of quasi-periodic state switching, the measurement of decade-scale precession of the Crab pulsar, etc. Increasing this effort to an even larger population is bound to both lead to new discoveries and to help answer longstanding questions – as mentioned in the proposal: do older pulsars glitch? How widespread is state switching, and what drives it? What is the nature of timing noise and how does it evolve? What does the long-term behavior of the ISM (refraction and DM variation) look like along 1000 lines of sight? The latter is actually quite interesting given the role of the ISM in processing and enhancing FRBs.

A risk here is that much of the power of such a project comes from very long timing baselines, over which proper motion and other parameters become measurable, and over which secular changes can be detected. Five years will be a good start, and hopefully the project will drive an N-thousand pulsar array for SKA. This is the most obvious 'legacy' component of MeerTime. The immediate public release of the data, if done properly, should bring many users to look into these many science questions. The non-public nature of the Jodrell Bank timing program has been an impediment to its use, so this is a welcome goal.

1.5. Young and Energetic Pulsars

While young pulsars are indeed interesting to time, the proposed science plan is nebulous, and this is probably the weakest part of the proposal. To measure a braking index requires an extremely young and rapidly spinning down pulsar, which are by definition rare, or a long timing baseline (more than five years). Measuring proper motion for high timing noise young pulsars requires a long timing span to disentangle the signature from timing noise. Likewise, characterizing pulsar environments is interesting, but a slow process. The only strong argument for the cadence is to catch glitches, but even then, bi-weekly is not often enough to catch short glitch transients. How many pulsars are intended to be timed? And how will the program differ from the long-term monitoring program at Parkes?

1.6. Magnetars

Magnetar dynamics are still poorly understood, and the proposal to obtain high-cadence timing data is interesting. Their pulse profiles also vary radically, and the large fractional bandwidth of MeerKAT could make for an interesting study, particularly of polarization. The current request for 100 hr with 50 session per year means about 25 minutes per session, which should allow plenty of time to observe a few magnetars. It would have been good to see more concrete plans of what will be done in terms of turning the complex data set into results: will it be a fishing expedition, or will explicit models be tackled?

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

The observation strategies proposed are generally sound, based on years of experience with similar programs, but the source selections could have been better explained in a number of sections. The requested cadences are reasonable assuming MeerKAT is ‘going it alone’. However, there is scope for collaboration with other groups doing pulsar timing, especially those at Parkes and Molonglo, with MeerKAT concentrating on the weaker sources. This could reduce the necessary observing time by 20% or more.

Since the S-band receiver is not likely to be available for the first year, the proposers should elaborate on how that impacts the MSP timing component, since PTA observations generally require near-simultaneous multi-band observations to measure and remove dispersion measure variations. Also, for the MSP timing, there may be tension between the goal of obtaining much higher precision than the current Parkes observing campaigns and the requested 24-hour block. Although some pulsars are available at any given LST, the majority of pulsars and the best timers are at Galactic LSTs (J1909-3744, J1713+0747, J1744-1134, etc.). Staggered 12-hour blocks may be more effective, or even longer blocks if the proposers are able to optimally interleave their targets with similar cadence (globular clusters, binary pulsars, Thousand Pulsar Array), but these targets also generally favor Galactic time.

Claimed sensitivities are based on the full tied-array beam. This is not applicable to e.g. the ‘thousand-pulsar’ component where sub-arrays are required to achieve the stated goals. If wide frequency coverage is needed for precise DM determination, this also applies to the other components. Also, it is yet to be demonstrated that arrays can routinely achieve the theoretical timing precision and accuracy over long time spans. This is challenging and is a significant risk factor.

2.2. Resources

The list of authors reads like a who’s who in pulsar timing. There is no doubt that they have the expertise to pull this ambitious project off. It is also noteworthy that the team includes people who can lead the interpretation of the results very effectively.

Still, 1000+ pulsars is a lot of data sets to maintain. It will be a challenge to keep up with the data and maintain the proposed release schedules. The greatest risk here is probably in the creation of a robust data archive, which is one of the least attractive things to work on and one of the most important to producing a legacy dataset. A huge amount of work needs to be done to support very high precision pulsar timing in terms of metadata, recording correct values from the instrument, etc., well beyond simply getting the signal processing right.

2.3. Management

The project is well established and has a sound management structure that is being implemented by people with proven track records in this area. We particularly applaud their data access policy in which the 1000 pulsar array data is available immediately. This will greatly increase the scientific value of the experiment. For the other components, the 18-month period is similar to what is done elsewhere. We note that in practice this can be hard to actually implement and encourage the team to really make this 18-month release something that happens.

2.4. Technical Aspects

This project uses standard techniques and tools, many of which have been developed by MeerTime team members. RFI is always an issue, but is one area where an array has a distinct advantage over single dishes. A big risk here is that very high-precision (<100 ns) pulsar timing over long timescales with interferometers has yet to be demonstrated. There is no fundamental reason why it should not be, but it certainly requires careful attention to gain stability and beamforming that are absent from single dish timing. Enough effort should be concentrated on this high priority task early on to catch any big problems early.

The Thousand Pulsar Array is a really cool idea, but curating such a large (in terms of things that can go wrong) data set will take a tremendous amount of time. Will the team do this work, or leave it up to the community?

A few pulsars (those that haven't been followed up / don't have published results; those with very strong timing noise, etc.) may have positional uncertainties comparable to the angular resolution of the longer baselines, particularly at S band. It might be worth doing an initial optimization to determine these positions well to obtain optimal S/N.

2.5. Scientific Aspects

There is little risk here. Some science is guaranteed (e.g. precision tests of GR with a sensitive telescope, adding high quality data to existing pulsar timing arrays). Because the amplitude of a low-frequency stochastic gravitational wave background is unknown, there is a 'risk' that the improved timing precision allowed by MeerKAT won't lead to a detection. However, upper limits are also useful in constraining black hole merger histories and dynamics.

2.6. Other relevant aspects

Nothing further to add here.

