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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2007 November 9th at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

M. Rowan-Robinson, *President*
in the Chair

The President. I think we'll get started. I'm told that there is at least one paid-up Fellow who is very keen to sign the book, so if there are any Fellows who have paid up, who have not yet signed the book, and who would like to be formally admitted, please stay behind at the end of the meeting.

Now it's my great pleasure to present the 2006 RAS Michael Penston Prize cheque and certificate to Dr. Kate Land of Oxford University for her thesis, 'Exploring anomalies in the cosmic microwave background'. [Applause.] You'll be hearing from Kate now, in fact — she's our first speaker.

Dr. Kate Land. The Big Bang model postulates that the Universe used to be a much hotter and denser place. A key prediction of this model is that there is a surface of last scattering for photons, when the Universe was about 400 000 years old. These photons have streamed free through the Universe ever since — cooling as the space has expanded — and form a background of radiation at a temperature of 2·73 K today (the microwave part of the electromagnetic spectrum). This cosmic microwave background (CMB) radiation was first detected in the 1960s, and it was seen to be extremely isotropic. Since then, new technology has enabled us to map the radiation with better sensitivity and resolution — and we have now observed fluctuations in the CMB (only 1 part in 100 000) that directly relate to perturbations in the energy density of the early Universe. These perturbations are the seeds of the structure we see today, which has since formed through gravitational instability.

The latest in a line of pioneering CMB experiments is NASA's *Wilkinson Microwave Anisotropy Probe* (*WMAP*). This CMB-dedicated satellite was launched in 2001, and since then three years' worth of data have been released to the community. *WMAP* has produced the 'best baby' picture of the Universe that has ever been seen, and provided high-accuracy constraints on our cosmological parameters — such as the age of the Universe being 13·7 billion years old (with an uncertainty of just 1%).

The CMB is the youngest observation we have of the Universe, thus it is our best probe of early-Universe physics such as any period of inflation. It is also the largest observation we have, and therefore powerful in constraining large-scale properties of the Universe such as the curvature of space. However, in constraining our cosmological parameters we must make assumptions about the underlying physics and our set of parameters (the model) to reduce the problem to a manageable one. For example, we generally assume that the CMB temperature fluctuations obey Gaussian statistics — a generic prediction of inflation theories. Under this assumption, all the information in the CMB map is in the two-point correlation function (or its harmonic counterpart, the angular power spectrum), and we need not worry about higher-order statistics. In cosmology we also usually assume that the Universe is homogeneous and isotropic on the large scales that we are dealing with. Indeed this 'Cosmological Principle' is a generalization of the Copernican principle, and it reduces Einstein's equations for our Universe to an analytically manageable set by allowing us to use the Friedmann–Lemaitre–Robertson–Walker (FLRW) metric to describe our space-time.

Clearly it is important to test our assumptions, as they have consequences for the inferences we draw from the data. Further, a violation of Gaussianity would tell us something interesting about the physics of inflation. A deviation from statistical isotropy (that the CMB fluctuations obey the same statistics in all directions) would violate the Cosmological Principle, and thus have wide-reaching consequences for all cosmology. Looking for such deviations from our assumptions — 'anomalies' — is what my research has focussed on, and in this talk I review some of the results.

There is a curious asymmetrical signal in the *WMAP* data of the CMB; the angular power spectrum is found to be significantly different ('significant' under the null hypothesis of statistical isotropy and Gaussianity) when evaluated using data in the northern or southern ecliptic hemispheres. This signal holds for all scales greater than about 2°. We followed up this claim by analysing the bispectrum (the harmonic equivalent of the 3-point correlation function) on different hemispheres and similarly found the bispectrum to be significantly different for the same sky cuts. Since then, other authors have found a number of statistics that vary across the sky (such as spherical wavelets, local curvature, and Minkowski functionals). Statistical properties that vary across the sky are a clear violation of the Cosmological Principle, and require some explanation. Alternatively the fluctuations may not obey Gaussian statistics in such a way that the asymmetrical feature is less improbable.

A separate 'anomalous' feature seen in the *WMAP* data is an unlikely correlation between the four largest harmonic modes of the temperature fluctuations (after the monopole and dipole). This manifests itself as a preferred frame, in which the multipoles $l = 2,3,4,5$ all have a remarkable level of symmetry. The symmetry holds up to a rotation about the z-axis of the frame (*i.e.*, there is only a preferred axis rather than a preferred frame), and this has been dubbed the 'axis of evil'. The largest harmonic modes relate to physical scales of the size of our cosmological horizon, and the existence of a preferred direction on these scales could indicate that our fundamental FLRW model is not correct.

However, before concluding that either of these results are of cosmological significance, it is very important to establish whether they could be caused by systematic errors of the satellite or through the data-analysis pipeline. Alternatively, they may be caused by some astrophysical phenomenon such as foreground contamination or gravitational lensing by large moving structures. The asymmetrical signature is maximized for hemispheres that are divided by the ecliptic plane. This is also correlated with the scanning strategy of the *WMAP* satellite and therefore may indicate some sort of systematic error. However, the scanning strategy treats both the hemispheres equally, and therefore it is hard to see how an asymmetrical feature could arise (*i.e.*, a dipole modulation).

The 'axis of evil' (AoE) correlation is also seen in the *Cosmic Background Explorer* (*COBE*) observations of the CMB, which is the only other satellite to observe the CMB fluctuations over the full sky. This was a very different satellite, with completely different systematics. The noise on these large-scales is also completely negligible, and therefore I feel we can trust that we really do have a true map of the radiation on these scales. But that still does not mean that the CMB map we observe is just cosmological. For example, we know that matter between us and the last scattering surface distorts the map on small scales by gravitational lensing. We also have large uncertainties on these scales because of Galactic foreground contamination. But so far no known astrophysical phenomenon has convincingly explained such a correlation as the AoE.

If these features are cosmological then what do they indicate? What alternative models exist that violate the Cosmological Principle while remaining consistent with the large wealth of excellent observations that we have? Non-trivial topologies of space are allowed by General Relativity, and can have Euclidean geometry. These models have space wrapped up and connected to itself, thus producing correlations between parts of the Universe that one might have thought were unconnected. Alternatively, there exist more general metrics than the FLRW metric, for example, with preferred directions such as the anisotropic class of Bianchi metrics in which the Universe can rotate. Once the assumptions of homogeneity and isotropy are dropped then there's an infinite number of possible metrics that might describe space!

As efforts are made to investigate these alternative models, it is worth bearing in mind the *a priori* nature of the statistics we have used; the significance of the anomalies is uncertain, as in many cases statistics were chosen after the anomalies were observed. A similar argument applies for the significance threshold (*e.g.*, 3σ). Our current concordance cosmological model, which finds excellent agreement with many different datasets, stands on more solid ground than these anomalies. However, we are now only limited by cosmic variance on the scales of the CMB that I have been talking about — and therefore we will never have any more relevant information. If a cosmological source to the features is proposed, but it has no observational consequences beyond the CMB features described (*e.g.*, predictions for large-scale-structure observations), then this theory cannot be tested. However, it seems likely that any cosmological explanation of the anomalies would also make unique predictions for the polarization of the CMB — which we will be able to test in the near future. [Applause.]

The President. It's a great pleasure to hear a talk that isn't just about the consensus model. So, are there any questions?

Professor O. Lahav. Kate, you showed this very interesting asymmetry in the north and south ecliptic coordinates. If you treat each hemisphere as a separate 'universe', and you do the analysis to derive cosmological parameters separately for the northern part and the southern part, which of the parameters remain robust and which are the most discrepant?

Dr. Land. The ones that change the most are the optical depth and the running, and the spectral index, because one has a bit more power, so it tips the whole spectrum; the optical depth tells you how much the small scales go up relative to large scales, so it's mainly the optical depth that changes. Ω_M and Ω_Λ are fine on the peaks, but the general tilt of the spectrum is what changes.

The President. Once again, the tilt and running of the power spectrum do probe the very early Universe, and it's very intriguing that there's an issue there. As you said, inflation is an assumption which is made, but is not really tested. So I think it's very interesting that things are a bit more open than people like to admit.

Dr. Land. You have to assume *something* to go and analyse your data. We don't really have many alternatives at the moment.

Rev. G. Barber. Is there a deficiency in the low-l-mode power spectrum, and would that be made worse if the 'axis of evil' was due to local contamination?

Dr. Land. In the first data sets we did think there was a significant low-l deficiency. In the third-year *WMAP* data set the low-l power isn't actually that significant — there's a bit of low power, but it's nothing to lose sleep over. But it does mean that it's quite hard to explain the axis with contamination, because contamination would always add power, so the fact that the power is low will make it even less likely that the data are contaminated.

Rev. Barber. Or it could be the power spectrum is not what we expect?

Dr. Land. It's a bit low on those scales, but not too significant.

The President. Well, thank you very much, that was a very interesting talk. [Applause.]

The President. Our next speaker also has an Imperial College connection [laughter] — I assure you I have no influence on the programme! Dr. Sanjeev Gupta from Imperial is going to talk about, rather topically today, 'A megaflood in the English Channel makes island Britain'.

Dr. S. Gupta. How did Britain become an island? Surprisingly the answer to this question has remained unresolved until now. Yet Britain's island status has governed its palaeogeography in relation to Western Europe, its archaeology, and ultimately its historical development. In this talk I will show new geophysical data from the floor of the English Channel that reveal morphological evidence pointing to a huge megaflood event(s) being the causative mechanism.

Twenty-thousand years ago, at the peak of the last glaciation, if you had stood at the present coastline of southern England and looked across the Channel you would not have seen the sea but instead dry land extending all the way to France. In the centre of the Channel a massive river formed from the combined Rhine and Thames rivers would have flowed westward to the Atlantic ocean. The Channel would have been dry land because during the glaciations, seawater gets locked in the ice sheets as these grow and expand and sea levels fall. In the past 2–3 million years, Earth's climate has fluctuated between cold glaciations and warmer interglacials and as a result sea levels have changed dramatically. In particular, in the past 500 000 years, sea levels have fallen by up to 120 m below present on five occasions. As a result the shallow shelf areas around Britain have become exposed to subaerial conditions. Once climate warmed and the ice sheets melted, shallow seas once more covered the English Channel and North Sea.

However, these two seas, which now join each other at the Strait of Dover, would not have been connected until the narrow gap at the Strait was created. Prior to formation of this gap, an extensive rock ridge made of chalk (that forms the famous White Cliffs of Dover) would have extended from southern England into France — this ridge is called the Weald–Artois anticline. The presence of this rock ridge meant that even during times of high sea level, the English Channel and North Sea were always disconnected. Breaching of the rock ridge to form the Dover Strait is thus the prerequisite condition for Britain to become an island during epochs of high sea level.

How did this rock ridge get breached? Up to now, it was generally thought that slow erosion over hundreds of thousands of years progressively cut this ridge back. However, a number of alternative mechanisms have been proposed. In the 1970s marine geologists from France and Britain discovered that the floor of the English Channel was carved by a series of large valley systems eroded into bedrock. These valley systems were proposed to have been eroded either by rivers during sea-level lowering, or erosion by advancing glaciers. The latter mechanism has been shown to be false as there is clear evidence that glaciers did not reach the English Channel. Prof. Alec Smith proposed an "outrageous hypothesis" in 1985 by suggesting that the valleys had been carved by a catastrophic outflow of water from a huge lake in the southern North Sea. This lake, he suggested, had been formed by the ice sheets blocking the North Sea area to the north and the rock ridge at Dover forming a southern barrier. However, he lacked the detailed evidence to convince others of this mechanism, and thus the hypothesis has remained untested.

We have used a new regional compilation of sonar data that reveals the bathymetry of the English Channel floor for the first time. This remarkable dataset has been collected over a period of 20 years by the Maritime and Coastguard Agency and the UK Hydrographic Office for civil safety at sea. Now it has found a scientific application. When we analysed the morphology of the landscape now under the sea we discovered clear geomorphic evidence to support Smith's hypothesis. The data reveal a huge valley, some 10–15 km wide and up to 50 m deep carved into the bedrock floor of the Channel. Within the valley we see a whole host of geomorphic features that taken together are characteristic of erosion by catastrophic megafloods. Features we observe include giant scours, longitudinal striations, and streamlined islands where channels in the valley have bifurcated. Such landforms are prominent in the Channelled Scabland in eastern Washington State (USA), where drainage of a huge ice-dammed lake — Glacial Lake Missoula — caused erosion of large channels some 15 000 years ago. The scale of the landforms we observe in the English Channel are similar to those observed in the Scabland, and other regions that have experienced megafloods, and thus strongly suggest that a megaflood origin for the English Channel valley systems is correct. Moreover it is quite clear that the landforms we observe could not be carved by normal river processes eroding into bedrock.

Overall our observations support the megaflood model, in which breaching of a rock dam at the Dover Strait instigated catastrophic drainage of a large proglacial lake in the southern North Sea basin. This event created a gap in the rock ridge that previously straddled the Strait thus allowing the English Channel and North Sea to connect during periods of interglacial high sea level. The flood thus explains how Britain became an island. Our discovery also provides an explanation for large-scale reorganization of palaeodrainage in NW Europe and patterns of early human colonization of Britain. The flood event(s) caused the diversion of the combined Rhine–Thames river systems to flow through the Dover Strait and through the centre of the English Channel forming the mega-Channel river. Archaeologists are also excited about our discovery. One of the great mysteries of the record of early human occupation in Britain is that early humans are missing from about 180 000 years ago to 60 000 years ago. We propose that the megaflood event may have changed the landscape so significantly that it became difficult for early humans to migrate from France to southern Britain. In addition, once sea levels rose during interglacial periods and connected the English Channel with the North Sea, the former land bridge formed by the Weald–Artois anticline would no longer have existed. Thus early humans would have been confronted by a seaway at the Dover Strait, making it exceptionally difficult to cross. The absence of evidence for early humans during the last interglacial may thus be a consequence of the megaflood event(s) we have described.

