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The Exploration of the Unknown

P. N. Wilkinson[a] K. I. Kellermann[b] * R. D. Ekers[c] J. M. Cordes[d] † T. Joseph W. Lazio[e] ‡

[a]University of Manchester, Jodrell Bank Observatory, Macclesfield, Cheshire SK11 9DL, United Kingdom; pnw@jb.man.ac.uk

[b]National Radio Astronomy Observatory, 520 Edgemont Road, Charlottesville, VA 22903-2475, USA; kkellerm@nrao.edu

[c]CSIRO, Australia Telescope National Facility, P.O. Box 76, Epping, NSW 1710, Australia; Ron.Ekers@csiro.au

[d]Cornell University and NAIC, Ithaca, NY, 14850, USA; cordes@astro.cornell.edu

[e]Naval Research Laboratory, 4555 Overlook Ave. SW, Washington, DC, USA; Joseph.Lazio@nrl.navy.mil

The Square Kilometre Array is conceived as a telescope which will both test fundamental physical laws and transform our current picture of the Universe. However, the scientific challenges outlined in this book are today's problems—will they still be the outstanding problems that will confront astronomers in the period 2020 to 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 that 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. We outline a philosophy for the design and operation of the SKA that can lead the radio astronomers in the 21st century to add to the many discoveries of new phenomena made by radio astronomers in the 20th century.

1. Prologue

> *"Now my own suspicion is that the Universe is not only queerer than we suppose, but queerer than we* CAN *suppose": J. B. S. Haldane*

Most of the phenomena we observe today, using telescopes to observe across the electromagnetic spectrum, were unknown a few decades ago and, to an amazing extent, were discovered by radio astronomers using increasingly powerful instruments and either looking for something else or just following their curiosity. Examples include non-thermal radiation, radio galaxies, quasars, the cosmic microwave background, cosmic evolution, pulsars, gravitational lensing, cosmic masers, molecular clouds, dark matter, and extrasolar planetary systems. These discoveries have changed astronomy in fundamental ways. Some discoveries resulted from increased sensitivity, others from better spatial or temporal resolution, still others by observing in a new wavelength band or even testing misguided theory. Most involved recognizing a new phenomenon and being able to distinguish it from a spurious instrumental response. This scenario is, of course, not restricted to radio astronomy. Perhaps the most spectacular example from astronomy in other wavebands was the discovery of γ-ray bursts by a military satellite—currently a major field of contemporary astrophysics.

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It is fashionable to imagine that all research follows some classical model of the scientific method—formulation of a model or hypothesis followed by experimental confirmation. Observations not based on testable theoretical predictions are sometimes called "butterfly collecting" or appeals to "serendipity" rather than "real science." Time allocation committees, referees of grant applications, and reviewers of instrument proposals tend to focus on specific questions that will be answered. Yet, astronomy is not an experimental science. We can only observe our Universe and its content with "eyes" as wide-open as possible. We cannot make little changes or experiments to see what happens, except perhaps for some areas of planetary research. So how do we plan for discovery? Despite the apparent capriciousness of our aim, history tells us that a basic requirement is to carry out systematic work with at least an order of magnitude improvement over what has been achieved before in one or more observing capabilities (sensitivity; spatial, temporal, or spectral coverage; spatial, temporal, or spectral resolution). An observing instrument which can offer major advances in several dimensions of parameter space is more likely to make transformational discoveries—history also shows that much greater sensitivity along with flexibility of operation is a wise path to follow. The sensitivity of the Arecibo telescope and the imaging capabilities of array telescopes are excellent paradigms.

Merely providing access to new areas of parameter space with new technology is not a surefire recipe for making ground-breaking discoveries, however. There are other, human, factors to take into account which are just as important for ensuring the SKA's success as a discovery instrument. We are all familiar with what is now the traditional method of using a large common-user telescope involving: i) a proposal to tackle a single small problem; ii) review by time allocation committees; iii) the award of a few hours or maybe even days of observing time; iv) the analysis of the data via a standard suite of software, and, then if all goes well, v) a publication following filtering by a referee. We dub this "the standard model" of observational astronomy and it is perhaps inevitable that the SKA will allocate much of its operations to this analytic mode. When, however, the phenomenon or problem is less well-defined, there can be a rich mix of possible "solutions," only some of which may have been explored by theorists and for which the standard model provides a poor response. It is, therefore, vital to develop a philosophy of design, operations, and data archival which allows individuals, small groups, and larger communities freedom to innovate and encourages users to explore completely new ways of collecting, reducing and analyzing data—in other words *to allow for discovery as well as explanation.*