3. Suitability and Uniqueness

MeerKAT will revolutionize pulsar astronomy in the southern hemisphere due to its great increase in gain, bandwidth, flexibility, and RFI environment, compared to previous instruments. It is extremely suitable for the proposed program. The only caveats are that Parkes spends 1000 hr/yr on PPTA, which is 2.5x what MeerTime is proposing, and Parkes is adding a wideband feed (0.7-4.0 GHz), which will lessen MeerKAT's advantage.

For projects like the Thousand Pulsar Array, the sub arraying capability allows MeerKAT to observe several pulsars simultaneously.

4. South African Capacity Development, and Outreach

The team has already been actively engaging the international timing community in South Africa over the past 5 years, and developing teams in South Africa. The LSP has engaged a large portion of the small but growing South African pulsar community, e.g. by holding the IPTA 2016 meeting in Stellenbosch. We particularly like their plan to fund a project manager in South Africa to oversee milestones and outreach activities. This is often an overlooked area of large projects and can mean the difference between success or not in the dissemination of results to both the scientific and broader communities.

The proposers mention a fee for joining. There is some concern that this may be an impediment for groups in countries with fewer resources. We also suggest that this fee not be charged to South African Co-Is.

There will be much data analysis work to be done, which will allow for large-scale student involvement and training, presumably involving South Africans. It is surprising that this possibility is not discussed further in the proposal. However, the J1141-6545 ‘single pulses around the orbit’ outreach project is clever. The www.meertime.org domain has not been registered for the project yet.

There is good infrastructure development by the group in terms of the pulsar processor and S-band receivers, and a large amount of resources are now available through OzGrav via the PI.

5. Recommendations and Miscellaneous Items

The Panel recommends allocating the full time requested, if possible, to four components of MeerTime: binary pulsars, millisecond pulsars, the 1000-pulsar array, and globular clusters. Pulsar timing is best done with long term planning and can be difficult to do under normal proposal call cycles, so it is a good example of an LSP. Timing also takes a long time to pay off, so frequent re-proposals are difficult. The proposal is well written and compelling.

The high ranking of binary pulsar timing reflects the fundamental and important physics questions that can be addressed early in the MeerKAT science program. Likewise, millisecond pulsar (MSP) timing draws interest from a wide swath of the astronomy and physics community due to its gravitational wave connection. (Go to any astronomy meeting; this is by far the most talked-about pulsar-related subject.) The high ranking of the 1000-pulsar array is because it uses unique MeerKAT capabilities to dramatically increase data collection and hence potentially transform its field (large-scale, long-time-scale study of pulsar rotation and magnetosphere characteristics).

The time request is reasonable. MSP and binary timing requires a large amount of telescope time per source to attain competitive timing precision. The canonical pulsar timing project (1000 pulsar) is remarkably efficient given the large number of sources to be observed. However, a source list would have been useful. As mentioned above (2.1), collaboration with other southern telescopes could result in higher efficiency and reduced observing time.

For globular clusters, the authors can more carefully select which ones will be timed. In particular, many globular clusters are visible to, and being timed by, the GBT, with higher sensitivity than Parkes, reducing the uniqueness of MeerKAT for these clusters. If a reduction is needed, the proposers can concentrate on southern globular clusters and those with the most pulsars. With a modest reduction, the science goals here would not be harmed. The Parkes wideband upgrade may not be as useful for some clusters due to the small beam size at high frequencies.

One of the tasks of the project manager should be working to keep the team on track for processing, archiving and releasing data according to the release schedule. We also note that making the 1000-pulsar data available immediately does not absolve the team of more thoroughly reducing and analyzing those data in a timely manner.

This group is to be commended for their effort in developing pulsar timing data acquisition equipment for MeerKAT, which will be open to all users and will surely be popular among open time users.

The project is open to new members (good) but a fee will be imposed on all members. The motivation for the fee is funding a project manager. The motivation is good. However, this may reduce the ability of some to join, especially institutions from developing countries.

g. TRAPUM: Transients and Pulsars with MeerKAT

1. Scientific Merit

This proposal is for several pulsar surveys that will be major scientific producers for MeerKAT. The development of the 400-beam tied-array beam former and the processing system to do real time pulsar searching is particularly valuable and will be an important pathfinder for future instruments. There is still significant risk in planning for real time processing, even with a few day buffer. The major leaps in sensitivity are mainly in the declination < -39 deg region, where MeerKAT using the core dishes to form beams is about a factor of 4 more sensitive than Parkes, the only other major instrument that can effectively reach the far southern sky.

1.1. Supernova Remnants (SNRs)

Sensitive, systematic searches for pulsed radio emission from SNRs will make a good use of the MeerKAT sensitivity, and will provide useful clues to neutron star population synthesis models. However, the SNR case would have been strengthened by being more specific about how many SNRs are proposed to be searched, a justification for the integration time based on flux limits, and some estimates of how many discoveries are expected based on beaming fractions, distances, SNR ages, dispersion and scattering. Although the discoveries are likely to be interesting, the yield per hour of telescope time is likely to be low, without more careful source selection.

1.2. Pulsar Wind Nebulae (PWNe)

The PWN portion of the proposal is more compelling than the SNR portion as PWN sources are guaranteed to harbor a pulsar, and the association of a pulsar and a PWN is even more useful than a pulsar + SNR, as the PWN provides a 'calorimeter' for the pulsar wind, which in turn contains much of the spin-down energy of the pulsar. Estimates of the lepton content of PWNe created the 'multiplicity problem' in which the ultra-relativistic particles accelerated directly in pulsar magnetospheres must, by the time they reach the wind zone, be converted to 10^5 mildly relativistic pairs in order to match synchrotron observations. Modern resistive MHD simulations are beginning to reflect the true structure of pulsar magnetospheres and provide accurate predictions with which to confront new data. Obtaining more pulsar/PWN pairs would be a huge step forward here. The proposal could have been strengthened by including some estimate of the success rates expected based on previous PWN searches and the TRAPUM sensitivity.

