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Exploration of the Unknown

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Although the discovery record of radio astronomy is outstanding, the rate of new discoveries has slowed markedly in the last decade; the time is ripe for a transformational instrument. Harwit's arguments, based on a combination of phase space and astronomical history, make it clear that the SKA must not only take a big step in sensitivity but also be technically (and, I contend, operationally) novel. The consensus that the SKA's field-of-view, and hence survey speed, must be much larger than offered by current arrays, is a first major step in the right direction. There are, however, other generic ways in which the SKA can be designed to maximise its discovery potential. But technology alone is not enough. A holistic philosophy of maximising the the "human bandwidth" is therefore proposed. The core ideas are: parallel rather than serial access to data—combining simultaneous access to many independent groups of users (via various forms of multi-beaming) coupled with a new attitude to operation which breaks, at least in part, the present-day paradigm of "science by proposal". Since the potential for surprise in data only increases logarithmically with the sample size, discoveries tend to be made by people getting a lot of telescope time and some SKA users of the future must also be given room to take observational risks. As the exploitation of the HST, SDSS and pulsar archives has shown, re-analysis of existing data is another highly parallelised route to exploration, free from the constraint of a proposal committee. What new observations to make, or where to seek patterns in archives, may be aided by the application of the "morphological" approach (MA), pioneered by Zwicky 50 years ago but largely forgotten by modern-day astronomers. MA is a way of forcing us to make unorthodox comparisons and hence to examine regions of multi-dimensional parameter space with the potential for providing new observational clues.

I believe that "Exploration of the Unknown" should formally become a 6th Key Science Programme and I propose, therefore, that additional flexibility provisions should be included in the design as long as they do not reduce the SKA's initial collecting area by more than ~15%. At this level, providing the potential for access to the "unknown unknowns" is likely to be a bargain.

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1. Introduction

Wilkinson, Kellermann, Cordes, Ekers & Lazio ([1]; WKCEL) began their paper "The Exploration of the Unknown" with the following words:

> "The SKA is conceived as a telescope which will both test fundamental physical laws and transform our current picture of the universe. However [these] are today's problems—will they still be the outstanding problems that will confront astronomers in the period 2020—2050 and beyond, when the SKA will be in its most productive years? If history is any example, the excitement of the SKA will not be in the old questions which are answered, but the new questions which will be raised by the new types of observations it will permit. The SKA is a tool for as-yet-unborn users and there is an onus on its designers to allow for the exploration of the unknown."

The SKA community must hold this "philosophy for the long-term" firmly in mind, as the design for the system begins to emerge and the pressures come on to maintain costs within a chosen envelope. WKCEL have already provided the foundations of the philosophy and have sketched in some ideas for taking "discovery" from the domain of abstract musing to that of practical realisation. More recently Cordes et al [2] and Cordes [3] have greatly extended the discovery arguments by introducing a wide range of astronomical "possibilities" (in particular associated with various types of time variable sources) coupled with quantitative analyses of the observational survey potential of the SKA. In this paper I want to take a different direction, in particular to stress the importance of scientific sociology and the need to allow for some degree of creative risk-taking.

Although astronomers have always looked to the "next big instrument" as one route to success, it was Harwit ([4], [5]) who introduced "discovery" as a topic for rational debate within the community. His singular contribution was to recognise and codify the fact that new phenomena can be systematically unearthed by the exploration of new areas of parameter space; for example, coverage of the EM spectrum with better spectral range and resolution; better brightness sensitivity, spatial coverage and spatial resolution; better temporal coverage and resolution. Harwit also pointed out that:

- parameter space is usually opened up by the application of new technology;
- discoveries more commonly emerge from the application of a new technique rather than from larger telescopes;
- design novelty is important, discoveries are often made soon after a new technique/instrument becomes available.