2. The Lessons of Astronomy History

In his 1981 book *Cosmic Discovery* [2] and in subsequent articles, Harwit has addressed the question of what factors lead to new discoveries in astronomy. He argues that a large fraction of the discoveries have been associated with improved coverage of the electromagnetic spectrum or better resolution in the angle, time, or frequency domain. He also notes that astronomical discovery is often closely linked to innovative new technology introduced into the field from outside, often from the military. Consequently, many major new findings have come about more by luck than through careful planning—although what constitutes "luck" is an arguable point that we discuss in §8. Nonetheless theoretical anticipation has usually had little to do with astronomical discovery—what matters most is the implementation of powerful new observing tools.

Will progress at the rate achieved in the second half of the 20th century be likely to continue? Harwit [2] has tackled this seemingly impossible question in two ways. First by estimating the fraction of observational phase space which has presently been explored and then by comparing the number of discoveries attributable to improved instruments with the number independently rediscovered, often by totally unanticipated means, with instruments of quite different kinds. His analysis suggests that we have already seen perhaps 30% to 40% of all the major astrophysical phenomena that will ultimately be revealed by photons, cosmic rays, neutrinos, and captured extraterrestrial material. While one may be sceptical about the quantitative accuracy of this prediction, qualitatively we do not doubt that the Universe still holds plenty of surprises.

In the first half of the 21st century, powerful tools in two completely new observational regimes, neutrino and gravitational-wave astronomy, will become available, and it is very likely that they will reveal genuinely new phenomena. Nonetheless, photon astronomy is far from exhausted, and the low energies of radio photons and relative ease with which they are generated and propagate mean that sensitive telescopes in the radio regime will surely contribute their share of new discovery and understanding.

Moreover, radio observations probe a wide range of conditions—from dense gasses to dilute, highly relativistic plasmas—are sensitive to magnetic fields, and yet are not affected by absorption from dust. The fundamental (baryonic) element of the Universe, hydrogen, has a key transition at centimetre wavelengths (the 21-cm hyperfine transition). Radio telescopes routinely make the highest angular resolution observations in astronomy. These capabilities have already been exploited to study some of the most extreme conditions known, e.g., the strong gravitational fields in binary pulsars. It is no surprise that the Key Science Projects currently identified for the SKA exploit all of the above advantages, and we consider it likely that any future discoveries—be they from photons, neutrinos, or gravitational waves—will require radio observations to understand them.

Astronomy at radio wavelengths is marked by a number of differences from shorter wavelength observations, differences that make radio astronomy a powerful technique for observing the sky:

- The sky is mostly empty, which allows unfilled apertures (i.e., interferometers) to operate;
- Long coherent integrations are possible;
- Large numbers of photons are collected so that the signal can be amplified and split without any loss in sensitivity; and
- Diffraction-limited imaging can be obtained via post-processing so that adaptive optics requires no moving parts.

Table 1 lists some of the key discoveries from radio astronomy in the metre and centimetre wavebands and indicates the telescopes and the enabling new parameter space. In addition to adding weight to Harwit's [2] emphasis on the importance of exploiting new technology, several more specific lessons can be learned.

Table 1: Key Discoveries that Illustrate Discovery Space in Radio Astronomy[♯]

Discovery	**Date**	**Enabled By**[♭]	**Telescope**
Cosmic radio emission	1933	ν	Bruce Array (Jansky)
Non-thermal cosmic radiation	1940	ν	Reber antennas
Solar radio bursts	1942	$\nu, \Delta t$	Radar antennas
Extragalactic radio sources	1949	$\Delta\theta$	Australia cliff interferometer
21 cm line of hydrogen	1951	theory, $\Delta\nu$	Harvard horn antenna
Mercury & Venus spin rates	1962,1965	radar	Arecibo
Quasars	1962	$\Delta\theta$	Parkes occultation
Cosmic Microwave Background	1963	ΔS, calibration, *theory	Bell Labs horn
Confirmation of General Relativity (time delay + light bending)	1964 1970s	theory, radar, Δt, $\Delta\theta$	Arecibo, Goldstone, VLA, VLBI
Cosmic masers	1965	$\Delta\nu$	UC Berkeley, Haystack
Pulsars	1967	Ω, Δt	Cambridge 1.8 hectare array
Superluminal motions in AGN	1970	$\Delta\theta$, *theory	Haystack-Goldstone VLBI
Interstellar molecules and GMCs	1970s	theory, $\nu, \Delta\nu$	NRAO 36-ft
Binary neutron stars + gravitational radiation	1974-present	Ω, Δt, theory	Arecibo
Gravitational lenses	1979	$\Delta\theta$, theory	Jodrell Bank interferometer
First extrasolar planet system	1991	Ω, Δt	Arecibo
Size of GRB fireball	1997	$\lambda\lambda$, ΔS, theory	VLA