1.3. TeV Sources

The searches of TeV sources provide an additional window into SNR and PWN energetics. In most cases, the TeV emission is attributed to shock acceleration in pulsar winds or SN ejecta. However, in some cases the TeV source is detached from the present pulsar/lower energy PWN position and represents an older population of electrons. In fact, PWNe generally show offset morphologies at different frequencies (e.g. GeV vs. TeV). This offset is typically not large relative to the tied-array beam FoV proposed here, but some care should be paid to available multi-wavelength coverage to ensure the tied-array beams cover the most likely present pulsar position! This is also a very timely survey component, which will provide important inputs for the physical modeling of the most powerful particle accelerators in the Galaxy. The synergy with the planned CTA (in combination with the PWNe experiment) makes this component highly relevant.

1.4. Fermi-LAT Sources

Because of the historical productivity of pulsar searches targeting Fermi-LAT sources, this is probably the most compelling of the proposed targeted searches. Thus far, GBT has been the most productive telescope in this work, and TRAPUM will bring that level of sensitivity to the southern sky. Many new discoveries are near certain and are important for completing the identification of Fermi-LAT sources. The plan to undertake three passes per target is essential. Working with the LAT team to get the most current source lists will be necessary.

1.5. Globular Clusters

Globular clusters are sources of exotic binary systems (and hence potentially unusual neutron star masses, pushing on the upper mass limit). MeerKAT provides significant improvement on sensitivity to southern clusters compared to Parkes. For those clusters, the claimed factor of 6 sensitivity improvement means a large number of MSPs can be discovered and they can be effectively timed with MeerKAT and other similarly sensitive instruments. Thus, interesting systems (double neutron star, double pulsar, triples, pulsar + BH) may be tractable and offer tests of GR, cluster evolution, etc. Targeting Fermi-LAT detected clusters is particularly promising. On the other hand, those clusters observed with GBT will offer fewer new pulsars, and these new pulsars will require essentially rise-to-set tracks to time until the advent of SKA1-MID. Even so, simply by discovering these pulsars and measuring their spin periods, we can learn a great deal about cluster dynamics. Therefore, this is an experiment worth doing, both to establish timing baselines for the faintest pulsars to be fully exploited by SKA and to learn more about clusters now. However, the discovery space for interesting pulsar timing systems is not large for most clusters, and Parkes spent inordinately large amounts of observing time on 47 Tuc, so new discoveries from that rich cluster will be limited. An important point is that because fortuitous scintillation can allow the discovery of pulsars well below the nominal flux limit of an observation, clusters that have already been observed many times will be less likely to yield new discoveries, even with a significant sensitivity improvement.

1.6. Nearby Galaxies

The substantial leap in sensitivity for the LMC and SMC will surely result in new discoveries pushing farther down the luminosity distribution in those galaxies, allowing comparisons of neutron star populations within different galaxies that have different compact object populations. However, the argument for probing metallicity/SFR/pulsar production is somewhat less compelling. It might be interesting, but was not mentioned, to probe the electron distribution more deeply via dispersion measure and compare this to multi-wavelength gas tracers. This type of tomography could better help us understand the ISM phases in our own Galaxy. Also, no mention is made of the possibility of detecting MSPs in these searches and what that would mean.

The giant pulse searches in more distant galaxies is more speculative, but worth trying. It is impressive that TRAPUM can substantially beat both HTRU and even the published Arecibo searches. Even a small number of discoveries will yield important information about the IGM and the capability of pulsars to produce (ultra) giant pulses.

1.7. Fly's Eye Transient Searches

This is an interesting component that is rather time critical. The surge of interest in FRBs means that there are multiple instruments/experiments coming online that are similarly capable of detecting luminous FRBs (e.g. CHIME, REALFAST, APERTIF, ASKAP, UTMOST, etc.). Once the population begins to expand, the return on investment for the proposed 720 hr becomes less compelling, especially since these hours will be largely useless for commensal observing.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

With this broad range of surveys, using established and proven experimental techniques, discoveries are essentially certain, though it is hard to predict in advance which of the different components will be most productive. The Technical Review identified a slight over-estimate of the calculated sensitivities and hence under-estimate of the observing time required. This will somewhat reduce the productivity of all components of the project except maybe the Fly's Eye observations. In addition, ~100 hours assigned to follow-up timing of the projected discoveries seems quite inadequate, even for determination of the basic parameters needed for a discovery publication. Many of the sources will be very weak, especially those in globular clusters, and difficult to time. Survey discoveries are much easier to time because they are far brighter, so a larger portion of the follow up time should be assigned to the pointed search discoveries.

The goal to estimate logN-logS with Fly's Eye detections of FRBs will be hampered by the ability to do primary beam corrections to get the true flux density. Perhaps the pointing strategy the authors allude to will help, but 'few arcmin' localization with a 1 deg beam sounds optimistic unless the overlap factor is so large as to substantially diminish the etendue.

2.2. Resources

The team includes world experts on pulsar searching and on study of the target sources such as Fermi gamma-ray sources and pulsar wind nebulae. They have outlined their plan for data reduction (e.g., time scales for data reduction, decimation for long-term storage) and they appear to be prepared to undertake the observations and data reduction. Based just on the proposal, it is hard to tell the technical readiness of the beamformers and backends required, but they seem to have the resources needed to produce them. Completing the development and demonstrating the performance and operational readiness of the system will be critical milestones.

2.3. Management

The team is well established, with pre-existing collaborations and an already functional management system. They appear to have a good data storage and archiving plan. However, they do not describe a plan for disseminating the data products except within the collaboration. Neither a time scale nor a method is given for releasing the data.

2.4. Technical Aspects

Development of the 400-beam tied-array system is key to much of this proposal. This is a challenging task and carries some risk. Data rates with the 400-beam searches are high and optimizing the data analysis will be challenging given the inability to store raw data for an extended period. RFI is always an issue for pulsar and FRB searches, but the very quiet site will be a major benefit.

The biggest difficulty will be pulsar candidate sifting/identification. This will be one of the first major pulsar surveys for which (mostly) raw data will not be available. The machine learning algorithms must be rock solid, and care should be taken to capture sufficient null result (non-candidate) data, e.g. by using a ‘periodic/blind’ trigger, to ensure a good sample of the null data in filterbank form is available to allow false positive analysis.

2.5. Scientific Aspects

There is a risk of some experiments not meeting expectations, particularly due to uncertainties in luminosity functions, and it would have been nice to see some discussion of confidence intervals in the predictions.