Harwit's ideas have become part of the intellectual "furniture" over the past 25 years. And so we have come to recognise and accept that, while discovery may be capricious, it *can* be planned for in a deterministic fashion. And with this in mind Cordes et al [2] have discussed the extent of the likely SKA parameter space in some detail and defined metrics to describe its coverage. But my thesis here is that technological innovation, allowing deeper exploration of parameter space, is not enough and that giving free rein to astronomers' sense of curiosity in the use of the SKA is just as important in creating the potential for discovery. In other words parameter space also has an abstract dimension—the "human factor"

Discovery	Instrument	Design Purpose	Forseen ?
Cosmic radio emission (1933)	Jansky telescope	Communications research	No
Solar Radio Bursts (1940)	Radar dishes	Defence radar	No
Non-thermal radiation (1942)	Reber telescope	Astronomy research	No
Radio galaxies (1950)	Special purpose interferometers	Astrometry of radio sources	No
Cosmological evolution (1950s)	Cambridge Cylinders; Sydney Mills Cross	Radio surveys/counts Radio surveys/counts	No
Noise storms on Jupiter (1955)	Washington Mills Cross	Astronomy research	No
Slow rotation of Venus (1962)	Goldstone	Spacecraft tracking	No
Compact radio sources and quasars (early 60s)	Jodrell Bank MkI; Parkes	Emission from cosmic rays; General purposes	No
Cosmic Microwave Background (1963)	Holmdel Horn	Communications research	No
GR time delay by planetary radar (1964)	Arecibo	Ionospheric backscatter	No
Spin-orbit locking of Mercury (1965)	Arecibo	Ionospheric backscatter	No
Masers (1965)	NRAO 140ft	The Sun	No
Pulsars (l967)	Cambridge 1.8 Hectare array	Interplanetary scintillation	No
Association of pulsars with supernovae (1969)	NRAO 300 foot parabola	General purposes	No
Dark Matter in galaxies (1970s)	Westerbork Synthesis Array	Source surveys/counts	No
Jets in radio galaxies (1974)	Cambridge 5km array	Mapping radio sources	Yes
Superluminal Motions in AGN (1978)	Large parabolas in VLBI arrays	Mapping compact radio sources	No
Interstellar Molecules and GMCs (1970s)	NRAO 12m	mm-wave astronomy	Yes?
Gravitational Lenses (1979)	Jodrell Bank Mk1, Mk2 Green Bank interferometer	Emission from cosmic rays; general purposes Astrometry and general purposes	No No
Megamasers (1980s)	Arecibo	Ionospheric backscatter	No
Grav. radiation loss in a binary pulsar (1974-1980s)	Arecibo	Ionospheric backscatter	No
Millisec pulsars (1982)	Arecibo	Ionospheric backscatter	No
Superluminal Motions in XRBs (1990s)	Very Large Array	High resolution imaging	No
Mysterious structures & strong magnetic-fields in Galactic Centre (1990s)	Very Large Array Australia Telescope Compact Array	High resolution imaging Radio imaging	No No
First extra-solar planetary system (around a pulsar) (1991)	Arecibo	Ionospheric backscatter	No
Mass of central object in an AGN (1995)	VLBA	Mapping compact sources	No
Size of GRB fireball (1997)	Very Large Array	High resolution imaging	No
Double Pulsar (2003)	Parkes	General purposes	No

Table 1: Some discoveries made with radio telescopes

1.1 Lessons of radio astronomy history

One lesson first drawn by Wilkinson [6] and then by WKCEL, conflicts to a degree with one of Harwit's maxims. In the metre-centimetre band it is the *large* telescopes of their day which have been at the forefront of discovery. This can be ascribed to the inherently low signal-to-noise ratio of observations in the radio compared to the optical —indeed it is only with a radio telescope the size of the SKA that one can expect to detect the radio emission from most objects detected in other wavebands. A second lesson is that the designers of these telescopes did not predict what they would be "known for". To provide more than anecdotal evidence for this I have, in Table 1, made a list of discoveries, the large telescopes which did the work, my understanding of their original design purpose and whether or not the discovery was foreseen by the designers. This is an extended version of Table 1 in WKCEL with alternative columns 3 and 4. No doubt your list would be somewhat different and there are probably a few misunderstandings in the third and fourth columns—but the message is robust enough to withstand a few errors. *Big radio telescopes preferentially make the discoveries and almost none of the discoveries was anticipated by the telescope designers.* It follows that, however much we have become attached to the Key Science Programmes, we must *expect* that the SKA's place in astronomical history will not be found writ large in the pages of the various versions of the science case.