[♯] This is a short list covering only metre and centimetre wavelengths.

[♭] $\nu \Rightarrow$ spectral coverage; $\Delta\nu \Rightarrow$ spectroscopic resolution; $\Delta S \Rightarrow$ sensitivity; $\Delta t \Rightarrow$ short time resolution.
$\Omega \Rightarrow$ survey with ample sky coverage. $\Delta\theta \Rightarrow$ angular resolution, FoV $\Rightarrow$ field of view, $\lambda\lambda \Rightarrow$ guided by multiwavelength observations;
"theory" $\Rightarrow$ theory played a role in motivating discovery or its search space.
"*theory" $\Rightarrow$ phenomenon was predicted but discovery was independent of the prediction.

- **Discoveries with radio telescopes have set a large part of the current astronomical agenda and radio telescopes are now studying largely what they themselves discovered.**

- **The majority of the discoveries (11/17) were *not* a direct result of theory.** Although there were previous theoretical predictions in two cases, they played no role in the observational discovery.

- **The largest radio telescopes of their day (of a wide range of types) have dominated the discoveries.** This contrasts with Harwit's conclusion that (mainly optical) telescope size was not a major determinant for success. There are several reasons for this difference. Most discrete radio sources are weak, continuum-only emitters. Thus, large radio telescopes, which combine sensitivity and angular resolution, are needed to detect them and to study their characteristics. This contrasts with the situation at optical wavelengths, for which even modest-sized telescopes can observe the myriad of stars with their rich spectroscopic properties. Moreover, the sensitivity of optical telescopes is often limited by photon statistics, so it increases only as the square root of the area of the aperture. For a radio telescope working in the Raleigh-Jeans part of the spectrum, sensitivity scales linearly with aperture. It is notable that there are no filled aperture radio telescopes less than 64 m diameter in great demand at centimetre wavelengths.

- **What a radio telescope was *built for* is almost never what it is *known for*.** Almost invariably, the discoveries in Table 1 were not, often could not have been, in the minds of the designers of those telescopes. For example, Jodrell Bank was built to study meteor trails, Arecibo to study the ionosphere, and the WSRT to do weak source counts. Table 2 shows that the VLA, which is one of the most productive astronomical telescopes of all time, spent only a quarter of its time during its initial decade of operation on the key science drivers listed in the funding proposal.

- **General-purpose telescopes now dominate the discoveries.** While special-purpose instruments dominated discoveries for the first 30 years, the majority of discoveries since then have been made with general purpose telescopes—large filled aperture and arrays of dishes; these are versatile instruments whose performance can be upgraded by new receiving and signal processing capabilities. This trend follows the move to "Big Science" [3] as the cost of facilities with enough sensitivity to continue the exponential growth required for healthy science becomes too expensive for small specialized groups. This lesson encourages us to look for ways in which operational versatility can be built into an inherently common-user instrument like the SKA.

3. New Technology for the SKA

It is well established that most scientific advances follow technical innovation. De Solla Price [3] reached this conclusion from his application of quantitative measurement to the progress of science across all disciplines.

Harwit [2] pointed out the most important discoveries in astronomy often result from technical innovation with the discoveries peaking soon after new technology appears, usually within 5 years. However, as a field matures, more general purpose instruments have more impact. During the first 30 years of radio astronomy's brief history, discoveries followed technical innovation, but we are rapidly approaching the limits of obtaining increased capabilities simply by upgrading existing telescopes.

De Solla Price also showed that the normal mode of growth of science is exponential. Historical examples included the rate of discovery of elements and the number of universities founded in Europe. Some more recent examples of exponen-