Our study may also help in understanding the surface evolution of Mars. Similar landscapes are also prevalent on the surface of Mars in the huge outflow channels. Comparison of the Channel landforms with those purportedly formed by megafloods in the outflow channels on Mars aids their interpretation. We are currently working with high-resolution topography data from outflow channels studying the mechanisms of catastrophic flooding and water flow on Mars.

The President. Thank you for that wonderful, fascinating talk. I wonder if I can ask you one thing: you mentioned human absence for a period, as a result of this flood, but I don't quite understand why people who were already in Britain didn't get on with it.

Dr. Gupta. To reconstruct populations, archaeologists use stone-tool densities — I thought geology was dodgy, but archaeology … [laughter]. They look at river terraces that are reasonably well-dated, and Britain has a remarkable archaeological background — we know that early humans were occupying Norfolk, where they would have been hit by the tidal surge 700 000 years ago. The archaeologists count stone-tool densities and what they see is that at about 180 000 years ago there are no stone tools in any of those gravels. So, there may have been humans, but there wasn't a large enough population actually to leave stone tools. The other thing is that during the peak of the glaciations, there wouldn't have been humans living here — it would have been too cold; they would have gone down to Costa del Sol, since they migrated up and down quite frequently. What probably happened was that they migrated back up as the climate warmed, they got to Calais, and they saw this huge gap that hadn't been there before, so they couldn't walk across.

Mr. M. F. Osmaston. How does your flood-rate estimate compare with that estimated for the Bosphorus and, more remotely, for the breakthrough at Gibraltar?

Dr. Gupta. The Bosphorus flood is really very disputed. What we have in the English Channel are land-forms that are distinctive for huge floods; I haven't seen any data that indicate land forms for the Bosphorus. For Gibraltar, again, we don't have any data, it is very speculative. This is something people have actually proposed that I go and look at, so we would like to go and do some surveying out there.

Professor D. Lynden-Bell. I noticed that on your pictures of Mars, your islands all have a crater at one end.

Dr. Gupta. They don't *always* have a crater; but a crater provides a buttress, so preferentially an island forms around a crater. Previously it was assumed that these were actually wakes and that the material behind the crater was actually sediment.

Professor Lynden-Bell. So you know the sense of flow is from the crater downwards?

Dr. Gupta. That's right, yes — obviously you don't have craters in the Channel, so there's no buttress; but there are plenty of islands on Mars that don't have a crater associated with them.

Dr. G. Q. G. Stanley. How long did this particular flood run for?

Dr. Gupta. I think it probably took several months. We can make a very rough estimate of the lake volume, and we know the discharge rate, so we can calculate the time from that; but these are all order-of-magnitude estimates.

Dr. Stanley. Could that have done enough scarifying in that time?

Dr. Gupta. Chalk certainly could have, so it could have been done.

Mr. H. Regnart. Presumably, river drainage as well as ice-melt built up the lake; can we assume that it was water starting to seep over the lowest part of the chalk? But then, my puzzle is, how did the acceleration from what was almost a two-dimensional seep into a catastrophic breakdown and flood occur? Was this an asymptotic process from the gradual to the catastrophic?

Dr. Gupta. The problem is we don't have any data for that area; it's remarkable that for the world's busiest shipping lane there are no digital data — I think it is shocking, although I think they are collecting some now. What is remarkable is that some of the earlier work showed from the seismic data a series of giant pot-holes, up to several hundred metres wide and eighty metres deep, and we think that those might have evolved from huge waterfalls. But again, that's something we are going to try and image, to see if we can actually identify them.

The President. Thank you very much. Our next speaker is Professor Renée Kraan-Korteweg from the University of Cape Town, speaking on 'What secrets of the Universe does the Milky Way hide?'

Professor Renée C. Kraan-Korteweg. Our location within the Milky Way hampers the study of the distribution of galaxies on the sky. Any line of sight through the Galaxy's disc passes through such dense star fields and light-absorbing dust layers that few galaxies can be recognized. This creates a so-called Zone of Avoidance (ZOA), clearly visible in, for instance, an equal-area sky projection of the apparent largest galaxies. Even dedicated multi-wavelength surveys — with the exception of H I surveys — have not been very successful in penetrating the inner $\pm 5°$ around the Galactic equator.

Why do we care about that part of the sky? Most of the fundamental questions with regard to large-scale structure, correlation functions, galaxy evolution, structure formation, and cosmology can be satisfactorily investigated in the unobscured 80% of the sky. However, the ZOA leaves some quite relevant questions in the open that pertain to our understanding of dynamics in the nearby Universe such as: Can we fully explain the dipole in the CMB? At what distance does the peculiar motion of the Local Group converge? Is the Great Attractor (GA) mass-overdensity traced by the galaxy distribution?

How can one unveil the galaxy distribution in the ZOA — on the sky *and* in redshift space? In my talk, I will highlight some results based on different, partly complementary, multi-wavelength observational approaches.

As simulations show quite impressively, the reduction in the apparent size and magnitude (and reddening) of galaxies that lie behind the Milky Way is a gradual process, due to the increasing dust column density closer and closer to the Galactic plane. Careful scrutiny of existing high-resolution optical sky surveys will reveal numerous fainter and smaller galaxies that are not intrinsically faint, but dust-obscured (close to 50 000 previously uncatalogued galaxies have been found in this way). Applying Cameron's laws to correct for these diminishing effects allows one to complement existing galaxy catalogues at low galactic latitudes, leading to a reduction of the ZOA by a factor of two to three.

Various suspected as well as newly-identified large-scale features could be identified in this way. One of the most important discoveries has been the realization that the galaxy cluster ACO 3627, now called the Norma cluster, which lies close to the GA's predicted centre, is actually a very rich and massive cluster. Follow-up redshift observations find a virial mass of $10^{15}\ M_{\odot}$. This was later independently confirmed by X-ray observations. This cluster marks, in all likelihood, the previously unidentified bottom of the GA's potential well.

Because of its relative proximity (it is the nearest rich cluster to us) the Norma cluster is not only of interest as a prominent component of the GA, but can serve as the local benchmark for evolutionary studies. A detailed dynamical analysis has already been performed and the determination of deep optical (*BR*) and near-infrared (*JHK*) luminosity functions are in progress. This cluster, furthermore, proves an ideal environment to study galaxy–galaxy or galaxy–cluster interactions. This became evident from the peculiar morphology of its member WKK6176, a galaxy that shows a jellyfish-like appearance — next to an extended (70-kpc) X-ray tail — with streams of material (luminous blobs) emanating from its disc along the major axis of the cluster as it moves through the inter-cluster medium.

But optical surveys do not penetrate the deepest dust layers where extinction exceeds 3 magnitudes. Here, the near-infrared (NIR) surveys like 2MASS were expected to revolutionize ZOA research and completely bare the extragalactic sky. While that indeed could be realized for the Galactic anti-centre, it has not been achieved in the wider Galactic Centre region. The culprit here is star-crowding rather than dust obscuration, due to the increasing sensitivity to the numerous, red, low-mass stars in the NIR bands.

At high extinction levels and star density, only radio surveys prevail. The redshifted 21-cm-line radiation of gas-rich spiral galaxies traverses the Galactic disc without hindrance. The success of this approach is demonstrated by the results of the deep, systematic H I survey performed with the *Multibeam* receiver at the 64-m *Parkes* radio telescope in Australia. This survey resulted in the detection of close to one thousand galaxies within the most opaque part of the ZOA ($\pm 5°$). It proved highly successful in tracking the GA Wall. The H I data (even though not sensitive to gas-poor ellipticals), in combination with the adjacent results from the deep, optical ZOA surveys, finds the GA to be a Great-Wall-like structure with the Norma cluster at its centre and some smaller clusters and groups forming the wall, entirely equivalent to the Coma cluster in the Great Wall.

The sampling of this GA Wall remains, however, rather sparse. A reliable mass estimate of the whole GA is still not in sight. A new window seemed to open unexpectedly following the discovery of two galaxy candidates at optical extinction levels of about 20 magnitudes, right where the GA Wall supposedly crosses the Galactic plane. They were found in the mid-infrared (MIR) *Spitzer Space Telescope* Legacy Survey GLIMPSE ($\pm 1°$). We immediately observed these dusty — hence, likely spiral — galaxy candidates with the *Australia Telescope Compact Array* (2006 July) to confirm their reality and their possible relation to the GA Wall. The analysis of the data indeed proves them be spiral galaxies that form part of the GA Wall.

The result led to the initiation of two further surveys of the GA: (*i*) a MIR survey with *Spitzer* to map a wider area around those two galaxies (time has been allocated); and (*ii*) a NIR survey of the footprint of the GA Wall where it bends across the ZOA (about 55 square degrees), with the *Infrared Survey Facility* (*IRSF*) on the 1·4-m Japanese telescope at the South African Astronomical Observatory (16 weeks of observing time were dedicated to this project in 2006 and 2007).

A similar combined NIR (*IRSF*) and MIR (*Spitzer*) survey has already been performed of the environment of the most massive spiral galaxy found to date (HIPASS J0843–36), a galaxy detected in the *Parkes* H I ZOA survey, which is (*i*) more massive than expected from hierarchical galaxy-formation models, and (*ii*) has very low probability to be found in the surveyed volume given the current parameters of the H I mass function. The NIR and MIR are the only chance to probe the surroundings of this interesting galaxy, which is completely obscured in the optical.

Given the success of the H I surveys, a much deeper H I survey (about 2 orders of magnitude) with highly improved spatial resolution is envisioned to be undertaken with *MeerKAT*. *MeerKAT* is the South African *Square Kilometre Array* (*SKA*) Pathfinder (see http://www.ska.ac.za), a radio telescope that will consist of 80 dishes of 12-m diameter to be built in the northern Karoo. *KAT* stands for *Karoo Array Telescope,* and the 'Meer' for the Afrikaans word 'more'. The 'Meer' is indicative of an extension from the originally planned 20 radio telecopes to 80 dishes following the announcement by the South African Treasury to provide funding towards a 10% *SKA* demonstrator. At this point in time, a prototype dish (*XDM*) has been built and seen first light (2007 July). A *KAT-7* prototype will be ready for testing in 2009, while the commissioning for *MeerKAT* is foreseen for 2012. Although an *SKA* technology demonstrator, *MeerKAT* will be a powerful radio telescope in its own right, and an ideal survey instrument, in particular for H I.

Hence, a deep H I survey of the GA region (one amongst other prospective surveys) in combination with the complementary, currently on-going, NIR and MIR surveys should allow a full mapping of the galaxy distribution in the GA overdensity and provide a much improved mass estimate. Given that these deep surveys will be sensitive to typical $M_\star$ and $L_\star$ galaxies all the way out to the Shapley distance, it should also shed light on the still highly debated GA–Shapley controversy, and provide a final answer as to whether the GA is at rest with respect to the CMB or whether we all — including the GA over-density — are being pulled towards the Shapley Concentration. [Applause.]

The President. Renée should be congratulated on a very nice talk, and also on these beautiful results from the programme she has been pursuing for many years; I know it has obviously been very hard work, and it's paying off now. We might have time for one question.

Professor Jocelyn Bell-Burnell. Thank you for a very interesting talk. Building on what you were saying just at the end, I was told that providing the infrastructure for *KAT* would cost far more than *KAT* itself, because they have to build a road and an airstrip, *etc*. Is that right?

Professor Kraan-Korteweg. No, at the moment what is being funded is all infrastructure. What is probably needed is more human resources, as is very often the case. There is just the funding that is coming from the South African Government — but there is a very strong interest to get international partners in, not only with funding, but also with expertise, because obviously the radio-astronomy community in South Africa is very small. We are trying to start and build it up, but it will take a while, so we are very keen on collaborations.

The President. Sounds like a very good offer! We have to stop there, so thank you very much. Our final talk is by Dr. Chris Davis of RAL, and he's going to talk about 'The NASA *STEREO* mission: improving the space weather forecast.'

Dr. C. Davis. I don't know how I'm going to follow those three talks, so I'm going to resort to cheap theatrics and ask my beautiful assistants here [Professor Richard Harrison, Dr. Andrew Breen] to come around the audience with some 3D anaglyph glasses.

The *STEREO* mission was launched last October on a Delta II rocket in Florida. I was lucky enough to be able to witness it and I'll tell you there's nothing that concentrates the mind more that watching your career strapped to the top of the biggest firework you've ever seen, hurtling into the distance. [Laughter.]

I'd like to acknowledge the *STEREO Heliospheric Imager* team at Rutherford: Chris Eyles, Danielle Bewsher, Steve Crothers, Jackie Davies, and Richard

Harrison. We also have international collaborators at the Centre Spatiale de Liège and the Naval Research Laboratory, Washington.

The *STEREO* mission is a fairly simple concept. It comprises two spacecraft, one launched ahead of the Earth and the other behind it, and they are both looking back at the Sun to observe the vast explosions that are coming out from the surface of our star. Each one of these coronal mass ejections (a very dull name for a very exciting phenomenon) contains about a billion tonnes of material travelling at a million miles an hour; if one of these storms comes towards the Earth, it can disrupt spacecraft, and it acts as a radiation hazard for astronauts. If we are to go back to the Moon and go on to Mars we really have to understand when these storms occur and in which direction they are going. And it's the direction of them that's so important for actually predicting where they go in space, because if you think about the Earth as a target in space, it's pretty small.