A third, less explicit, lesson is that, in the majority of cases, the people involved in these discoveries had access to lots of telescope time "as of right". A final, equally important, lesson is that the rate of discovery has slowed right down in the last ten years. Clearly the time is right for a new, transformational, instrument: the SKA.

2. Discovery with the SKA

2.1 Generic Principles

With these lessons in mind we can ask: what are the basic principles favourable to discovery? First there are *technical* design principles for the SKA which will maximize its scientific potential. WKCEL presented these and for completeness they are summarized here, albeit in a slightly different order:

1) Seek the maximum flexibility in the design
2) Plan for upgrades right from the start by adopting a modular approach to the design
3) Facilitate time-buffering and archiving of as much of the raw data as possible
4) Seek ways for the multiple re-use of expensive components
5) Begin the software development early
6) Purchase the [fast-changing] signal processing hardware late in the project

Principles 1) to 3) are generically part of the broad philosophy of discovery; principles 4) to 6) are more directly practical issues. We can extend the architectural principle 1) as follows:

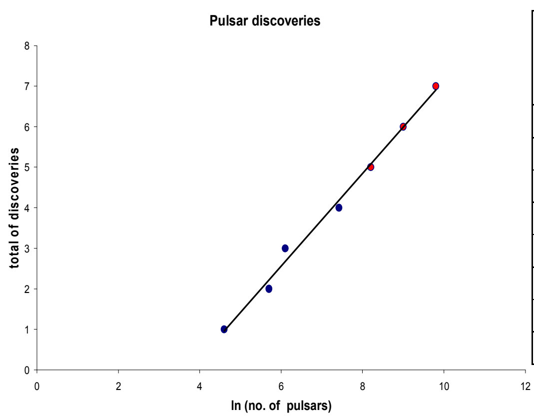
1a) Use small elements producing a wide FoV
1b) Enable them to be connected in many different ways – even if not all on day one
1c) Enable independently steerable fields-of-view – even if not all enabled on day one

1d) Bring back as many bits to the central processor as possible - even if not all can be processed on day one.

Principles 1b,c,d are also really aspects of principle 2) and are aimed at "future-proofing" the performance of the array and adding novelty to the design.

Secondly there is the issue of the amount of information gathered. In his consideration of astronomical discovery, Disney [7] has drawn attention to the information theoretic argument that the amount of "surprise" in a data set rises only as the logarithm of the number of independent data elements within it. The power of this idea is underlined by the finding of Fisher et al. [8] that the number of newly identified varieties of an animal species rises only as the logarithm of number of specimens collected—it is worth noting that this work predated the development of information theory.

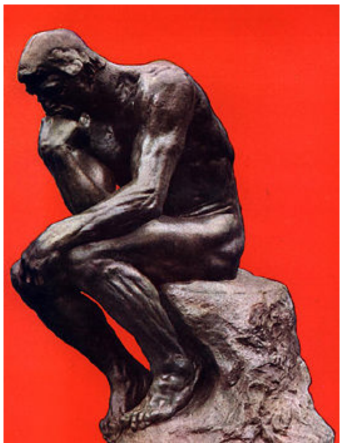
This idea can be tested against the well-documented history of pulsars. A plot of the cumulative total of further major discoveries (after the initial discovery in 1968) against the logarithm of the total number of pulsars known at the time is approximately linear. Extrapolating to the next three significant discoveries yields a prediction that the next new phenomenon (perhaps anticipated by Cordes et al [2] and Cordes [3]) will be found when the current number of pulsars is approximately doubled and that there will be a total of three new pulsar phenomena through "the SKA era". We expect that one will be a pulsar-black hole binary.