In several places, figures could have been effectively used to make key points to the Readers and Panel. Also, there were no references in the proposal.

2.6. Other relevant aspects

Most of the pulsar search components under consideration are based on follow-up of existing multi-wavelength observations. As is usually the case, fast transient searches benefit from coordinated multi-wavelength programs. This is mostly relevant for the Fly's Eye observations, and the proposal mentions the possibility of using night-time observations to maximize possible synergy with optical follow-up programs. The details of such synergy, which must not be trivial given the very large FoV of the Fly's Eye mode, are not described in the proposal. The possible liaisons with MeerTime and ThunderKAT are explicitly mentioned, and should be further pursued.

The consortium has achieved valuable outside grants to provide resources for the telescope and for the data analysis (computing power) needed for this specific program.

3. Suitability and Uniqueness

MeerKAT is well suited for the proposed science, and for most of it uniquely so. Pulsar search work is best done at relatively low frequencies (0.5–2.0 GHz), so a sensitive L-band system is perfect. Without a doubt, for the southern sources which are currently only accessible to Parkes, MeerKAT's sensitivity is unmatched and provides a compelling case for the searches considered here. The focus of the targeted searches should be in the part of the sky not accessible to other instruments in the northern hemisphere (e.g. GBT, Effelsberg and Arecibo), where the factor of ~5 improvement over Parkes will provide a very effective boost over previous surveys to guarantee a number of high profile results. In the southern hemisphere, there is no other experiment that would be in operation that could even come close to the sensitivity projections of TRAPUM. A critical feature is the availability of a sufficiently large number of tied-array beams, and the computing resources for off-line acceleration processing. The commitment of the TRAPUM team to make resources available is commendable.

4. South African Capacity Development, and Outreach

The team proposes an interesting plan for outreach and school activities, and the training and collaborative goals of the team are well matched to a sound development plan for the South African scientific community. It is also noteworthy that TRAPUM team members regularly visit South Africa, and that MPIfR are supporting an engineer to be based in South Africa and Germany.

We encourage the TRAPUM LSP to consider an open joining policy for SA Co-Is similar to that adopted by MeerTime.

5. Recommendations and Miscellaneous Items

The Panel recommends allocating the full time request, if possible, to four components of TRAPUM: young pulsars (SNRs, PWNe, TeV), Fermi-LAT sources, nearby galaxies and globular clusters. The Panel made no alterations to the requested time despite the fact that the Technical Review noted that the proposers overestimated the sensitivities and they did not account for loss of data due to RFI. For all components of this program, the yield of pulsar or transient discoveries is proportional to observing time granted. The program could be pared back (e.g., fewer pulsar wind nebula candidates or Fermi-LAT sources searched), in which case the rate of discovery would be proportionally reduced. If reductions are needed, concentrating on far southern sources is clearly the way to go.

For the purposes of ranking and scheduling components, the committee chose to merge the SNR, TeV and PWN topics into one, with the focus being on sites likely to host young pulsars. We discuss the science justification for each above, but for scheduling we propose that a number of hours be allocated to the merged components and the team can provide the most compelling source list they can compile from all three target classes.

The follow-up timing was not split out as a separate component of the proposal. However, it is required and should be given very high priority. There is no point in discovering pulsars if they are not followed up. It should be noted that many of the pulsars discovered in Fermi follow-up, cluster searches, or the Magellanic Clouds, etc., may be quite faint. More than ~100 hours will be required for follow up. Where possible, this follow-up timing could/should be coordinated with MeerTime to optimize observing, and/or offloaded to smaller telescopes where possible. The panel recommends that the SKA SA office consider two alternatives for meeting this important need: (i) leave the total time for the LSP fixed, and have the TRAPUM team carve out from existing allocation any extra time they need for follow-up; or (ii) have the TRAPUM team submit regular proposals for any extra follow-up time they need beyond the current estimated request.

For the Fly's Eye search, the required modes will take significant time and personnel effort to develop. The Panel did not feel that the scientific competitiveness relative to other world-wide efforts was sufficient on the necessary timescale to warrant strong justification of this development over other enhanced MeerKAT capabilities.

The proposal does not state whether new transient discoveries will be detected in true real time (the Fly's-eye mode events are said to be 'processed very similarly to the real time transient detection mode'). It does not state whether provision will be made to rapidly communicate detections of events to the general astronomy community, as is done for, e.g., Gamma Ray Bursts. Such communications should be encouraged.

The proposal does not mention proprietary periods or a data release policy. Clearly the raw data, which is too voluminous to be kept, cannot be released, but other products should be made available.

h. ThunderKAT: The Hunt for Dynamic and Explosive Radio Transients with MeerKAT

1. Scientific Merit

ThunderKAT is a transient science program designed to study various populations of primarily synchrotron-emitting radio sources. It has two primary observational components:

- A nearly real-time commensal transient survey based on image-plane searches for slow transients ($t \geq 1$ s) that continuously piggybacks on observations made for other LSPs. A well-designed commensal survey offers the potential of identifying new transient source populations at no cost in additional observing time. The ThunderKAT *image-based* search complements the TRAPUM *beam-formed* transient search, together they probe the full range of slow and fast transients.

- Monitoring of known transients (scheduled) and rapid target-of-opportunity (ToO) follow-up of transients discovered either commensally or triggered by external optical, X-ray, or gamma-ray telescopes. The Panel and the Readers were asked to review seven of the ten source populations from the ThunderKAT LSP. The SKA SA Science Committee determined that tidal disruption events (TDEs), the afterglows of fast radio bursts (FRBs), and the radio counterparts of gravitational waves were not to be part of this review. The science review below looks at each of the seven source populations separately.

ThunderKAT will be supplemented by MeerLICHT, a 65-cm optical telescope located nearby in Sutherland, South Africa. During clear nights, MeerLICHT will provide optical fluxes and colors in the MeerKAT field-of-view with a 1 minute cadence.

1.1. Black holes and neutron stars in X-Ray Binaries (XRBs)

Improving our understanding of accretion and jet production in Galactic X-ray binaries is important for understanding both stellar evolution and accretion/AGN physics. The field of black hole (BH) astrophysics in general seems poised to enter a period of transformational advances in our understanding of the environments and event horizon structures of BHs. The radio/X-ray fundamental plane is a key tool underpinning many of these advances.