So, what instruments does *STEREO* carry? Well, it contains the usual suspects: telescopes that look towards the Sun, and coronagraphs which generate an artificial eclipse (they have a disc over the brightest part of the Sun and that shows up the faint atmosphere around the Sun); there are also particle detectors that can 'taste' the solar wind as it comes past the spacecraft. The UK is responsible for the *Heliospheric Imager* (*HI*). While all the other telescopes are pointing towards the Sun, the *HI* looks between the Sun and the Earth to see if it can see any of these clouds coming towards us.

By imaging at various wavelengths of extreme ultraviolet light, we can study the surface of the Sun at different temperatures, from several tens of thousands right up to millions of degrees. So for the first time with *STEREO* we are able to get a comprehensive picture of how these mass ejections evolve on the surface of the Sun and how they extend into space. Now the need for 3-D imaging is not just a gimmick. The surface of the Sun, as you will see, is very complicated and very dynamic and it's important to understand the difference between something changing shape and something moving if you want to try to deconvolve what's going on.

The sunward-pointing telescopes are working well and they are producing the sort of 3D imagery that is expected of them. [The speaker showed several *STEREO* images and movies of the Sun which could be viewed in 3D with the anaglyph glasses.] The Sun has an eleven-year activity cycle, and when the Sun is particularly active there are around two mass ejections a day; when it's not so active there are perhaps two a week. The active regions are particularly violent concentrations of the Sun's magnetic field at the surface; there is a lot of energy stored here, and it's these that act as the seat for the mass ejections. We hope that *STEREO* is going to help us understand when they occur and why they occur.

The *Heliospheric Imagers* were built by Chris Eyles and his group at the University of Birmingham. They represent an incredible piece of engineering — to image one of these coronal mass ejections, we must detect the intensity of light scattered from the mass ejection, at about 10^{-8} of the intensity of the Sun; and the Sun is just outside the field of view. This is made possible by the design of the high-precision optics and the baffles which reduce the stray light. So with these, for the first time ever, we can image the entire space between the Sun and the Earth. The *HI1* camera points just off the limb of the Sun, and has a $20° \times 20°$ field of view; the *HI2* camera has about a 70° field of view. So between the two cameras we can map right out towards the Earth.

Using the different instruments, we can trace a mass-ejection event with the solar imagers, through the coronagraphs, and into the field of the *HI*s. We are able to image the light from scattered dust, the F corona, which surrounds our Sun. The good thing about the F corona is that it is very stable, very static, and so we can measure it through several images and take away that part of the image that doesn't change, and this reveals the transient structures.

We have had some spectacularly unexpected results. The door was open on the *HI* as we flew past the Moon, and we were able to detect the dark side of the Moon. The dynamic range of this instrument is really quite amazing. When we opened the door of the other camera, we saw, quite unexpectedly, Comet McNaught. We have a wonderful sequence of observations with the solar wind blowing the tail of the comet out like a large mass spectrometer, so from each of the fronds you can infer information about the material that is being released from the cometary nucleus. In fact the first *STEREO HI* paper, from an Italian lead author, showed neutral iron in the comet tail.

Other comets interact with the ejected material we are interested in: poor little Comet Encke had its tail ripped off by a coronal mass ejection. We also detected Comet Machholz, which we observed for an entire month as it orbited around the Sun. So the *HI* cameras are helping us to understand how these mass ejections interact with planets and Solar System bodies.

While the mass ejections are what the instrument was actually designed to detect, astronomers are getting very excited because we have detected lots of noisy spots which we didn't particularly want — we're interested in mass-ejection studies — but these are all stars. We can see stars down to 12–13th magnitude. So now there is great hope that we will be able to use the stars in the field for studying variable stars and looking for extra-solar planets, and a group at the University of Birmingham along with Glenn White at the Open University are working with Danielle Bewsher in the *HI* team to identify stars and to plot light curves. We have a thermally very stable platform in space and we are able to monitor stars continuously for the time it takes them to drift across the field of view — about twenty days for a star to move across the inner field of view and about seventy days for the outer field of view. As well as studying variable stars, we hope that we will even be able to look for transiting extra-solar planets.

If you want to access the data, it's all available on-line at www.stereo.rl.ac.uk. You can have access to the data (if you want to roll your sleeves up and actually get in to look at the data) and you can have access to the movies. We have identified mass ejections from features that are moving out from the Sun and so we have an event list (we have nearly a year's worth of data now from two spacecraft). We have a gallery of our favourite pictures, most of which you have seen today, and there is documentation describing how to use the data and how to get more out of it. So to conclude, the *STEREO* mission is working extremely well and we look forward to the next several years trying to understand everything that we've seen.

The President. Well thank you for that highly entertaining talk. We have time for some questions. Have you seen any supernovae?

Dr. Davis. No, not personally, but I'm sure there must have been some.

The President. In 12th-magnitude stars, I'm not sure, but anyway, it's interesting that you have seen variable stars.

Professor D. W. Hughes. Can you limit the size of any inter-mercurial planet yet?

The President. What kind of a question is that?!

Professor Hughes. I come from Sheffield, and Vulcan, that used to be between Mercury and the Sun, is standing on top of our Town Hall, and it would be lovely if we could find something that was there. There's no reason that there shouldn't be something between Mercury and the Sun.

The President. But Vulcan was explained by General Relativity.

Professor Hughes. It doesn't have to be as big as Vulcan.

Dr. Davis. It is certainly true that we see near-Earth objects; we see asteroids with the cameras too. It is mainly American amateurs who have the time to trawl through the images looking for asteroids, but we have seen a 10th- and a 12th-magnitude asteroid moving through the field of view. What we are particularly good at is looking at that region of space between the Sun and the Earth, where near-Earth-object surveys are quite difficult to do from the Earth because the Sun's in the way; so we're actually adding to that kind of study.

Professor Hughes. You should be discovering comets as well.

The President. Do you see the zodiacal dust?

Dr. Davis. Yes.

Dr. Barbara Bromage. When you showed the movie from the coronagraph, there was a CME which went off there and it looked as though just before the main CME there was a spherical blob of material which broke off. Was I seeing things?

Dr. Davis. Probably not: there is so much structure in these events.

Dr. Bromage. Do we actually see the CMEs separate from the Sun, or do we perceive that they actually remain attached?

Dr. Davis. What's certainly been shown is that we can not only see CMEs but we can actually see the pile up of plasma occurring at the boundaries between fast and slow streams in the solar wind. So it's very difficult to deconvolve: if you see a density enhancement, is it something that's broken off, or is it something that's rotated into our field of view? The sensitivities of the instruments are dependent on the look direction of the material.

Dr. D. H. P. Jones. Comet Holmes has recently had a large increase in brightness — could that occur because it was struck by a CME?

Dr. Davis. We were talking about this the other day. We are of the opinion that we don't think a CME could impart that amount of energy. But we haven't got it in our field of view: it has quite a high declination, I understand, and we look along the ecliptic so we haven't actually seen it. It may well be coming into our field of view soon. We would have to look to see if there was anything that went off in that direction; but I would be sceptical that it would be capable of doing it, because it's a very intense increase in brightness, is that not right?

Dr. Jones. From magnitude 14 to 3.

Dr. Davis. Well that's quite a lot isn't it! [Laughter.]

Professor Hughes. There have been theories suggesting that these coronal mass ejections were triggers for cometary outbursts.

The President. Well let's thank Chris and the other speakers again. [Applause.]

I expect you've probably noticed there are people inside Burlington House again. The refurbishment programme at Burlington House is almost complete, and, as planned, the December 14 RAS Specialist Discussion meeting on 'Astronomy from the Moon' will be held in the new RAS lecture theatre. But, for the time being, on the afternoon of that meeting the A&G open meeting will be held here.

Now the reason for that is that if it was just this crowd we could probably fit in, but we think that the next meeting will have a rather big attendance, and therefore we won't actually fit into the lecture theatre. So we probably will have open meetings there in the future, but this next one we will have here. The drinks party after the December meeting will be held in the library and the new Council Room at the RAS. So after the meeting here we will go across to Burlington House and you can then see what wonders have been done there. Quentin has asked me to say that those who would like to join him in a tour of some local pub and possibly a local restaurant should meet with him in the archway afterwards. The meeting will now close, and the next meeting is on Friday, December 14.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2007 December 14th at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

M. Rowan-Robinson, *President*
in the Chair

The President. I welcome you to this monthly open meeting. The first thing I have to do is to announce the awards that have been made for 2008 at yesterday's Council meeting. The Society's Gold Medal for Astronomy is awarded to Professor Joseph Silk, FRS, of the Nuclear and Astrophysics Laboratory, Oxford, the Gold Medal for Geophysics is awarded to Professor Brian Kennett of the Research School of Earth Sciences, Australian National University, the Herschel Medal is awarded to Professor Max Pettini of Cambridge University, the Jackson-Gwilt Medal is awarded to Dr. Stephen Shectman of the Carnegie Institution of Science, and the Chapman Medal is awarded to Professor Andre Balogh of Imperial College, London. The Fowler Award for Astronomy is awarded to Dr. William Percival of the Institute of Cosmology and Gravitation, University of Portsmouth, the Fowler Award for Geophysics is awarded to Dr. Christine Thomas of the University of Liverpool, the Award for Service to Astronomy is given to Dr. Gunther Eichhorn of the Smithsonian Astrophysical Observatory, and the Group Achievement Award is awarded to the *2dF* Galaxy Redshift Survey Team. The following have been made Honorary Fellows of the Society: on the astronomy side, Michel Mayor, Professor of Astronomy at the University of Geneva, Professor Tim de Zeeuw from Leiden University, and Dr. Michael Hoskin, historian. And on the geophysics side, Dr. Spiro Antiochos of the Center for Space Research at the Naval Research Laboratory, USA.

Now before we start our very exciting talks, I want to summarize, for those of you who were not at the meeting yesterday, the current situation with STFC, and then I will say what we are trying to do about it, and then suggest something that you can do about it. Basically the CSR settlement was good for science — a 5% per annum increase; only science and the health service got above-inflation increases. The big gainer in science was the Medical Research Council. STFC, and for that matter EPSRC, got essentially 'full economic costs' and then a flat settlement in cash terms, which obviously meant difficulties because of inflation.

Now, in addition, the running costs of the new *Diamond* and *ISIS* facilities, the fact of increasing international subscriptions, and the cost of the Shared Service Centre, which is a collaboration between Research Councils, add up to a nett deficit of £80 million over 3 years. STFC decided to identify a further £40 million of savings, firstly for contingency, but mainly to allow competition between the non-core items that have been identified for possible cutting and new projects. There is a programmatic review in progress in STFC to prioritize all the possible cut items and new projects and they will bid against the £40 million. So the core programme is the exploitation of major facilities like *Diamond, ISIS*, and so on, exploitation of international subscriptions, *e.g., ALMA, Scuba 2, LHC*, and *ELT, SKA*-design studies, Aurora, robotic exploration of the Moon, completion of instruments for *Herschel* and *JWST*, *Gaia* data analysis, and *Bepi-Colombo*. Things that are described as definitely cut are *Gemini South*, ground-based solar-and-terrestrial-physics facilities, high-energy gamma rays, La Palma, the *International Linear Collider*, and £22·6 million in the STFC establishments of which £3·7 million has to be found from ATC. So then we have in limbo those things that may rise from the dead, cuts of 25% to grants, and notionally studentships, fellowships, *UKIRT*, ground-based gravitational-wave experiments, Dark Energy Survey, *Zeplin III* dark-matter search, *CLOVER* CMB experiment, *Liverpool Telescope*, *MERLIN*, Astrogrid, and 30% post-mission support for current missions (those already in orbit); and all these have to compete with new projects for the £40 million through the programmatic review. Keith is here so he can tell me if that's an incorrect summary, but I hope that it's broadly correct.

So what is the RAS doing about this? Well you may have seen that we've put out two press statements, one on *Gemini* and the other on the 25% grant cut, of which I had advance notice. Both of those press statements have gained wide coverage and we hope this may help the situation eventually. The STFC has decided to negotiate for continuing involvement in *Gemini North* as we requested, and this is a definite concession. The DIUS (Department for Innovation, Universities and Skills) Secretary of State, John Denham, has set up the Wakeham review into the health of physics. This is also an important concession and could ultimately, if you're very optimistic, lead to some help. Today we put out another press statement welcoming the setting up of this physics review and committing to full participation in it — and John Denham specifically asked me that the RAS should do that, which is a good sign, so we welcome it obviously — but also expressing dismay at what I regard as the probably unintended consequence of the merger between CCLRC and PPARC and the CSR settlement of savage cuts to astrophysics, space science, Solar System science, and particle physics. We have reiterated the consequent damage to many UK physics departments, reminding the government of the importance of our fields to the UK economy, to training of school graduates and postgraduates, and to drawing in university applicants to physics and children to the study of science. We will continue to lobby the DIUS and the Treasury on these issues and we also plan to talk to the STFC senior management about consultation issues and about the need for STFC to have a clear mission in support of astronomy and particle physics.

Now what can you do? A very important activity in the past couple of weeks is that, perhaps for the first time, we have had MPs asking questions about this. There was a question yesterday in the House. This is something the AAS does very well and we need to start doing this. You have to get your departments and universities to provide input to the Wakeham physics review because it's absolutely crucial to the survival of our fields that the Wakeham report speaks positively about them. Make sure that the STFC committees know your views on the priorities for the programmatic review. And finally, continue to work on outreach to increase public support for astronomy. Well, that's all I wanted to say and we have got a few minutes for discussion. Obviously it was a very big meeting yesterday — the discussion went on for more than two and a half hours and today we have just five minutes! But if there are any questions or comments I'd be happy to take them.