	Number of further discoveries	Number of pulsars known	ln (no. pulsars)
Initial discovery	0		
Binary pulsar	1	100	4.6
Millisec pulsar	2	300	5.7
Planetary system	3	450	6.1
Double pulsar	4	1600	7.4
Next discovery	5	3650	8.2
Next discovery	6	8100	9.0
Next discovery	7	18000	9.8

The power of the logarithm is also implicit in the well-known axiom that in order find out usefully new things in any field one needs to make an improvement by at least a factor of ten in an experimental or observational measurement. The SKA design has already implicitly taken the logarithmic idea on board since it has become axiomatic to combine raw sensitivity with a large field-of-view to create an instrument with unprecedented *survey speed* thus opening up a large volume of the Universe to study. Cordes [3] has provided a comprehensive discussion of the SKA as a "synoptic survey" instrument.

Thirdly, and leading on from the point just made, one should consider the obvious—people make discoveries not hardware! History tells us that discoveries are made by curiosity-led people[1] getting large amounts of telescope time and thus in a position to differentiate the unusual behaviour from the standard phenomenology. Many authors have grappled with the qualities and characteristics needed but no consensus on scientific creativity has ever emerged. Conscientiousness and persistence are their most desirable characteristics rather than "genius" . It is obvious, however, that having young scientists in influential positions within a team is good. They have acquired the basic skills but may not know why "something cannot be done[2] ".



"Genius is 1% inspiration and 99% perspiration. I never did anything worth doing by accident, nor did any of my inventions come by accident. They came by work."

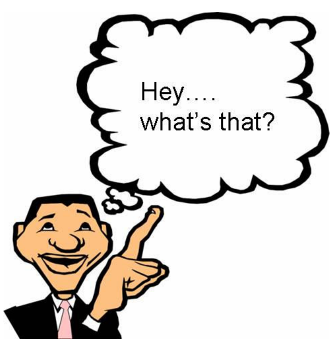
Thomas Alva Edison

"In the field of observation chance favours the prepared mind"

Louis Pasteur

2.2 Maximising "the human bandwidth"

Who can forecast which hard-working people are going to be in the right place at the right time? No-one, so the only response must be to allow as many different groups as possible to gain access to the data. Thus to extend the "reach" of the SKA we should recognize that the "brain multiplexing factor" is an additional axis to add to technology-based phase space. Another way of stating the same idea is that we should strive to maximise the "human bandwidth" – and based on the logarithmic principle, *a goal should be to have up to ten independent observations going simultaneously for at least part of the time.* .



The most exciting phrase to hear in science, the one that heralds new discovery, is not "Eureka" but "That's funny…"

Isaac Asimov

[1] A few examples: Galileo; Herschel; Hubble; Jansky; Hewish&Bell; Penzias&Wilson; Taylor&Hulse

[2] Quotation from R. Hanbury Brown about his partnership with R. Twiss on the intensity interferometer: "we were the right people at the right time and didn't know too much!"

So how can we achieve this goal? The primary requirement (Principle 1a-d) is to provide ten independent observing beams allowing:

- many groups to gain access to different real time data thus increasing the types and volume of data and the number of minds engaged;
- scope for speculative or high risk observations;
- scope for programmes requiring large amounts of telescope time e.g.
  - long-term temporal monitoring of particular targets
  - deep integrations on selected areas

Multiple beams can be provided by means of many sub-arrays of small dishes—as are currently being utilised with the VLA in the MASIV project (Jauncey et al., this conference) which makes use of five separate sub-arrays for *high signal-to-noise ratio* monitoring of time variable sources. But for *noise-limited* studies of n targets the "area-splitting" technique is inefficient; the signal falls by n for each sub-array whereas with the alternative of "time-splitting" with the full collecting area the noise is only increased by $\sqrt{n}$ for each target. Aperture Arrays, on the other hand, can provide independent beams with the full collecting area and hence are not subject to this SNR penalty. They are also part of the vital design novelty of the SKA whose generic importance has also been stressed by Harwit. In practice, the ten independent beams will likely be the sum of some sub-array beams and some aperture array beams.