Key lessons from the past two decades of XRB monitoring are: (i) the need for well-sampled broadband monitoring of XRB outbursts and (ii) the ability to track sources down to faint radio fluxes, to properly characterize “radio-faint” and “quiescent” systems (neutron stars in particular). These are both met by the ThunderKAT survey; the flexibility and continuum sensitivity of MeerKAT will advance the state of the art by providing densely sampled radio light curves.

The study of XRBs is hurt more than other components of this LSP by the relatively low frequencies that are available for MeerKAT. The Readers’ scores reflect this. This topic is discussed in more detail in Section 3.

1.2. Ultra-luminous X-Ray Sources (ULXs)

ULXs appear to comprise several distinct classes of objects including pulsars and neutron stars accreting at highly super-Eddington rates, stellar-mass BHs also exhibiting extreme accretion conditions, and a few genuine intermediate-mass black holes (IMBHs).

While the use of ULXs as probes of IMBHs is interesting, the Panel felt that this aspect of the proposal was not as well thought out as some other science drivers. It was not clear, for example, how the proposed observations would provide key results such as determining the mass of the putative black hole or how IMBHs could be distinguished from other classes of ULXs.

1.3. Cataclysmic Variables (CVs)

CVs are an important class of variable stars with wide interest for which radio studies have only recently been possible thanks to sensitivity improvements in radio interferometers. The results presented for SS Cyg demonstrate clearly the prospects of what might be possible with the discovery of a large sample of CVs, pushing the study of accretion physics into new source/flow classes.

The impending availability of good distances and the unique leveraging of the MeerLICHT project make the ThunderKAT ToO time invested a good value. Coordinated radio and optical observations will undoubtedly reveal new details on the dynamic coupling between accretion flows and jets.

1.4. Novae

While the current absence of southern radio telescopes for monitoring Galactic sources is unfortunate, the proposal did not describe the advantages of ThunderKAT except for sky coverage, so the science goals and anticipated sample seemed rather incremental in this area. Both the Readers and the Panel felt that novae science program outlined here appeared to be less transformative than the relativistic accretion project, for example. Higher frequencies and higher (VLBI) resolution are required to make progress in this area.

1.5. Gamma-Ray Bursts (GRBs)

MeerKAT can contribute by significantly increasing the number of observed events and giving unique southern-hemisphere coverage.

The detection of more afterglows for short hard bursts (SHBs) is of particular interest, given their likely link to neutron star coalescence and gravitational waves. Determining the “true” unbeamed GRB rate is important for understanding their energetics and connection to SNe.

The two-pronged ThunderKAT proposal, targeting bright long-duration GRBs to characterize afterglows and using commensal observing to find orphaned afterglows, is a powerful approach making good use of MeerKAT time. Counting orphaned GRB afterglows is a promising way to estimate the true GRB rate, but only if these afterglows can be reliably classified.

The poor match of L band to the higher-frequency GRB synchrotron emission is somewhat offset by the larger field-of-view in the blind commensal search. While there are fewer GRB events detectable at L band than at high radio frequencies, those that are detected will give superb constraints on the jet geometry, circumburst density, and total energetics (via calorimetry).

The final ranking of this LSP component was made assuming that only short, hard bursts would be subject to follow-up. The advantages of MeerKAT's L and S bands show most clearly for SHBs.

1.6. Core Collapse Supernovae (CC SNe)

Radio monitoring of CC SNe gives an important window into relativistic particle acceleration in SN ejecta and into the structures of both the ejecta and the circumstellar medium.

The proposal noted that ThunderKAT may yield an estimate of the radio luminosity function, but it did not say what could be learned from it. Nor was it clear how often one should expect events like SN2014C as shown in Figure 3 of the proposal. L band seems a poor match to CC SNe, which are known to have a dense circumburst medium, with opacity effects reducing the peak flux density and stretching the time-evolution to multiple years at L band.

1.7. Type Ia Supernovae (SNe Ia)

A better understanding of SNe Ia is vital given their role as standard candles in modern cosmology. Radio observations have the potential to distinguish among different progenitor models.

Although the single/double degenerate scenarios are not quite so unexplored as the proposal suggests, it is certainly true that both detections and strong upper limits in radio would be extremely interesting. Unlike for some other source classes, L band is a better match owing to the faster source evolution and low opacity of SNe Ia, and the excellent sensitivity of MeerKAT is critical for achieving constraining upper limits.

In order to put new, stronger constraints on the progenitors of SNe Ia, the proposed trigger distance of $D < 20$ Mpc may have to be tightened. The existing deep limits on SN2011fe mentioned in the proposal are for M101 at $D = 6.4$ Mpc, and there is a wealth of deep radio continuum observations from years of JVLA monitoring of nearby SNe Ia by Chomiuk et al. (2016, ApJ, 821, 119). While there is always value in increasing the sample of non-detections, the proposers should be prepared to show that any new ToO will lead to equal or stronger constraints than previous work. This may mean waiting for nearer (but rarer) events before triggering. The final ranking of this LSP component was made assuming a trigger distance limit $D < 10$ Mpc.

2. Feasibility

2.1. Observational Strategy/Source Selection Criteria/Time Request

For most of the ToO and monitoring source classes, the selection criteria, observational strategy, and requested time are reasonable. Some source classes are more likely to lead to higher impact and even transformational science than others.

Most of the targeted programs rely on triggers from gamma-ray telescopes, X-ray telescopes, or other sources. There is always a risk of unavailability of triggers for individual classes of target objects depending on future operations of these telescopes. Given the wide variety of source classes proposed for ThunderKAT ToO/monitoring observations, the "average risk" for this program is small.

Night-time observing for ToO events will be necessary for fully simultaneous MeerLICHT observations, although monitoring could occur day or night.

2.2. Resources

The team is large and has remained active in recent years. It is also experienced, being composed by scientists who have by-and-large driven the field of radio follow-ups of various classes of transients in the last decade. The team has been very visible within the community, holding regular meetings and scientific workshops, and has a lively organizational structure, with an open membership policy.