Professor A. Fitzsimmons. I'd just like to make a comment that, certainly for a lot of us in the community, one of the overriding things that we're concerned about is the consultation aspect that you placed today in the RAS press statement or certainly that you plan to talk to the STFC about. Particularly when things come out of the blue, such as the closure of *Gemini*, this puts us in a very uncertain position. We do listen to messages from STFC informing us of the importance of the strategic pathway that we should perhaps be following in our research but it's very difficult to plan a road map for our research in future years when we don't know what's going to happen from one day to the next.

The President. That's a very good point. I think this programmatic review is supposed to be completed by the end of January so by then I suppose we will have a better idea of what the future holds.

Mr. H. Regnart. Could we have a briefing provided either urgently or otherwise, perhaps in *Astronomy and Geophysics*, to assist in writing to our MPs, and has the point been made — or will it be made — that to a considerable extent medical science depends on physics and a considerable proportion, if not a large majority, of entrants to medical and dental schools have a science background and therefore there has to be the physics input and the draw of physics to make those students available?

The President. I hope all the medics have a science background! [Laughter.] That's a good point. If you go to the RAS web page you will find the various statements we've put out and I think those will give you the basis for writing a letter to MPs.

Professor M. A. Barstow. It's clear that a lot of us are going to share the pain, particularly with the closure of astronomical facilities, but I think there's one group among our overall membership that has been particularly badly hit and that's the solar-and-terrestrial-physics area, which has been almost removed in its entirety from STFC at one fell swoop, and I think we probably should pay some special attention to that particular issue. I certainly welcome some comments from people in the room who are working in the area as to what their views are about that.

Dr. M. M. Dworetsky. I understood that was only for the ground-based facilities?

The President. For ground-based STP, yes.

Professor Barstow. The rumours that have reached my ears are somewhat contradictory in that area. I think a bit of clarity as to what's going on would help.

The President. I think I heard Keith Mason yesterday saying that it was only ground-based STP. Is Keith here? Keith's nodding, it is only ground-based STP that is targeted: ground-based radars.

Mr. J. Stone. Firstly I find it difficult to correlate these cuts with the Government's announced intention to raise the number of A-level entries in physics from 24000 to 34000 by 2014 when in fact they've fallen by nearly 30% since 1991. One thing that I would say to everyone is the advice you've been given "write to your MPs" — that will have an effect; don't have the feeling that you'll just get ignored. When we had to make a decision on our position on working with the Aurora project, I set up a campaign called 'UK for Aurora' and encouraged people to write to their MPs saying that we should be involved in this, and when Lord Sainsbury made the announcement he said that public opinion was a deciding factor in that. So do please write those letters — they will be a great help.

The President. I think that's absolutely right. I think we're all stunned by the scale of this. I certainly haven't found it much fun being in the position I'm in, in the midst of this awful crisis. It's certainly the worst that I remember, but we just have to keep campaigning to improve the situation and to make sure it doesn't happen again in the future.

Now we turn to our programme. Our first speaker is Dr. Mike Griffin who is head of NASA, and he's here with a party consisting of Dr. Michael O'Brien, assistant administrator for external relations, British-born astronaut Piers Sellars, NASA European representative Dr. William Barry, and David Mould, head of public affairs. He is going to tell us about 'Lunar Exploration: the value of expanding the human range of action'.

Dr. M. Griffin. Thanks very much for inviting me to be here tonight. Coming on the heels of an address at the Royal Aeronautical Society last night, I definitely feel that I am making the rounds, and very enjoyably so.

My topic tonight actually touches on a very old debate in the space community — the relative merits of human and robotic space activity. When I was a child, even before *Sputnik*, interested in everything about space — starting with an astronomy book by the way — the debate between manned and unmanned space exploration was in full voice. It has continued for five decades since. In my judgment there has been no winner and the contentious nature of the argument has made losers of us all. Now this conflict is between those who pursue two goals that are in my view complementary: those of scientific discovery and expanding the range of human action. I have simply never seen a valid reason for a conflict between them but it does exist, and so tonight I would like to delve into some of the reasons I see as to why this is so, and at the same time offer a different and more unifying view for our space community.

As I see it, the issue at its core is one of mutual respect as much as it is any accounting of costs and rational benefits. Though this certainly has not always been so, we live today in an era in which the merits of vigorous national programmes of scientific discovery are well accepted. Scientists, the science community, and the scientific method are highly respected in all modern societies. This being so, one wonders why there is not often a more rigidly logical approach to applied political decisions, but I will leave off that [laughter]. But, if, as Shakespeare put it, "all of life is a stage", then the value of enlarging the very stage upon which we humans have to put forth our play, of expanding the scope and range of human activity, is not well recognized.

So I would like to make the case for the value of human space exploration beginning with a return to the Moon. Now, in point of fact, scientific discovery itself is a basic element of the rationale for human exploration of the Solar System. The entire history of science is intimately bound up with the observation of the world from new vantage points both physical and mental, and quite often it seems to require a change of physical venue to accomplish a needed shift in a scientific paradigm or to gather new information which was simply not available in old cases. So our reason for exploring beyond the frontier certainly does include the ability to enable scientific discovery. Exploration offers new opportunities to do exciting new science in new ways from new places, and because of this the rationale for exploration would inevitably be weaker without a component of scientific discovery. But we go too far when we say, as some have, that the only acceptable reason for manned space flight is to conduct new and better science, or that we say, as some in my country evidently have, that "exploration without science is tourism". The opposing view has, in my opinion, been best and most closely captured by our own President's Science Advisor, Dr. John Marberger, in a speech last year at the American Astronautical Society's Goddard Symposium. Jack noted that space exploration is not fundamentally about scientific discovery, rather it is fundamentally about bringing the economic resources of the Solar System within the sphere of mankind. Now to me, this is precisely right; the issue is very simple. We live here on Earth in a concentrated region of matter and energy around a rather nondescript star. There is an enormous gap of time, energy, and technological capability separating us physically from other star systems. I am not one of those who believe that travel to other stars is forever beyond our capability but I do believe that it will be a long time coming. For perspective, I would remind us all that two millennia of human endeavour separated the *QE2* from Roman triremes paddling about in the Mediterranean. Our space-faring capability has not yet, by analogy, even reached the trireme stage: we have a long way to go. So for the foreseeable future we can sense but not interact with the realm beyond our own Solar System. But within our Solar System, and even with our present rudimentary capability, we have already shown the ability of interacting with our environment by means of our people and our tools.

This presents us with a very simple choice: we can choose to bring the Solar System and its resources within the reach of our civilization or not. The President and Congress of the United states have chosen to do so from the 2005 Authorization Act for NASA, quoting: "the Administrator" (that's me), "shall develop a program to maintain a sustained human presence on the Moon, include a robust precursor program to promote exploration, science, commerce and the United States' pre-eminence in space as a stepping stone to future exploration of Mars and other destinations". The wording of the Act is significant. It requires us to promote the goal of exploration but to do so together with economic, scientific, and leadership objectives and in a way that encourages and allows further progress. Stated this way the Act promotes US leadership on the space frontier but recognizes implicitly that leadership without a worthy purpose has no point, thus the goal is not solely to explore our Solar System but to use accessible space for the benefit of mankind. It is not a goal that can be accomplished in a decade nor one that is restricted to a single destination for the purpose of planting a flag. The goal is to begin, now, to incorporate our Solar System into our way of life. So a key point must be made: exploration without science is not tourism, it is far more. It is about the expansion of human activity beyond the Earth. Exactly this point has been made by no less than Stephen Hawking who joins others in pointing out a basic truth: the history of life on Earth is the history of expansion events and the expansion of life into the Solar System is, in the end, fundamentally about the survival of the species. So, to me, exploration is, in and of itself, a human endeavour fully as noble as that of scientific discovery. Now I have learned, to my chagrin I will say, that portions of the scientific community feel deeply a lack of respect, and I can think of no other word, when I, or anyone else, implies that space exploration is not primarily about science. I believe that response is misguided; space-exploration enterprise stands, and should stand, on a much broader base. So with all of that said, when the *International Space Station* (*ISS*) is completed, I believe that the Moon is the proper next step in this enterprise and that science will be one of the early beneficiaries. This is because scientific exploration of the Moon is fundamentally important to Solar System science.

Many key questions remain to be answered. Our best current theory states today that the Earth–Moon system was formed by the impact upon a proto-Earth of a Mars-sized object, thus the histories of Earth and Moon are intimately linked. The outer layers of the Moon were thought once to be an entirely molten magma ocean which later differentiated to form the earliest crust and mantle. But if that is so, then why is there a dramatic chemical difference between the near-side and the far-side? Additionally, the size, composition, and nature of the lunar core remain almost unknown but are fundamental in understanding the origin and evolution of the Moon. The absolute chronology of impacts on the Moon is critical to understanding the inner Solar System because it's one of the few places where that chronology is preserved. However, the impact history of both Earth and Moon remain poorly understood for the first 600 million years after the proto-Earth formed and for the last 3000 million years before the present. It is believed, but not confirmed, that there was a terminal cataclysm, an intense peak in the rate of large impacts on the Moon and therefore also on the Earth about 3·8 to 3·9 billion years ago.

If so, is it a coincidence that the end of this period of heavy bombardment of Earth and Moon roughly coincides with the emergence of life on Earth? Numerous specific investigations into certain aspects of the lunar environment are also interesting. Why are the lunar poles rich in hydrogen? Are there significant deposits of water ice in the permanently-shaded polar regions or is the hydrogen in another form? Are the poles significant cold traps for other volatiles? Why does there remain a tenuous lunar atmosphere and why do we see dust lofted to high altitudes? Characteristics of the lunar regolith and the plasma and radiation environment near the Moon are both scientifically important and critical to future human exploration. Can we find ancient regolith which may record radiation and plasma environments as well as a history of past solar activity? The Moon is a unique and important future platform for astronomical observations. In particular it offers radio-quiet areas on the far side that do not look through a thick ionosphere and thus allow the use of low-frequency astronomy to access a new window into the early Universe. Just as in space exploration generally, exploration of the Moon is about broad scientific discovery. It is the first and closest location where we can learn how to extract and process and use extraterrestrial materials. It is something we must do if there is ever to be a significant human presence carrying out important activities in space.

The Moon is another world placed three days from home where we can learn the techniques necessary to live off-planet, the techniques necessary for longer voyages to Mars and beyond. Once we set out for Mars the first crew to try it will be gone for years, and when that happens it will be essential to have behind us the experience of living for months at a time on the *ISS* and the Moon. Space exploration is an enterprise for the ages. We will be doing it for as long as we are human beings. We in the United States are committed to it and we want you to join us. As President Bush said, "we invite other nations to share the challenges and opportunities of this new era of discovery".

The scenario I outlined today is a journey, not a race, and I call upon other nations to join us on this journey in a spirit of cooperation and friendship. I am here with you tonight for precisely that end. Thank you. [Applause.]

The President. I wonder if I could pose a question to start with? You mention both exploration of the Moon, and also a series of scientific questions about the Moon, and I suppose my response is: why does it have to be with human exploration? Why can't we achieve all these goals with robotic exploration?

Dr. Griffin. Well, I believe some of them can and will be, and are in the process of being achieved by robotic exploration, but some of them are best suited to being accomplished by human beings, as a collateral activity of, again, a larger effort to extend the reach of human presence and human capability. So I have never thought there has to be a doctrinaire goal that we must accomplish with humans or we can accomplish with robots. I think we human beings use the tools that we create to advantage, and each decision must be taken on its own merits at the time one takes it.

The President. Well that's very good to hear, but it is very much our view, in the Royal Astronomical Society, that space missions should be selected on the basis of their scientific merit, and not for some goal of putting humans in space.

Dr. Griffin. My view is quite the opposite. My view is that scientific merit is one reason to undertake space missions, and an enormously valid reason, one I have spent big portions of my life upon, and one in which I believe deeply, but it is not the only reason. The extension of human activity and capability into space is itself, and for the reasons I have stated, an entirely valid and entirely worthy activity and should be seen as such.

The President. Thank you very much. John?

Professor J. Barrow. Just following up the last point, it leads one to ask the question, who do you think owns the territory on the Moon and all those resources which you want us to have within our compass?

Dr. Griffin. I will stay away from that question if I may! [Laughter.] I believe it to be potentially highly charged, politically. UN treaties, to which most of our nations are signed up, declare materials beyond the Earth to be the common property of all mankind. The definition of exactly how the property is to be divided has not yet been accomplished and I don't want to be the one to do it! [Laughter.]

Mr. N. Spall. Just one quick point on which you may want to comment is perhaps in terms of the product of science from human space flight. Perhaps you could remind us of the excellent science that came out of the final Apollo mission, the *Apollo 17* mission, which I understand had an extraordinary level of productivity.

Dr. Griffin. Well, *Apollos 15, 16,* and *17* all were three-day missions, so called J-series of higher-capability lunar modules, once the initial test flights had been done; they all featured rovers and more advanced scientific equipment, and it was the results from those last three missions, in fact, that largely led to the construction of today's hypothesis of lunar origin, along with the facts that we have this annoying discrepancy between chemical composition on the near side and far side. So, much of what we know of the origin and evolution of the Earth and the Moon in today's form came from those missions.

Professor Lord Rees. One of the things that makes the NASA programme so expensive and slow is the safety culture imposed by politicians and the public. The astronauts themselves are prepared to take high risks. So don't you think manned space flight could be left at the stage where it could be done by cut-price, privately financed initiatives? [Laughter.]