Archiving (Principle 3) is another aspect of the philosophy of letting more people gain access to the data by allowing a range of different types of post processing such as:

- short-term (seconds to minutes) post-hoc beam forming in a specific direction triggered by externally-discovered transient events (targets of opportunity);
- long-term reanalysis of large data sets as new algorithms are developed;
- comparison with other archived survey data sets in the context of Virtual Observatories

Techniques for "data mining", using various visualization methods as tools to bring out subtle patterns in the data, are advancing rapidly in all areas of science. A recent success in the optical is the discovery of many new dwarf spheroidal companions to the Milky Way by trawling through the Sloan Digital Sky Survey data (e.g. Belokurov [9] and references therein). In the radio a new type of transient source, the RRATs (McClaughlin et al. [11]), was uncovered by re-analyzing the Parkes pulsar survey archive for bursts. The productivity of the HST, SDSS and pulsar archives underlines the fact that money spent on saving the data is money well spent.

There is another important sociological advantage to multiple observers gaining access to the SKA. Harwit has pointed out the importance, for discovery, of the competitive environment into which each new telescope is born. But the SKA will, for many decades, be unique and hence it will be healthy if it can provide its own competition—the philosophy of maximizing the human bandwidth, by designing the SKA system with parallel rather than serial access in mind, inherently provides this.

2.3 Gaining access to SKA data

Providing the *means* of maximising the human bandwidth is not enough—we must provide the opportunity. In its role as a common-user facility the SKA will be operated in part as an analytic tool doing. "science by proposal" where the question to be answered is known. Brains

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therefore also come into discovery parameter space via the time allocation committee—but here they may hinder rather than help! In its role as a discovery instrument we must, therefore, develop and allow new observing paradigms for the SKA, providing people the "room" to make discoveries. One should not always have to ask permission to explore some ideas which may be tentative or based on a hunch and hence hard to justify in the formal sense. Allowing a degree of risk taking is also more likely to attract scientists with the personality trait, "cognitive complexity", which favours discovery (Hollingsworth [11]).

One of the new observing paradigms is already implicitly in place. In contrast to the operational mode of current large radio telescopes, dominant fractions of the SKA observing time are likely be given over to the extensive surveys underpinning the Key Science Programmes. These are likely to be carried out by large "legacy" consortia and will provide a rich source of data able to be mined for products other than those required for the KSPs. Access to these data will become available to the community through the data archive after a proprietary period. The HST and SDSS have shown how effective this can be in generating additional science products. A judgement issue will be whether, when the data are being taken, to open the "collection window" more than is strictly necessary for the KSP in question. With only a modest extra observing load, can we add qualitatively to the survey data set being taken and hence increase the post-observation discovery potential via data mining?

What about other observing paradigms? Discoveries are much more likely to be made by experts completely familiar with the equipment than by astronomers comfortable only with standard observing techniques and standard software. Although not stated explicitly, this opinion was implicit in WKCEL's list of other possible routes to SKA data, thus we should seriously consider:

- Awarding some time, as of right, to successful groups or collaborations on the basis of their past record – a "rolling time allocation grant" which can be sustained or closed down on the basis of performance integrated over several years.
- Allowing a definite fraction of observing time for high-risk or unproven new-style observations. The availability of independent beams will help to make this feasible without compromising conventional observing programs.
- Building a system that allows new user-produced software to be employed, for example for adaptive beam-forming. (Given the scale of the SKA it is unlikely that user-produced hardware will ever be employed.) .

3. Exploring Parameter Space – the Morphological Approach?

Cordes et al [2,] identified a range of new types of objects and phenomena which might, potentially, be observed with the SKA and also made the first detailed consideration of the SKA's multi-dimensional parameter space, concentrating on potential measurements under our control. New objects or unexpected phenomena are likely to be uncovered in previously unconsidered regions but, as Cordes et al [2] recognized, the problem arises in defining these areas. In attempting to think about the problem purely phenomenologically, I drew up the following crude sketch to illustrate the potential for cross-comparisons in the basic data, stressing the need to look for *patterns* (which does not automatically imply periodicity).