The consortium has garnered extensive external funding, most notably for the exciting MeerLICHT experiment.

2.3. Management

The management structure is sound with a well-divided framework into technical and scientific working groups. Data rates are not high for ToO/monitoring observations and should be quite manageable for well-designed commensal searches. Data management, although very challenging, will be tractable if the existing funding can be used to provide the storage needed. The release of proprietary data after 1 year is reasonable, but there do not appear to be any detailed plans for serving this public data. This represents a modest risk.

2.4. Technical Aspects

The ToO/monitoring part of this project uses conventional observing and data-reduction techniques and thus seems to pose no new technical risks. The main advantages of MeerKAT itself are its high instantaneous sensitivity and the potential availability of large amounts of ToO time, which is often difficult to obtain in facility-type instruments such as the JVLA. However, the Panel notes that making full use of this sensitivity at L band will require achieving a very high dynamic range in short snapshot images. This may or may not be possible even with the sophisticated approaches described at the end of Section 6 of the ThunderKAT LSP proposal. Only when the full dynamic range is reached can the continuum sensitivities (and hence source detection rates) be achieved.

For the commensal observations, the necessary agreements with other LSPs are in place. The nearly real-time detection of transients relies on fast and complex database operations that have yet to be used in routine day-to-day operations of existing instruments. This will be interfaced with multi-wavelength catalogs for source identification, once the proper evaluation of image artifacts and false positives is taken into account. It appears that substantial work is needed to bring the whole system into a state of rapid and efficient “science quality” data production mode.

2.5. Scientific Aspects

In general, the assumptions about source populations, physical processes and expected rates are well justified, and they should not contribute significant risk. However, there is always the possibility that small deviations from assumptions will result in a reduced number of detected sources. In this section, we detail the impact of having only the UHF, L, and S bands for the first few years of the MeerKAT science program.

The commensal part of this project has the possibility of detecting hitherto new, unknown source populations. There was some concern by the Panel that the design of the commensal survey is sub-optimal. Most of the LSPs being used for ThunderKAT are carrying out deep observations covering small solid angles. To maximize the number of bright transients worthy of detailed multi-wavelength follow-up it is preferable to observe wide and shallow (see Appendix of Mooley et al. 2016, ApJ, 818, 105). On average the transient sources found in a deep, narrow survey will be fainter and more difficult to follow up.

L band lies in the “intermediate frequency gap” (Altschuler et al. 1989, AJ, 84, 1784) between lower frequencies (where extrinsic variability caused by plasma effects such as interstellar scintillation dominates) and higher frequencies (where intrinsic variability is stronger). ThunderKAT suffers from the loss of X band on MeerKAT.

The Panel notes that MeerKAT is not the first instrument to carry out an unbiased exploration of transient phase space on timescales of 1 to 1000 seconds. The CHILES transient project (PI: Chomiuk) should have taken enough data at this stage to give the ThunderKAT team some sense of what types of transients could be expected at L band in a narrow, deep survey. Several years of JVLA randomly pointed archival data (wide, shallow) have also been analysed looking for such transients (PI: Hallinan). We think it would be prudent that the team (and the SKA SA office) look at the results of these on-going efforts before dedicating significant amounts of time to implementing the nearly real-time commensal mode. Of course, analysis of early archival MeerKAT data could also answer this question.

2.6. Other relevant aspects

The plan to use MeerLICHT is a strong and unique aspect of this proposal, introducing a new and powerful axis of serendipity.

X-ray and gamma-ray observations are required to take maximum advantage of monitoring of flaring sources, and searches for ULXs. The competitive time allocation and future availability of sufficient time on CXO, XMM, Swift, and NuSTAR is a risk. X-ray all-sky monitors are required for some triggers: HMXT is not yet successfully launched, so good coverage depends on continued successful operation of Astrosat and later successful implementation of HMXT. VLBI is important for some source classes, but is not as readily available for southern sources.

3. Suitability and Uniqueness

Positives: The main advantages of MeerKAT are its instantaneous continuum sensitivity and the availability of large amounts of ToO time. Also unique are the new MeerLICHT observations in parallel with MeerKAT. Many of these transient source classes require sensitivities of a few microJy. As the most sensitive instrument in southern hemisphere, MeerKAT can contribute to significantly increasing the number of observed events and (of course) giving unique southern hemisphere coverage.

Negatives: The 2016 ThunderKAT LSP is disadvantaged by the loss of X-band receivers after the 2010 LSP. Despite the excellent sensitivity of L band and the pending deployment of S-band receivers, the simple fact is that most of the science proposed here is better done at shorter wavelengths. Most synchrotron transients are brighter and evolve more rapidly at high frequencies (peak $S \propto \nu^1$, not $S \propto \nu^{-1}$ as stated in Section 3 of the proposal). As a case in point, the authors point to a nice result from the JVLA observing a dwarf nova (T Pyx) in outburst as motivation for being able to do the science at L band. However, the source was barely detected at L band, and not at all during the initial brightening phase. The well-characterized spectrum, important for estimating the source brightness and geometry, requires measurements up to $\nu \approx 30$ GHz.

The lack of spectral-index information is a related handicap for a deeper understanding of the physics for many of the sources considered here. Despite the large fractional bandwidth of MeerKAT at L band, it is not well matched to characterizing the cm-wavelength spectrum, especially as it transitions from optically thick to optically thin. To some extent, this can be supplemented with follow-up observations at other radio observatories.

Some components of the proposal suffer less. The final rankings reflect this risk-adjusted preference for some components over others. For example, a blind search for GRB afterglows is certainly done better at higher frequencies, where sources are brighter and vary more strongly. However, L band offers a large field-of-view, and with some preparatory work to understand GRB afterglows at low frequency, this may represent a good use of MeerKAT's capability. The blind search is commensal, but understanding the results warrants targeted follow-up ToO.

Likewise, searches for SN Ia counterparts and the afterglows of short hard bursts, where the ambient density is small, are well suited to L band observations.

4. South African Capacity Development, and Outreach

The team appears well engaged in the development of a stable long-term base of South African astronomers, having already trained, with some level of recognized success, a number of postgraduate and postdoctoral fellows in the last 2–3 years. Also, it is commendable the fact that capacity development in South Africa has been pursued with early-science works (KAT-7 papers).