Dr. Griffin. Well, Lord Rees, thank you, it's always a pleasure to interact with you. [Laughter.] But I must say, no, I don't. I have been most specific in my comments on this point in the past and I will be again. I believe that the development of space flight has suffered from being, as today's younger generation in America would say, all government, all the time. It's a neat phrase, and I think it is responsible for a much slower pace than we could have had. I think the equal and opposite flaw would be to leave government out of it entirely and put it all on commercial enterprises and private activity. That is a recipe for deciding that whatever happens when Nature takes its course is better than we could plan. I happen to think I can plan better than I can let Nature take its course. The development of aviation was characterized, if one looks back over the last hundred years, by a very, very aggressive partnership in most of the western nations, between government, laboratory, and government-sponsored developments of advances in aviation, followed by intense improvements in the efficiency of those technologies in the commercial sector. We've been doing space flight for half as long as we've been doing aviation, and progress has been nowhere near so great because we have left behind the engine of competition. But because I propose to bring into play the engine of capital's competition does not mean I think there is no rôle for government. One without the other, I think, is silly, either way.

Professor T. Shanks. Do you think that the exploration aspect of manned space flight should come out of the science budget?

Dr. Griffin. Absolutely not. I think it is a worthy enterprise on its own merit, should be funded separately, and is so in the United States, and I believe should continue to be so. It would be silly of me to say that I believe enterprise is worthy of funding on its own merits and then suggest that the money be taken from someone else.

Mr. D. Boyce. How is the direction of NASA going to change now we're starting to see the rise of commercial space flight?

Dr. Griffin. I don't know, I will need to see an actual accomplishment or three before … [laughter]. Forward progress is wonderful. I like to believe I'm one of the people who have engendered some of that forward progress by putting some actual NASA money on the line if people are able to step forward and qualify for it. But I'm not ready to say "over to you, commercial space flight sector"; I need to see some accomplishments before I can change my direction.

Professor Heather Couper. I'm just wondering, when you think back to the origins of space flight, and you had dedicated engineers like Wernher von Braun, shouldn't NASA be letting the engineers have their heads? I say this for two reasons: one is that I've been bitterly disappointed about the two space-shuttle accidents, where the engineers were overruled by the managers, but also I can't help thinking we've got a whole load of absolutely fabulous guys at NASA who come up with the most amazing ideas and really want to push forward to the next century, to get to Mars and beyond. Couldn't you let them have more of a say in the administration?

Dr. Griffin. I actually consider myself to be an engineer. [Laughter.] I think we at NASA get a lot of help from government supervision [laughter], above our heads, but that is the way of it in a democracy. I have been in the commercial sector for extensive periods of time, and then in several different aspects of the government sector — defence and civil space and others. Those who choose to work in government, in any western democracy, have some additional burdens. Decision-making is not as efficient as it can be in the private sector. It is not as quick, it is not as clean, it is not as optimal, because there are considerations that go along with the expenditure of public funds. That said, I don't see the direct relationship of that with the loss of *Challenger* and *Columbia*. The people making the decisions weren't some arbitrary set of managers who were disconnected from engineering, they were managers who had been engineers, and they made mistakes. And that does happen; I haven't met my first perfect human — and he is not me. It is often said among pilots, and I have been a pilot for decades, that the federal aviation regulations are written in other people's blood. Most of what we know about doing and not doing in aviation is the result of lessons learnt from someone else's accidents, and, I regret to say it, it's not going to be much different in space flight. We will not again make the same mistakes that brought down *Challenger*. We will not again make the same mistakes that brought down *Columbia*, and there were mistakes. But there are other mistakes lying in wait for us out there, because we are not infinitely knowledgeable, and it is sad to say it, but it will happen. It does not mean that people were evil, it does not mean that engineers were not making decisions, it means that the right decision was not made at that particular time, and it is regrettable, but it will happen.

The President. I'm going to have to stop. I can see this could go on all evening.

Dr. Griffin. It often has! [Laughter.]

The President. Thank you very much for coming and talking to us, and for answering questions.

Dr. Griffin. Thank you. [Applause.] Before Mario speaks, I would like to offer this shuttle flag of the United States and Great Britain, flown on *STS-121*, by your very own Piers Sellars, whom we stole, and who's in the audience tonight. Piers, if I haven't already put you to sleep raise your hand [laughter]. So I hope you enjoy the montage, a gift to the Royal Astronomical Society from NASA.

The President. Thank you very much. [Applause.] Well, our next speaker is Professor Mario Livio from the Space Telescope Science Institute who's going to tell us about 'The greatest scientific achievements of the *Hubble Space Telescope*'.

Professor M. Livio. [The speaker said that this was very much a personal selection of *Hubble* highlights. He started with the accelerating Universe and showed how *Hubble* had contributed observations of the most distant high-z supernovae. This had confirmed that the expansion of the Universe was accelerating due, it seems, to 'dark energy', which constitutes 72% of the Universe and about whose nature we have no idea. Compare this situation with that of the Earth where 70% is covered by water — imagine if we did not know what water was! We are currently interested in trying to derive some of the qualities of dark matter, in particular its equation of state, *i.e.*, how strong it is and how permanent it is. By measuring the most distant supernovae and galaxies we expect to reduce the uncertainty on this parameter. There are four other missions looking at dark energy at the moment.

Secondly, the speaker discussed the distance scale and the Hubble constant, *i.e.*, the age of the Universe. We can determine the recession speed using the Doppler effect but what is much more difficult is to find distance. The *HST* used Cepheids to determine distances to the Virgo cluster. The estimated value of the Hubble constant has varied by a factor of two for many years, sometimes implying globular cluster ages which were greater than that of the Universe, but it is now known to a precision of 10% and we hope to improve this to 4% within the next year.

Progress on the evolution of galaxies and determination of the early star-formation rate is a consequence of the Hubble Deep Field — the deepest exposure of the Universe ever taken. The area of sky covered by the HDF contains almost only galaxies — some 10 000 in all. Some of the galaxies look normal but many appear to be irregular or disturbed. They also appear to be smaller, which fits in well with the hierarchical-structure theory of formation. The history of the Universe is one of mergers and coalitions. By looking to $z = 6$ we are able to see what galaxies looked like when the Universe was less than a billion years old. We are able to show that the global star-formation rate for the Universe was actually higher than it is today but still less than the peak rate which happened about 7 or 8 billion years ago. Re-ionization occurred at about 400 000 years and the Universe became transparent — sources then began to appear and ionize spherical regions, which led to a complete re-ionization of the Universe. This happened between $z = 13$ and 6 and is supported by *WMAP* observations. Observations by *Chandra* and *Spitzer* help us to formulate a complete picture.

Until 1992 there were no known extrasolar planets, and in 1995 a planet was found around a normal solar-type star. Most have been found from the ground but *Hubble* did something unique. It measured an eclipse of a transiting planet (HD 209458b) so accurately that it was possible to say this planet had no satellites bigger than the Earth and no rings. Even more remarkable, by looking at particular spectral lines it was possible to determine the abundance of elements in the planetary atmosphere; there appears to be sodium, carbon, oxygen, and an escaping hydrogen atmosphere. More recently, the extrasolar planet TrES-1 has shown a bump at the bottom of the light curve which appears to indicate the occultation of a star spot. In another survey *Hubble* looked at 180 000 stars in the direction of the Galactic bulge for a whole week and 16 planetary candidates were found. These are distant stars and the result proves that the frequency of planets is the same as for the solar neighbourhood, implying the existence of billions of planets in our Galaxy.

In the area of stellar populations in M31, *Hubble* found two populations, one old (10–13 billion years) and the other young (6–11 billion years). This seems to indicate a major collision in the past which threw stars from the disc into the halo.

Hubble has caught the excitement of discovery and brought it to people's attention. The speaker finished by showing a series of images which he called "sheer beauty", including M16, η Carinae, V838 Mon, SN 1987A, NGC1300, and the Tadpole galaxy.] [Applause.]

The President. I think the images speak for themselves, but are there any questions?

Dr. A. Chapman. Looking at these spectacular photographs, and other similar images from *Hubble*, I never cease to be amazed at the parallel between the nature of telescopic images of deep-sky objects and other things seen under the microscope: bacteria, physiological tissue, the alveolar sacks of the lungs, and I wonder whether there are genuine parallels running through Nature or if it is the human eye identifying similar structures — it's a fascinating possibility. Do you have any observations?

Professor Livio. I believe it is more of the latter! At the same time, there may be, for example, things that do have something fundamental, say fractal structures, and that may be repeating over many scales, and so on. Now, I'm not suggesting any particular connection; mostly, I think our mind latches onto the symmetric parts.

Professor Barstow. When you talked about the Hubble constant and you gave that 'preview' of the latest results, it reminded me of when we were looking at globular-cluster ages, which were still somewhat larger than 13·7 billion years. What happened to that side of the story; did those ages come down, or is it just that the error bars now overlap?

Professor Livio. Well, a number of things happened: one was that the ages of globulars tended to come down somewhat; secondly, the error bars on both became better constrained; and thirdly, we discovered that the Universe is accelerating, which made its age somewhat older than previously thought.

Dr. Jaqueline Mitton. Among the topics that you have chosen as your greatest achievements, I note there were none to do with planets and the Solar System. Was there any particular reason for that, or have you had no time to include any of the Solar System achievements of *Hubble*?

Professor Livio. In 2006, I published an article for the public in *Scientific American* where I'd chosen ten topics and one of them was actually the collision of Comet Shoemaker–Levy 9 with Jupiter, so I definitely had a Solar System object in my top ten. I think that was a very spectacular event, and we were very lucky to have seen it at all because it happens maybe once every thousand years and we were able actually to see those plumes arising as the fragments entered the atmosphere, almost like nuclear mushrooms; it was pretty amazing.

The President. Last question.

Mr. Spall. May I rather cheekily suggest that *Hubble* is a good example of what Mike Griffin was just telling us about, where human space flight and robotics are joined together. *Hubble* needs human presence to maintain it.

Professor Livio. Well, there is no question that *Hubble* was built to be serviced and servicing proved in its case how to turn a failure into a huge success. Were we stuck with the original mirror, we would have been in real trouble. But through human ingenuity and the courage of astronauts, we were able to put in corrective optics and make it even better than originally planned.

The President. It cannot be denied. Mario, we thank you for a wonderful talk. Our next talk is by Professor Martin Rees — Lord Rees — and he is going to talk to us about 'Astronomical challenges for the next 20 years'.

Professor Rees. Astronomy, like all sciences, has seen surges, followed by eras of consolidation — even stagnation. The most spectacular surge was in the pioneering days of space exploration. It was in 1957 that *Sputnik 1* was launched into orbit. Only 12 years later, Neil Armstrong took his "one small step" on the Moon. But of course the *Apollo* project was an end in itself — there was no imperative to maintain the momentum, and it's 35 years since the last men walked on the Moon. Young people today enjoy the film *In the Shadow of the Moon*, but to them it's ancient history — they know the Americans went to the Moon, just as they know the Egyptians built the Pyramids, but the motives may seem almost as arcane in the one case as the other. Manned spaceflight, *via* the Shuttle and the *Space Station*, has lacked the same glamour — only the disasters have riveted public interest — but we wish Mike Griffin luck in redirecting NASA's programme in more inspirational directions. However, there have been immense 'unmanned' developments. We depend on space technology for everyday life, quite apart from the science it has given us since the 1960s.

There was another golden anniversary this year — that of the giant radio telescope at Jodrell Bank. It was finished just in time to track the first *Sputnik*. It's now called the *Lovell Telescope* and Sir Bernard is still there to celebrate this monument to his vision and persistence. It is now as much part of our national heritage as Stonehenge, but at the same time is doing amazing science, of a kind that couldn't even have been conceived when it was built. A few years ago, a binary system was discovered in which both members were detected as pulsars, and in a 2·4-hour orbit around each other. Relativistic effects are more conspicuous than in any other system. By measuring these little stars with microsecond precision, Michael Kramer and his colleagues can test Einstein's theory amazingly well. Bernard Lovell could never have guessed that, 50 years after he built it, his telescope would be using then-unenvisioned objects to test Einstein's theory with a precision ten-thousand times greater than any test available at that time.

That's an encouraging lesson, reminding us that serendipity may still lead to the greatest advances from the instruments now being planned or built. Predictions will be duller than the reality, but we can make some extrapolations 10 — even 25 — years ahead. Twenty-five years ago, in 1982, plans for *HST* were well advanced, and as we've seen it is still doing great stuff. So we can infer that the major space instruments active 25 years from now will be among those now at the concept or planning stage. The timescale for big projects is very long — depressingly so.

I'm going to mention some areas where there will be progress. The first of these concerns black holes. We have overwhelming evidence for black holes in the sense of 'gravitational pits'. They are now recognized as the engines for AGN, as was clearly conjectured by Donald Lynden-Bell back in 1969. But we still don't know if they obey a Kerr metric, though we would be astonished if they didn't, given the vindication of Einstein's theory from other tests. I'm hopeful that probes of the innermost regions of AGN will indeed offer firm clues.

Observations of quasars with redshifts exceeding 6 indicate that the cosmic dark age ended — and the first stars formed, and at a much higher redshift still — perhaps only 200 million years after the Big Bang. It's still uncertain, however, what the first 'Population III' stars were like, and how many of them formed. Some of the answers must await *JWST*, or the next generation of giant ground-based telescopes. If we are lucky, it may turn out that some Population III stars end their lives as ultra-luminous gamma-ray bursts, which could be detected at redshifts well beyond 10. If we can't find discrete objects at these ultra-high redshifts, the best hope of mapping how the primordial gas got heated and ionized may be to detect 21-cm emission from the neutral component — it's a very weak signal compared to other radio backgrounds (0·01K); but because H I emits a line and not a continuum, one can do 3-D mapping — 'tomography' — thereby probing the z-dependence of the fraction of gas that remains neutral, the scale of its density inhomogeneities, and the scale of the ionizing.