(Polarised) Intensity
- Strong
- Weak

Temporal variations
- slow-rate patterns
- fast-rate patterns
- single bursts

Spatial variation
- small-scale patterns
- large-scale patterns

Spectral features and variations
- narrow-band patterns
  - → new lines; variable lines
- broad-band patterns

Obviously some simple inter-relations are already standard radio astronomical observables: (polarised) intensity temporal variations of individual targets; small-scale spatial patterns in (polarized) intensity images; (temporally variable) spectral features (lines) etc. Cordes [2] has extensively explored the possibilities of finding new types of transient source lurking broadly within the phase-space plane defined by temporal patterns vs intensity. Other simple inter-relations have not yet been exploited: for example large scale patterns in spectral line images and large scale patterns in polarized intensity images. These are in fact the basis of two of the SKA KSPs (involving Dark Energy and the Magnetic Universe) developed from astrophysical thinking. They could also have been sparked into life by this programmatic approach.

This line of thought led me on to Zwicky's "morphological astronomy" and the discovery that he had developed the "inter-relation" method to the extent of writing two books on it (Zwicky [12] [13]). "General Morphological Analysis" (GMA) has now become a respectable means of attacking messy, ill-formulated problems encountered in operational research. GMA can be used for an exploration of the SKA's multi-dimensional parameter space, for encouraging a free association of ideas and making unorthodox comparisons. The hidden assumption is that parameter space is occupied to an interesting extent everywhere and it is up to us to leave no stone unturned. I refer the interested reader to the pedagogical web site of the Swedish Morphological Society http://www.swemorph.com/. It has been said[3] that only Zwicky understood how to make the morphological approach work. But given his extraordinary prescience perhaps we should try harder. I leave this effort to another paper.

4. Exploration of the Unknown (EoU) as a Key Science Programme - but at what cost?

In 2004 when considering the potential Key Science (level-0) Programmes the SKA Science Working Group (then working as the Level Zero Science Committee, LZSC) stated: "… the

[3] Freeman Dyson: Nature, **435**, 1033, 23 June 2005

topic of "serendipity" does not meet the [level-0] definitions… The recommendation of the LZSC is therefore that serendipitous discoveries and the expansion of phase space not be included as level-0 science but that serendipity be explicitly included in the science case as an additional motivation for building the SKA."

I contend that we should be bolder. To keep faith with the astronomers of the future we must recognise that the flexibility required for the EoU deserves to be a costed part of the design and hence become a KSP. But we proponents of EoU have to accept that there will be an additional cost burden from designing in the required level of flexibility. How much is reasonable? This is purely a judgement call but my suggestion is that, as a 6^{th} KSP, we should be prepared to build up to ~15% less collecting area for a given overall budget in order to increase the ways in which this area can be utilized. The downside may be about $1/3^{rd}$ longer to carry out a given project to a specified signal-to-noise ratio but the upside is greater flexibility allowing more projects to be carried out and more people to get involved. It is likely to be a good bargain.

5. The "unknown unknowns"

At the time of writing the following quotation from Donald Rumsfeld, a former US Secretary of State for Defence, was much quoted—often incorrectly. It was often, also incorrectly, derided as typifying his confused thinking. But in fact it is the embodiment of clarity and is useful for codifying thoughts about how to find out things in science—as my commentary (to the right) is intended to show.

As we know,
There are known knowns.
There are things we know we know → *"read the literature"*
We also know
There are known unknowns.
That is to say
We know there are some things
We do not know. → *"write the proposal"*
But there are also unknown unknowns,
The ones we don't know
We don't know. → *"hold fast to the vision"*

The vision is that of seeing the SKA as a holistic system prepared for discovery by means of:

- Large data collecting power: from raw sensitivity plus large FoV plus multiple beams able to be dedicated to specific tasks or specific groups.
- Large human bandwidth: from multiple independent beams plus a complete data archive.
- Freedom to take risks: from multiple independent beams plus new time allocation paradigms.
- Internal competitiveness: from teams operating on multiple independent beams and free access to the archive.

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