Notably, public outreach activities from the ThunderKAT team have already achieved significant recognition and a range of ongoing programs demonstrates the commitment to this aspect of the program.

5. Recommendations and Miscellaneous Items

The Panel recommends allocating the full time request, if possible, to the CV and SNe Ia ToO/monitoring components of ThunderKAT. We also recommend the GRB component but suggest that the focus of the GRB program should be for short bursts rather than long bursts, and thus the amount of telescope time could be reduced. The trigger criteria for SNe Ia should be tightened to a distance limit of D < 10 Mpc unless the team can show that more distant SNe Ia would lead to equal or stronger constraints than already exist in the literature. In Section 2.4 we note that to achieve the required rms noise levels for their science, the team needs to address a technical challenge, showing that they can make high dynamic range images with short snapshots.

The commensal search for image-plane transients should proceed as is. In Section 2.5 the Panel raises some concerns about the design of this part of the survey (i.e. frequency, field of view and depth) and makes several recommendations that should be investigated before SKA SA or the team dedicates too many more resources.

The proprietary period of 1 year for all ToO/monitoring data is reasonable provided the team follows through on its promise to promptly disseminate commensal transient discoveries in a timely way (via ATels, GCNs, etc.).

V. Other Recommendations

a. SKA SA Support of EPO Efforts

Each LSP was asked to describe their efforts both in education and public outreach (EPO) and in South African capacity development. It is the Panel's view that the LSP teams are well-positioned for carrying out capacity development. The LSPs are composed of world-leading scientists who have the expertise and are in a position to contribute to training the next generation of South African researchers.

The same cannot be said of the EPO effort. While there were some exceptions (e.g. MeerTime, LADUMA), the Readers noted that most of the EPO proposals were generic and lacked excitement. The Panel did not consider EPO plans in their rankings. It is the Panel's view that most individual LSPs lack the "critical mass" to carry out effective EPO programs. It is the Panel's recommendation that SKA SA take on the responsibility for coordinating and creating an effective EPO program that will serve all of MeerKAT.

b. A Uniform Data Proprietary Period

The LSPs were asked to define their own preferred proprietary period. There was a large variance among LSPs for when the data would be released, with periods N typically between 12 and 24 months. Some LSPs would only release data N months after *all* observations were done, while other LSPS would release data N months after a *single observation* was made, or N months after all the data had been collected for *an astronomical target.*

The Panel recommends that the SKA SA office develop a comprehensive data release policy that applies to all MeerKAT observations. The trend in large facilities is toward shorter proprietary periods with some NASA missions making all *raw* data public immediately. A common proprietary policy at other radio observatories is to make the raw data public 12 months after the observations. Extensions can be granted by the Observatory Director on request for extenuating circumstances, e.g. data being used for PhD student theses, long-term timing of individual pulsars, etc.

c. Serving Higher Order Data Products. Release and Long-term Curation

All of the LSPs have been encouraged to identify the resources that they will need to process data and store the final data products. This generally has been a good thing as it has led to substantial resources being identified outside of the MeerKAT project. The Inter-University Institute for Data-Intensive Astronomy (IDIA) has emerged as a significant partner for several LSPs, for example. However, this does not mean that the SKA SA office should abrogate its responsibilities for ensuring the release and curation of science data products that arise from the LSPs. It is the Panel's recommendation that SKA SA review its plans to ensure that the wider community get access to the final data products. At a minimum there should be a memorandum of understanding negotiated between SKA SA and other relevant parties (e.g., IDIA, individual LSPs).

d. Annual Reports

The Panel recommends that each LSP submit regular reports on its progress. One key aim of these reports should be to assess how closely the scientific returns of each LSP are tracking the intended returns. Based on the outcome of such reports, observing strategies and time allocations could be modified, or LSPs could be brought to an end if the returns are not as expected. Such reports are standard practice at other facility instruments with Large proposals. For example, NRAO requests annual reports that are required to address the following questions:

- Status of data acquisition (milestones, challenges, etc.)
- Status of data reduction (not reduced, partially reduced, complete)
- Status of data release to the community
- Students participating in the program
    - Name/institution
    - Date PhD expected
    - Student funding source
- Publications that have resulted from the program
- Presentations and press releases based on the program
- Link to the program web site

Time-domain proposals with changing source lists used in timing or monitoring programs should be asked to submit updated source lists. Target of opportunity proposals should be required to provide up to date trigger criteria including information such as target type (e.g. SNe, SHB, & CV), origin of trigger (e.g. Swift, ATel), trigger event description (e.g. SNe Ia D < 10 Mpc, m_v < 15), number of events expected, and the required response window. These precautions should allow efficient scheduling of the telescope with minimal conflicts between Large and Regular programs.

e. Source Lists for Pulsar LSPs and Opening Up Timing/Search Capabilities

For both MeerTime and TRAPUM, the committee had significant concerns about the LSPs potentially monopolizing all pulsar searching and timing on MeerKAT, with the community outside the LSPs unsure what they can propose for. Therefore, we recommend the following. Before any open proposal call, the LSPs should be required to provide a source list and observing plan for each of their components that are awarded time in the coming year (which ones are awarded time, and how much, will have to be decided by SKA SA based on receiver and backend availability and scientific and technical priorities). These plans will be made public, so that proposers will know what overlaps might exist with proposals they are considering. This is similar to what GTO observers do on NASA missions with both GTO and GO time available. As noted above, we recommend that the LSPs be required to submit a progress report as part of an annual review, showing what has been achieved so far.

For TRAPUM, a critical part of that review should be how the analysis and candidate evaluation and confirmation is proceeding. For MeerTime, a critical part of that review should be the success in achieving ~100 ns timing residuals for MSPs, a demonstration of results from the other timing programs, and the success with observing, processing, and disseminating the 1000 pulsar array data.