There has been astonishing progress in cosmology: we now have a 'standard' model, where the early Universe is characterized by a few numbers — the baryon/photon ratio, the density ratio of dark matter and baryons, and so forth. We'd like to understand these better. Another key number, Q, measures the amplitude of the fluctuations. This characterizes the amount of 'roughness' in the metric of the early Universe: it determines the fluctuations in the microwave background and the scale of present-day clustering. Q is about 10^{-5}. To see why its value is important, I want to engage in some counterfactual cosmology. This is analogous to what biologists do when they ask what would have happened if the dinosaurs hadn't been wiped out; or historians when they ask what would have happened if we Brits had held our ground in 1776. So, imagine a counterfactual universe, where the physics and key numbers were the same as in ours, except for a different value for Q. Were Q, say, 10^{-4}, the Universe would be a more interesting place. There would be individual disc galaxies as big as the Coma cluster. It could be a good universe to live in, especially for astronomers — the only problem is that stars may be too close together for stable planetary systems. If Q were higher still, however, say 10^{-3}, the Universe would be an inclement and violent place. There'd be no stars and galaxies because huge black holes would form soon after recombination and gobble up much of the material. What about a smoother Big Bang, with Q much smaller than its actual value? This would generate an anaemic universe where stars and galaxies might not form at all. One of my favourite magazine covers showed a red circle, with the caption "the universe when it was a trillionth of a trillionth of a trillionth of a second old — actual size". According to a popular theory, our present Hubble volume 'inflated' from a hyper-dense blob no bigger than that, under the influence of a cosmic repulsion 120 powers of ten fiercer than might exist today. The fluctuations, now stretching across the sky, could then be quantum effects imprinted at that era of inflation. In 1982, when the 'inflation' theory was new, an influential conference was held in Cambridge. We're having another one next week, to re-address the same question. Despite 25 years' of effort there is no generic argument that tells us what Q should be. We await theoretical progress that pins down the physics of the ultra-early Universe, in the way that the physics at the nucleosynthesis era was pinned down 40 years ago; or else observations of CMB fluctuations — the 'tilt' in the spectrum, the detection of tensor modes or of non-Gaussianity — which will discriminate among various models.

How big is 'our' Universe? We can only see a finite volume — a finite number of galaxies. That's essentially because there's a horizon, a shell around us, delineating the distance light can have travelled since the Big Bang. But that shell has no more physical significance than the circle that delineates your horizon if you're in the middle of the ocean. There are galaxies — almost certainly hugely more — beyond our 'horizon'. If we were in a decelerating universe, more and more of those galaxies would eventually come into view. But we're in an accelerating universe, where galaxies now beyond the horizon stay that way. They came from the same Big Bang, but we shall never, even in principle, observe them. Yet they're surely as much part of physical reality as they would be if we could one day see them as 'real' as Andromeda. I may have laboured this point but there's a reason: it's an exercise in 'aversion therapy'. If you're scared of spiders, you get used to a little one a long way away, then a closer and bigger one, and end up happy with tarantulas crawling over you.

So, the next step is this: if there are galaxies in 'our' Universe — the aftermath of our Big Bang — which will never be in causal contact, is it then much of an extra leap to envisage unobservable galaxies that came from other big bangs? There are theories that predict this: one option is 'brane worlds'; another is 'eternal inflation'; yet another is what string theorists call a 'landscape'. We don't know if these theories are right, but they're speculative science, not metaphysics. Numbers like Q, Λ, and the dark-matter density, are uniform within our Hubble volume. But if that's a tiny fraction of physical reality, maybe they're not truly universal. Settling that issue is a challenge for 21st-Century science. Many physicists hope for a uniquely self-consistent equation that will determine the recipe for the Big Bang, but they may be doomed to fail: what we've traditionally called fundamental constants and laws could be mere parochial bylaws in our cosmic patch. This latter option introduces a word beginning with A. I hesitate to speak it, because it makes some physicists foam at the mouth. However, I think they'll really have to get used to it, so my hesitation is only momentary. Anthropic arguments may be irrelevant, but they may, alternatively, be the best explanation we'll ever have for some features of our Universe.

Four hundred years ago, Kepler thought that the Earth was unique, and its orbit was a circle, related to the other planets by beautiful mathematical ratios. We now realize that there are 'zillions' of stars, each with planetary systems. Earth's orbit is special only insofar as it's in the range of radii and eccentricities compatible with life. Maybe we're due for an analogous conceptual shift, on a far grander scale. Our Big Bang may not be unique, any more than planetary systems are. Its parameters may be 'environmental accidents', like the details of the Earth's orbit. The hope for neat explanations in cosmology may be as vain as Kepler's numerological quest. Our Universe isn't the neatest and simplest. It has the rather arbitrary-seeming mix of ingredients, in the parameter range that allows us to exist.

But I wouldn't want to end on this flaky note, so I'll conclude by highlighting another challenge, one that Kepler would certainly have relished: extrasolar planets. We already know of one star surrounded by at least five planets. In the coming decades, huge numbers of planets will surely be found, with a wider range of masses. Perhaps some are 'twins' of our Earth, harbouring life far more interesting than even the optimists hope to find on Mars or Titan. And exobiology, and the study of life's origins on Earth, will surge forward. David Gross, Director of the Kavli Institute for Theoretical Physics in Santa Barbara, rates the liveliness of fields discussed at the Institute's 'workshops' by its inverse correlation with the average age of participants. By this criterion, the hottest topic is planets and protoplanetary discs. So, in conclusion, I'd highlight three broad questions for the future: can we delineate the stages whereby our cosmos evolved, over nearly 14 billion years, from its dense beginning to its present complexity; how 'special' is our observable Universe in the grandest scheme of things; and how special is our home planet, the Earth?

The President. Well, thank you, Martin, for that wonderful panorama of our subject in the future. I didn't quite understand how there could be an answer to your second question, "Is our Big Bang special, or not?"; is there a way of settling that?

Professor Rees. Well, if we had a theory that predicted with some probability distribution what the constants of Nature were, over a whole range of parts of the Universe, or separate Big Bangs, then we could ask, 'are we in a region typical of the subset in which we could have existed?'

The President. Do you think string theory is close to generating such a probability distribution?

Professor Rees. Not close, and until it can do that, we can't progress far, but certainly Mario and I delineated the range of Λs and Qs in which you can have galaxies and stars, but to put a probability metric on that will be very hard to do. I think we may learn within ten or twenty years whether there is a unique set of Laws of Nature, or whether you do have to accept that the Laws are an environmental accident of the way that our part of the Universe emerged.

The President. That will be very exciting if we are there to see it. Any questions?

Professor P. Thomas. The history of astronomy is one of surprises. It is not possible to predict surprises.

Professor Rees. So, what's your question? [Laughter.]

Professor Thomas. Would you like to speculate on … [Laughter.]

Professor Rees. Well, I wouldn't really. I think there will be lots of surprises in extrasolar planets and I think we will find more classes of gamma-ray bursts and more instances of extreme physics and so on; we may find that there are some corrections to Einstein's theory at a high level or on the Hubble scale. You may have been surprised, incidentally, I didn't even mention dark energy in what I said. Despite its importance now, it is not at all important during the formative stages of the structures in our Universe — it would be important for the last half of the Universe, but all the formation happened independently of dark energy. I think it is important to decide if the dark energy really is just Λ or some time-dependent 'quintessence', or if it requires some small change to Einstein's equations. So that might be a surprise. I mentioned the episode of the *Jodrell Telescope* and Michael Kramer's marvellous work, just an example of how instruments that are around for a long time can remain 'cutting edge'.

Professor P. G. Murdin. To put that last question in a slightly different way, are there areas of astronomy that we're a bit too complacent about, which might pay picking over?

Professor Rees. Speak for yourself! What I would say is that great precision is important even in standard branches of astronomy. Established fields should be pursued with great precision. I don't think we're complacent at all!

Mr. N. Henbest. On the subject of speculative ideas, Martin, you also haven't mentioned SETI with the *Allen Telescope* array coming along; what do you think — well, I won't ask what the chances are — would you care to comment?

Professor Rees. Well, I think we're too ignorant to say even whether it is likely or unlikely. But I think we should distinguish 'simple' life and 'intelligent' life. We don't know how likely simple life is, but I would imagine that within 25 or 50 years we will understand how life began here. Once we understand that we should have a clue as to how likely it is that there is life on these other Earth-like planets that we are sure to find. As for how likely it is that this simple life will evolve into complex biospheres like on Earth, and how likely it is that any intelligence would be recognizable by distinctive signals it sends out rather that being passive and contemplative, we just don't know. I think it is great that the SETI programme is being pursued. There is such great public interest from far beyond the normal scientific community that it is appropriate that there is private funding.

The President. I'll take one more question; Donald Lynden-Bell.

Professor Lynden-Bell. Martin, I noticed that you didn't put anything on inflation and its connection or otherwise to Λ.

Professor Rees. Well, I did implicitly mention inflation when I said that primordial fluctuations formed when the Universe was that big [gestures the size of a ball]. But of course the repulsive force that drives inflation is 10^{120} times higher than the repulsive force causing the present day acceleration, and that is a well-known problem to which I don't think anyone has a solution yet. I think the idea of scale-independent fluctuations is important; of course it goes back to Harrison and Zeldovich. In my view, that is not in itself a confirmation of inflation, but I do think that the tilt which is now found supports something like inflation, and in future we could do the other tests I mentioned, like the scalar–tensor ratio.

Professor Lynden-Bell. So you think the tilt is now definite?

Professor Rees. Yes, with new, better data, I believe it is now a 2σ result.

Professor Lynden-Bell. And that is despite the fact that the quadrupole is small, so it goes the other way?

Professor Rees. Yes.

Professor Lynden-Bell. So it's a bent tilt? [Laughter.]

Professor Rees. Yes! [Laughter.]

The President. Well, I'm going to stop you now since we have run out of time; let's thank Martin. [Applause.] Before you rush off, I'd like to welcome you to the official opening of our newly refurbished apartments, across the courtyard. There will be a short ceremony prior to the reception, which will be in our new lecture theatre, just inside the door. The first 102 people there will fit into the lecture theatre, and the remaining 60 or 70 will need to go up to the second floor to the Council Room, where there is what is called an 'AV' link, but unfortunately it is only 'V'! [Laughter.] I look forward to seeing you over in the lecture theatre in a few minutes and there is a reception which will take place on the first and second floors.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 200: κ PERSEI, β LEONIS MINORIS, 56 URSAE MAJORIS, HR 4593, AND 39 CYGNI

By R. F. Griffin
Cambridge Observatories

Orbits are presented for five bright late-type giant stars which have periods of exceptional length for spectroscopic binaries. Kappa Persei was recognized as a binary 98 years ago, but only now has it been observed sufficiently systematically for its 29-year orbit to be derived. Beta Leonis Minoris is a well-known visual binary with an orbit of high eccentricity (0·7) and a period of 38 years. It has been observed round a complete cycle, but only at the approach of the periastron passage did the double-lined nature of the radial-velocity traces become apparent. The orbit is unsatisfactory owing to apparent, somewhat systematic, deviations of the velocity of the secondary from a Keplerian curve. The widely accepted classification of the system as a whole as G9 III probably indicates the combination of an early-K giant with an F-type main-sequence object, but the spectrum has never been recognized as composite, let alone double-lined, and in fact has been designated as a particularly reliable MK spectral standard. 56 Ursae Majoris has been classified as a mild barium star, and the fact that its radial velocity showed a slight monotonic variation for 30 years therefore seemed to be of sufficient significance to warrant a note that was published to that effect in this *Magazine* in 1996. Hardly was the ink dry on that publication when the velocity trend suddenly reversed, and the 45-year orbit with $e \sim 0·56$ is now quite clear. An example of a very common sort of binary that is scarcely ever documented, although it readily yields to observations made systematically once or twice a season, is HR 4593; it is a late-type giant with a period of 35 years and a quite eccentric orbit ($e \sim 0·58$). The observations just cover a complete cycle, but in the early years they were sparser and, inevitably, less accurate than the more recent ones. A casual observation of 39 Cygni, made in 1971 with the 200-inch telescope at Palomar when the radial-velocity spectrometer there was new, was not in very good agreement with the mean of eight measurements which had been made at Lick long before and themselves exhibited a suggestive discordance between different epochs separated by 15 years. The velocity was therefore monitored, and after 20 years of apparent constancy there set in a change, which

has recently carried it past a nodal passage and allows the proposal of a preliminary orbit having an eccentricity of 0·5 and a period of 86 years.

Preamble

Celestial time-scales are not tailored to human convenience. Scientific observation has not lasted, and is not at all likely to last, for long enough to follow in 'real time' such processes as, for example, the dynamical evolution of galaxies, or even the evolution of stars except in certain atypically rapid phases.

At a less extreme level, the observation of classical 'visual' binaries (as that term has been understood until quite recently) has been recognized as a task that transcends the individual human lifetime. *So* much was immediately apparent to Sir William Herschel, to whom we owe the recognition[1] of the *genre* in 1802*. Fortunately, the Herschels, the Struves, and all the distinguished visual observers of double stars who have followed them have readily accepted that as a 'given', and have been content to accumulate observations in the full knowledge that the payoff in the form of definitive orbit determinations could not in most cases be accomplished within their own lifetimes. Present generations are just some of the beneficiaries of their far-sightedness, and are now reaping important benefits from the selfless diligence of Wilhelm Struve[5] nearly 200 years ago and from all the other observers then and since.