In addition, we recommend that MeerTime should have a clear policy for how outsiders could ‘contribute’ new pulsar discoveries to be timed by the LSP. If an outsider discovers a new MSP that might be relevant for the MSP component, or a slow pulsar that should be added to the 1000 pulsar array, can they notify the MeerTime project and get timing data taken without losing their ability to publish their discovery? Terms for this kind of sharing should be worked out with SKA SA and posted on the project website, to prevent any future misunderstandings.

f. Time allocation, Overhead and the Fraction of Time for Large Programs

With one exception (MALS), the Panel did not adjust the time allocation of LSPs based on the “Findings of Technical Review of proposal done by SKA SA”. Any recommendations for reduced time were based solely on science considerations. Likewise, there is a large variance in the estimates of the calibration overheads from one proposal to the next (from <10% to 20%). We view time allocation and scheduling to be the responsibility of the SA SKA office. Adjustments and final time assignments should be made by the SA SKA office in consultation with LSP team leads, using the latest performance data from the array, RFI statistics, and the results of commissioning observations.

From the onset of the MeerKAT project there has been a goal that approximately 70% of the available science time would go to Large proposals and 30% to open calls for Regular proposals. Note that Director’s Discretionary Time (DDT) is also to be allocated ~5% of the available science time. While the Panel was not given a fixed amount of telescope time to allocate, it appears to us that scheduling just the A- and B-ranked LSP projects could already unbalance the 70/30 allocation. The total time requested in A- and B-ranked components in Table 1 is ~17,400 hrs. Those members on the Panel with some experience commissioning large, complex interferometers would be pleasantly surprised if ~21,000 hrs of science time were made available over the first five years of MeerKAT operations. While estimates of telescope commissioning are necessarily uncertain, once prior commitments of science time allocated outside of this review process are accounted for, the total A- and B-ranked LSP time could account for nearly all remaining available science time. Regular open calls for proposals and a small DDT allocation are necessary to maintain the momentum of South African capacity development and to allow MeerKAT to stay nimble as the scientific landscape evolves. Given the importance of regular proposal calls, the Panel recommends that the SKA SA office take the necessary steps to maintain the balance of Large, Regular and DDT time at approximately 67%, 28% and 5%, respectively.

Appendix A. **A Timeline of Important Dates**

1. January 16, 2017. Review Panel received MeerKAT LSP proposals, technical reports and Terms of Reference.

2. March 17, 2017. Review Panel received Reader reports on MeerKAT LSPs.

3. April 24–25, 2017. Face to Face meeting of MeerKAT LSP Panel in Cape Town. A rank-ordered list of the MeerKAT science program was completed and draft LSP evaluations were written.

4. May 23, 2017. Final Panel report submitted.

Appendix B. **Summary of Key Panel Recommendations**

1. The Panel recommends awarding time to the A- and B-ranked proposal components but it does not recommend awarding time to the C-ranked LSP proposal components.

2. MALS. The Panel recommends the UHF component but does not recommend the L band component.

3. MHONGOOSE. The Panel recommends allocating the full-time request provided the team can show they can achieve the required spectral dynamic range.

4. Fornax. The Panel recommends allocating the full time request provided the team can show they can achieve the required spectral dynamic range.

5. LADUMA. The Panel recommends allocating the full time request provided the team can show they can achieve the required spectral dynamic range (especially at UHF).

6. MIGHTEE. The Panel recommends that the L-band component of MIGHTEE tentatively be granted its requested observing time, subject to the requirement that it can demonstrate to SKA SA management oversight that the rms noise continues to integrate down so that they can achieve the required total intensity dynamic range of ~10^5:1.

The S-band component of MIGHTEE had a lower science ranking than the L-band component but the Panel stills recommends allocating telescope time as it is an important complement to the L-band science program.

7. TRAPUM. The Panel recommends allocating time to four components: young pulsars (SNRs, PWNe, TeV), Fermi-LAT sources, nearby galaxies and globular clusters. The Panel does not recommend the TRAPUM Fly's Eye component. For the follow up timing component, the panel feels that the ~100 hours currently budgeted will prove to be insufficient. The panel recommends that the SKA SA office consider two alternatives for meeting this important need: (i) leave the total time for the LSP fixed, and have the TRAPUM team carve out from existing allocation any extra time they need for follow-up; or (ii) have the TRAPUM team submit regular proposals for extra follow-up time they need beyond the current estimated request.

8. MeerTime. The Panel recommends allocating time to four components: binary pulsars, millisecond pulsars, the 1000-pulsar array, and globular clusters. This allocation is subject to the requirement that the team can demonstrate very high precision (<100 ns) timing using an interferometer. Two other components, young pulsars and magnetars, are not recommended. Both programs are well-suited for regular proposal calls.

9. ThunderKAT. The Panel recommends allocating time to three ToO /monitoring components: CVs, GRBs and SNe Ia. We recommend that the focus of the GRB program should be on short hard bursts and that the trigger criteria for SNe Ia should be tightened to a distance limit of D < 10 Mpc. The Panel does not recommend the other four components. Some of this science could be proposed in the regular proposal call, or in Director's Discretionary Time in the case of rare, bright events.

   For the commensal part of ThunderKAT the Panel recommends that the team investigate existing efforts and archival datasets to help constrain the types and rates of transients expected in deep, narrow surveys before dedicating significant effort to implement a near real-time mode.

10. The Panel recommends that the SKA SA office take on the responsibility for coordinating and creating an effective EPO program that will serve all of MeerKAT.

11. The Panel recommends that the SKA SA office develop a comprehensive data release policy that applies to all MeerKAT observations.

12. The Panel recommends that the SKA SA office review its plans to ensure that the wider community get access to the final data products.

13. The Panel recommends that each LSP submit regular reports on its progress.

14. The Panel recommends that before any open proposal call, the time-domain LSPs be required to provide a source list and observing plan for each of their components that are awarded time in the coming year.

15. The Panel would like the MeerTime LSP to have a clear policy for how outsiders could ‘contribute’ new pulsar discoveries to be timed by the LSP.

16. The Panel recommends that any adjustments and final time allocations should be made by the SA SKA office in consultation with LSP team leads, using the latest performance data from the array, RFI statistics, and the results of commissioning observations.

17. Given the importance of regular proposal calls, the Panel recommends that the SKA SA office maintain the balance of Large, Regular and DDT time at approximately 67%, 28% and 5%, respectively.