Regrettably, spectroscopic observers have not been so public-spirited. In extenuation, certain qualitative differences could be pointed out between visual binaries and spectroscopic ones. In general terms, among visual binaries it is those of very long period that are the easiest to measure, because — other things, notably their distances from us, being equal — they are the widest. Short-period visual systems are inevitably very close in angular terms and are correspondingly difficult to measure. On the other hand, among spectroscopic binaries it is the short-period ones that are the *easiest* to measure, owing to their large amplitudes; moreover, there is the added attraction that after only a brief observing campaign their orbits can already be determined. In the early days of radial-velocity measurement, the abundance of systems exhibiting large velocity amplitudes and short periods led, naturally enough, to a concentration of interest on such objects, almost to the exclusion of long-period binaries whose velocity changes were necessarily small as well as slow.

That situation has been greatly exacerbated in recent decades by the manner in which astronomy has developed. The majority of observers has become increasingly dependent upon large and expensive observing facilities, mostly set up under national or even international ægis at remote and expensive sites, and typically administered through time-allocation committees which see a need to demand quick results as the price of continued hospitality. Interest in, *e.g.*, short-period variable stars has accordingly flourished, but in present circumstances nobody who hopes to obtain regular access to a telescope that is in heavy demand can

*In the cited paper, Herschel proved the physical nature of binary stars through their mutual gravitation, as demonstrated by the apparent orbital motions of many pairs whose relative positions he had measured over intervals up to 25 years. Michell[2] had already shown by a statistical argument that the mutual proximity of many pairs must imply physical connection; it is a convincing argument that seems still not to be fully appreciated, as it needed to be rehearsed again comparatively recently[3] to defend the reality even of Albireo, whose status as a physical binary had been impugned[4] just because orbital motion is not yet apparent.

realistically propose observing programmes that may take decades, a lifetime, or more, to come to fruition, notwithstanding that such programmes are just what are requisite for the documentation of a large and significant class of spectroscopic binaries.

In such circumstances, observers with access to local facilities that are either under modest demand or else are ruled with far-sighted benevolence may find themselves at an advantage. It could be argued that the Lick observers were in that position a hundred years ago; they had their own on-site instrumentation, in the form of the wonderful 36-inch refractor and Campbell's *New Mills Spectrograph*[6], a combination which would readily have permitted the documentation of orbits such as (and indeed including) those that are presented in this Paper 200, but they largely neglected to make *systematic* measurements of the many slow spectroscopic binaries that they discovered, or to maintain their observations of them after the end of their great and excellent survey programme[6] of 1896–1925. On a more positive note, one might recall with gratitude the relaxed attitude of the Palomar Observatory time-allocation committee (no doubt mesmerized by the brilliance of my collaborator James E. Gunn) in granting the use of the 200-inch reflector in the 16 seasons 1971–86 for the observation of Hyades radial velocities, before they saw any major result[7] in print.

Edging now towards more specific pertinence to the series of papers of which *this* one marks a distinct milestone, I mention the support that I enjoyed from my Ph.D. supervisor and Director of The Observatories at Cambridge, Prof. R.O. Redman, in developing at the on-site 36-inch reflector the cross-correlation method of measuring stellar radial velocities. The method was largely and seemingly resolutely rejected at the time and for several years afterwards by what I regarded as the Establishment — although how we would have obtained the evidence that presently exists for such diverse objects as black holes and extra-solar planets if spectra had continued to be measured line-by-line instead of by cross-correlation is difficult to imagine. From the first (1966 February), I received a considerable share of observing time at the 36-inch telescope; quite soon after Prof. Redman's retirement in 1972 the competing demands for observing time evaporated altogether, and for more than 30 years I have had full-time use of the telescope and been, in effect, on a permanent observing run. The present series of papers was started in 1975 specifically with a view to advertising the cross-correlation method, which had still not 'caught on' at that time, by flourishing the phrase 'Photoelectric radial velocities' in the series title at short and regular intervals by way of an 'orbit' paper in every issue of this *Magazine*.

Although spectroscopic binaries were not a particular priority during the early years (1966–1975) of photoelectric measurement, yet there were enough relevant data already 'in the bank' by 1975 that I felt confident that, as long as my own interest held up and the Editors would accept the papers, I could keep the series going indefinitely. At the start, I did not really foresee more than several years into the future; in any case, it does not occur to a comparative youth that there are definite limits to 'indefinitely', one of which I actually over-stepped some five years ago with compulsory 'retirement'! Within a few years of the start of the series, the method *had* in fact caught on, and specific advertisement therefore became otiose. Indeed, the series title began to seem positively tautological by the time — which arrived long ago — when practically *all* radial velocities were being measured photoelectrically. The author's innate conservatism, however, precluded a change of title once the series was fairly under way, although it has been noted previously[8] that the *numbering* of the series did change from roman to arabic numerals after

Paper I to avoid the monstrous numbers that could be foreseen long before Paper CLXXXVIII stood up!

The above-mentioned proviso concerning the rôle of the Editors leads me gratefully to acknowledge that, in approaching the present milestone, I have obviously been extremely fortunate in encountering Editorial patience that matches my own. This series had been going for a few years before the Editors (the *other* Editors — I was myself one of four at the time) seemed fully alert to what was going on, and just when they had begun to feel some alarm lest they be regarded as a laughing-stock by astronomers at large they started also to receive occasional unsolicited expressions of approval — not orchestrated by me, I hasten to add — that dissuaded them from immediate foreclosure; and of course the longer the series continued the more embarrassing it would have been to halt it by *fiat* and thereby implicitly to admit that the whole series had been a ghastly mistake right from the start!

Despite the risible nature of Cambridge as a site for astronomical observation, a census of usage over recent years shows that I have typically observed on about 130 nights a year. Not only, therefore, is the *quantity* of observing time available on the local telescope far greater than any individual observer could ever hope to obtain on an expeditionary basis (counter-intuitive though that might appear to a visitor from an astronomically good site), but the *distribution in time* of that observing time lends itself to the systematic observation of binaries of different periods in a way that occasional access to better instrumentation at a better site would not. Within the limitations imposed by weather and instrumental power, binaries of no-matter-what period can be observed at frequencies tailored to their respective rates of velocity variation, frequencies that can be increased appropriately at the approach of a dramatic periastron passage and reduced during leisurely apastron phases.

It seems likely that in the near future, if not already, it will be possible to undertake such a programme by automated observations at a good and possibly remote (even extra-terrestrial) site without the substantial personal effort that my own situation entails. I tried hard but unavailingly to obtain funding to do exactly *that* some 20 years ago, but my experience of funding agencies is that they regard my proposals as falling into one or other of only two categories — either so old-hat as not to be worth supporting or so *avant-garde* that the idea has not risen above the adjudicators' horizon! Looking a bit further ahead, to *Gaia* and its successors, one might foresee a time when every star in the sky down to twentieth magnitude or so might be measured for radial velocity every week — or day. I do not see that it will be practicable then to write up all the orbits cherishing each object individually, as is attempted in the present series of papers, but perhaps that is part of the price — or benefit, as some may see it — of progress. Even if and when such observational over-kill is available, it seems unlikely to put people like me immediately out of business because we have got many years' start on some of the objects with long periods, and it is hard to believe that it will ever be possible to make good the lack of data that are missing from past epochs.

Introduction

Contrasting with the rather discursive prologue above, this short section offers a more focussed introduction to the content of this particular paper, the serial number of which might well be held to warrant treating it as somewhat of a celebration of longevity. With that idea in mind the writer has selected for discussion some orbits which have unusually long periods for spectroscopic binaries. Up till now, the longest period for a good orbit based solely on spectroscopy appears to be that of Polaris, about 30 years; after a preliminary effort by Moore[9] as long ago as 1929, it was determined in 1965 by Roemer[10] from more than a thousand Lick spectrograms obtained systematically (not all by her, of course!) over the interval 1896–1958. The observations included ones made in every one of the 37 successive years 1899 to 1935 and must surely represent the most systematic set of data ever obtained for a spectroscopic binary. Subsequent (if less systematic) measurements enabled a further improvement to be made to the orbit in 1996 by Kamper[11], who found the period to be 29·59 years (10 808 days). One other good orbit with a period exceeding 10 000 days has been published — that of 58 Per, 10 470 days, derived by Sanford[12] in 1953 after a false start[13] in 1931 when he proposed 6270 days. There are of course many cases in which visual binaries with tolerably well-known orbits of longer periods than those of Polaris and 58 Per have been observed spectroscopically over parts of a cycle, particularly at periastron passages, usually with a view to the derivation of values for γ and K (and thereby the masses of the components). Examples in which the present author has been concerned include HD 172865[14] (92 years), ADS 14396[15] (32 years), 24 Aqr[16] (49 years), ξ UMa[17] (60 years), HD 10297[18] (40 years), HD 26090[19] (60 years), and HD 30869[20] (95 years). In other instances, orbits with longer periods than those of Polaris and 58 Per have been derived from radial velocities alone, without the help of visual (or interferometric, *etc.*) observations, but the orbits concerned cannot be characterized as good; examples are ε Cyg[21] (55 years) and γ Cep[22] (66 years, recently corroborated by others[23]).

κ Persei (*HR 941, HD 19476*)

Kappa Persei, being appreciably brighter than the fourth magnitude, is readily seen (sometimes even in the light-polluted skies of Cambridge) near the midpoint between Mirfak and Algol (α and β Per). At the time that this paper was being prepared, the vicinity of κ Per was greatly enlivened by the presence of Comet Holmes, which was brighter than the star (though much more diffuse!) and passed within about 2° of it, remaining almost as close as that for more than a month. Photoelectric *UBV* magnitudes of κ Per were published almost simultaneously by Argue[24], Häggkvist & Oja[25], and Johnson *et al.*[26], and somewhat later by Eggen[27,28], with accordant results of $V = 3^{m}\cdot80$, $(B-V) = 0^{m}\cdot98$, $(U-B) = 0^{m}\cdot83$, save that Argue's V of $3^{m}\cdot77$ differed slightly from the others'. A V magnitude of 3·79 is given in the *Hipparcos* catalogue[29] (Volume **5**, p. 294; analogous references will be cited like **5**, 294) as having been transformed from the mean of 91 measurements in the *Hp* photometric band, with an r.m.s. scatter of $0^{m}\cdot004$ per observation. Although before the days of photoelectric photometry it may have been reasonable to have included κ Per in the *Catalogue of Suspected Variable Stars*[30] on the basis of unconfirmed evidence from visual estimates, it seems the reverse of helpful that the star should still appear in the 1982 *New Catalogue of Suspected Variable Stars*[31] just because in 1901 Guthnick[32] asserted it, on the evidence of visual observations all made in an interval of two months, to be variable with a range of nearly a magnitude. A similar comment might be made about *Simbad*'s choice (no matter under what designation one invokes the bibliography of κ Per) to call it, as its main identifier, "SV* ZI 177", after its number in an arcane 1929 listing[33] of suspected variable stars and in defiance of an IAU-inspired dictionary of nomenclature[34] that defines ZI as referring to a listing of supernovae. The note 'Var?' regarding the magnitude of κ Per in the 1964 edition[35] of the *Bright Star Catalogue* misled Cristaldi *et al.*[36] into worrying that the putative variability of that star might be responsible for discrepancies in their photometry of Algol.

The *Draper Catalogue*[37] of 1890 recognized κ Per as being of type K. In 1897 Miss Maury[38] placed it in her Group XVa, equivalent to K0 in the letter scheme of classification (the type star is α Cas), and indeed K0 is the type that appears in the *HD*[39]. There was a bit of vacillation between K0 and G8 at Mount Wilson[40] before Adams *et al.*[41] came down in favour of G8, as did Young & Harper[42] at the DAO, and that was the type generally adopted for κ Per before the advent of the MK system[43] of classification. The parallax was first published[44] in 1919 from Allegheny, where it was measured at $0''\cdot032 \pm 0''\cdot007$ ('probable error'), indicating a distance modulus of two or three magnitudes and thereby showing that, at $M_V \sim +1$ or $+2$, the star is a giant. Such a conclusion was soon reinforced when spectroscopic parallax determinations were undertaken. An initial effort in 1921 at Mount Wilson[40] indicated an absolute magnitude of $0^{m}\cdot9$, a subsequent one[41] $0^{m}\cdot4$; Young & Harper[42] made independent estimates of $2^{m}\cdot1$ and $1^{m}\cdot9$, respectively. In his first catalogue of spectroscopic parallaxes from the Norman Lockyer Observatory, Rimmer[45] found it to be $0^{m}\cdot4$; in the second one[46] he again included κ Per with an absolute magnitude of $0^{m}\cdot4$, but it is clear from the position and other properties of the listed star that on that occasion he misidentified it — the object to which he was really referring was the roman-lettered k Per (HR 918), a star whose bibliography *Simbad* actually brings up with k Per as its preferred designation. In an early MK classification Keenan[47] gave a type of G9 III, and noted that the star has particularly strong CN bands. His work was performed visually upon plates obtained with a slitless spectrograph mounted on the Yerkes 24-inch reflector, at the very low dispersion of 425 Å mm^{-1} (averaged between Hβ and Hε); a spectrum of κ Per is reproduced in his paper.

Keenan was not the first to concern himself with the matter of CN strength, which was of considerable interest because it had been recognized as representing a potential method of assessing the luminosities of late-type stars from spectra of such low dispersion that they could be obtained to very faint magnitudes. Lindblad[48], working with objective-prism spectra, obtained relationships of CN intensities to spectral types and absolute magnitudes, and derived a luminosity of $-0^{m}\cdot9$ for κ Per; Öhman[49] similarly obtained $-0^{m}\cdot6$, but later Lindblad & Stenquist[50] moderated it to $+0^{m}\cdot4$. Setterberg[51], working with slit spectra taken at the comparatively high dispersion of 22 Å mm^{-1} at the 40-inch reflector at Stockholm, developed three luminosity criteria based on the CN bands, and said that two of them, in comparison with spectroscopic parallaxes listed in the large work by Adams *et al.*[41], gave a dispersion in luminosities of only $0^{m}\cdot5$ in the cases of giant stars; then, allowing for r.m.s. errors of $0^{m}\cdot5$ in Adams' data, he considered that the 'CN luminosities' that he derived for giant stars had errors of $0^{m}\cdot0$! He agreed with Lindblad & Stenquist[50] in obtaining $+0^{m}\cdot4$ for the V-band luminosity of κ Per. Plassard[52] used 63-Å mm^{-1} spectra obtained with the 1·2-m Haute-Provence reflector and found the absolute magnitude to be $+0^{m}\cdot1$; he claimed an accuracy of $\pm0^{m}\cdot3$ for his 'CN luminosities', but his reasoning is hard to follow on that point. Gyldenkerne[53], following Strömgren's method[54] of measuring apparent magnitudes of stars through interference filters that transmitted fairly narrow but ill-defined bands of wavelength, centred in his case near $\lambda\lambda$ 4210 and 4282 Å, derived a 'cyanogen index' from the difference between those two bands but did not interpret it immediately in terms of absolute magnitude.

He was more concerned with spectral type, and from the relationship between the precision of his measurements and the average gradient of his index with spectral type he concluded that he could determine types very much more accurately than even the most expert classifiers of spectra; like his mentor Strömgren[54], he discounted the existence of any appreciable 'cosmic scatter', attributing all discrepancies almost wholly to errors of classification. Griffin & Redman[55] overcame the problems caused by the uncertainty and irreproducibility of filter band-passes by using windows in the focal plane of a dispersing spectrometer to isolate accurately defined wavelength bands; that enabled them also to observe several bands simultaneously. They too hoped to determine astrophysically significant parameters from objective measurements, but recognized in the light of their observational results that they had been too optimistic: instead of deriving absolute magnitudes, they formed 'CN anomalies' which represented the discrepancies between the measured CN intensities for the various stars in comparison with the values to be expected from the *mean* relationship of CN with type and luminosity. Kappa Per has a positive CN anomaly but by no means an exceptional one. CN anomalies were also discussed by Yoss[56]. Hansen & Kjærgaard[57] interpreted measurements made by Dickow *et al.*[58] in the Strömgren fashion through narrow-band interference filters as indicating an absolute magnitude of $+1^{m}\cdot 3$ for κ Per.

Simbad records that κ Per features, with a type of K0 III, in the 1953 paper[59] by Johnson & Morgan in which the latter re-defined the standards of the MK classification system, but the writer cannot confirm that. That type was, however, put forward with the rider 'wk-l' [weak-line] in a paper by Miss Roman[60] at about that time. A sufficiently careful reading of the paper shows that the 'wk-l' suffix is actually intended to mean that the Ca I line λ 4226 Å and the *G* band of CH are both *stronger* than in the average star of the same type, thereby giving conflicting indications of type because whereas the calcium line strengthens towards later types the *G* band does so towards earlier ones. Mustel & Koumaigorodskaya[61] confirmed the division proposed by Miss Roman as a fact, at least in a small sample, by comparing the strengths of the features concerned in the 'wk-l' stars κ Per and ε Tau with those in the 'st-l' objects ε Cyg and μ Peg.

The classification K0 III was affirmed by no less a partnership than that of Keenan & Morgan[62] in 1971, and although in the 1980s Keenan promulgated several successive listings[63–67] of MK standards for late-type stars, making small changes and/or refinements to some of the classifications, κ Per remained fixed at type K0 III throughout. In a final re-classification[68], however, which was work in progress at the time of Keenan's demise (but since he was working through the sky in order of right ascension κ Per was encountered quite early in the process), our star appears as G9·5 IIIb. Keenan had naturally been extremely interested in the luminosities that stemmed from the *Hipparcos* parallaxes; he actually found[69] that they largely vindicated the small distinctions that he had already made in subdividing the major luminosity classes, but he did make some additional minor changes in the light of the new parallaxes. At one time, in collaboration[70] with Egret and Heck, he had put his *imprimatur* to the value $M_V = 0^{m}\cdot 0$ for κ Per; the (presumably definitive) *Hipparcos* value is $1^{m}\cdot 12 \pm 0^{m}\cdot 05$.

In the course of investigating the kinematics of strong-CN stars, Janes & McClure[71] derived a 'photometric type' of G9 III for κ Per. The only other independent classification that has come to the writer's attention is Abt's[72] G8 III. There have been a number of spectroscopic and spectrophotometric investigations that have produced estimates of the metallic abundances and/or absolute magnitude for κ Per. As far as abundances are concerned, one may summarize with cavalier brevity the work of Williams[73–75], Hansen & Kjærgaard[57], Rego *et al.*[76], Gottlieb & Bell[77], Gratton *et al.*[78], McWilliam[79], Flynn & Mermilliod[80], and Xu[81] by saying that the consensus is that κ Per is if anything slightly metal-rich with respect to the Sun, but some early enthusiasm in that direction has tended to give place to results that are at most marginally super-solar. On the absolute-magnitude front, now that the true value is known quite accurately from the parallax, as noted at the end of the last paragraph, it may be seen that in general the efforts of spectroscopists have been on the right track; in fact the estimate already noted of Egret, Keenan & Heck[70], surprisingly, seems to be the furthest of all from the truth. Olin Wilson's *K*-line result[82] of $+1^{m}\cdot 7$ is further 'off' than most of the absolute magnitudes determined by his technique; κ Per did not feature in the original *K*-line paper[83] of Wilson & Vainu Bappu.

An interesting point is that Eggen[28,84] repeatedly assigned κ Per to the 'Hyades group' while tabulating its principal kinematic properties again and again in his characteristic fashion; in all too many other instances there were[85] capricious changes in his 'group' assignments if not indeed in the actual data attributed to the stars concerned. Although there were some small variations in the luminosity that he gave as corresponding to membership in the group (M_V $+1^{m}\cdot 14$ to $+0^{m}\cdot 9$), that luminosity was always close to the real value, so it appears possible that κ Per really *is* associated in some way with the Hyades. Boyarchuk *et al.*[86] went so far as to include κ Per in a section of their Table 1 headed 'Hyades giants' which includes just four stars, the other three of which actually *are* Hyades giants.

Radial velocities and orbit of κ Persei

Radial-velocity measurement of κ Per has had a long history, starting with observations made with the (original) *Mills Spectrograph* on the Lick 36-inch refractor in 1899. Over several years the star's velocity appeared to undergo a slight drift, which was considered as a real change and prompted announcements to that effect in 1910 by Campbell[87] and by Campbell & Albrecht[88]. The authors of the announcements, eschewing undue modesty, explicitly credited the discovery of the velocity variations to themselves, and said that they made it from the fifth plate. That was one taken in 1907, which was recorded in the Campbell & Albrecht announcement[88] as having been exposed by Moore and measured by Miss Hobe; the assignment of credit[87,88] accordingly tends to conjure up in the mind's eye a rather feudal picture of one set of people doing the actual work and another, superior, set looking over the results that were fed to them and making their great discoveries! Actually the latter set must not have been too sure of their discovery in the case of present interest, because although Lick announcements of velocity variability were being published quite frequently, in this case more than two years elapsed between discovery and announcement, presumably while the discoverers waited to see results from spectra taken in the two succeeding years, before they finally published in 1910 the discovery they said they made in 1907. Close inspection of the new results presented below will demonstrate that the reinforcement for which Campbell & Albrecht waited and which appeared to have been furnished by the measurements of 1908 and 1909 was actually illusory; we can see now that it really stemmed from unusually large errors of fortuitously favourable sign (see the first two of the three highest points, plotted as open circles, in Fig. 1) rather than from any further significant change in the actual velocity of the star. However, the final compendium[6] of Lick velocities, published in 1928, includes ten measures of κ Per, which taken together are sufficiently discordant fully to warrant the claim of variability, even though that claim may scarcely have been justified at the time when it was initially made. In an interim summary of the Lick radial-velocity programme as far as 1913, Campbell[89] estimated the γ-velocity of κ Per as $+30$ km s^{-1}.

Since the Lick epoch, however, little further interest has been shown in the radial velocity of κ Per, and as time has gone on the early assertion of variability has in some quarters been forgotten, doubted, or actually rejected. Although the *Bright Star Catalogue*[90] continues to note κ Per as a spectroscopic binary, the *Radial Velocity Catalogue*[91] does not, simply recording a mean velocity and moreover attributing to it quality *a*, the highest quality. A large survey[92] of evolved stars, which routinely gives notes on the binary nature and/or the existence of orbits for the listed stars, has no such annotation against κ Per. As recently as 2002, κ Per was included in a catalogue[93] of 'calibrator stars for long-baseline stellar interferometry', among whose selection criteria were radial-velocity stability and lack of evidence of multiplicity. In 2007, authors[94] who had made a 450-day observing campaign of high-precision radial velocities noted κ Per only as a *possible* spectroscopic binary, with a period which exceeds 450 days — which we show below that it *does*, by a large margin.

Although the Lick Observatory was the first to *observe* the radial velocity of κ Per, it was not the first to publish it: that distinction fell to the Bonn Observatory, where Küstner[95] used the 12-inch *Repsold* refractor with a spectrograph giving 15 Å mm^{-1} at Hγ to obtain three plates of the star. Harper[96] listed two measurements, made on the same night as one another with the Victoria (DAO) 72-inch reflector. One measurement was made by Tremblot[97] with the Haute-Provence (OHP) 1·2-m telescope. Four were reported by Jones & Woolley[98] to have been made during a visit by the latter to Mount Wilson, where he used the *d* camera (21 Å mm^{-1}) of the *X Spectrograph*; the result was given only as a mean. Four more were obtained by Beavers & Eitter[99] with their photoelectric radial-velocity spectrometer[100] at the Fick Observatory, Ames, Iowa. One, made with a portable *Coravel*-type instrument used with the 70-cm telescope on the Moscow University campus, was published by Rastorgouev *et al.*[101]. Fehrenbach *et al.*[102] made six measurements by their objective-prism method, whose innate precision is not satisfactory for present purposes. De Medeiros & Mayor[92] used the OHP *Coravel* to obtain two observations, which are included in Table I with others made by the present writer with the same instrument. Most recently, Eaton & Williamson[94] have reported a mean value of no fewer than 57 measurements of κ Per, made over such a short interval that they were unsure whether or not they saw the object as a spectroscopic binary.

The writer's observations of κ Per began in 1969, after he had selected it as one of 18 stars shown to be spectroscopic binaries in the great Lick survey[6] but still at that time lacking orbits although otherwise suited to observation with the (then quite new) photoelectric radial-velocity instrument[103] at Cambridge. ('Suited' here refers mainly to criteria of spectral type and declination: each of those criteria mandates rejection of approximately half of any random sample of objects.) It was mentioned in a recent paper[104] that κ Per was one of only three stars still without a published orbit out of the original 18. That has happened (as in the other cases) not through any lack of diligence on the part of the observer, who has in fact followed κ Per tolerably systematically, making measurements in every one of 40 consecutive years, but simply because of the leisurely nature of the velocity variations. Inevitably there have been changes in instrumentation during that long interval, and the early observations now appear a trifle ragged in comparison with recent ones, which are themselves well behind the present state of the art. If only current best-practice measurements were acceptable, however, it would never be possible to document orbits whose periods were other than short.

Three instruments have successively been the mainstay of the observing campaigns on κ Per and on the other stars (to which this paragraph equally applies) treated in the present paper. First was the original radial-velocity spectrometer[103], which was brought into operation in 1966. (It was then an instrument of unique power, and was sent, at the Museum's request, to the Science Museum when it was decommissioned in 1991.) Forty-four measurements of κ Per were made with it. Overlapping the epoch of that instrument was the quasi-routine use (23 measurements) of the OHP *Coravel*[105], through the courtesy of Dr. M. Mayor and the Observatoire de Genève, between 1986 and 1998; and latterly (1999–2008) the programme has depended upon the *Coravel* instrument that, after much trouble and delay, replaced the original spectrometer at Cambridge (29 measurements). Those three principal sources have been supplemented in the case of κ Per by six observations made with the spectrometer[106] at the coudé focus of the 48-inch reflector at the DAO and one with the spectrometer[107] at the Palomar 200-inch coudé. All of the data, including the published measurements insofar as they are given individually with dates, are set out in Table I, with the sole exception of the objective-prism observations. The two measures made on the same night by Harper[96] have been averaged. The velocities published by others have all been increased by 0·8 km s^{-1} and also, where appropriate, by the offset listed in Table 3 of the *Radial Velocity Catalogue*[91] for the observatory concerned and for stars of type G (as κ Per was considered to be at the relevant times).

TABLE I

Radial-velocity observations of κ Persei

Except as noted, the sources of the observations are as follows:
1969–1986 — original Cambridge spectrometer (weight 0·05 1969–1976, 0·15 1977–1986);
1987–1998 — Haute-Provence Coravel (wt. 1); 1999–2008 — Cambridge Coravel (wt. 1)

Date (UT)			*MJD*	*Velocity km s^{-1}*	*Phase*	*(O − C) km s^{-1}*
1899	Nov.	2·33*	14960·33	+27·9	$\bar{2}$·239	−0·3
	Dec.	4·18*	992·18	28·6	·242	+0·4
1900	Oct.	17·53*	15309·53	29·4	$\bar{2}$·273	+0·7
1905	Jan.	14·83†	16859·83	31·0	$\bar{2}$·420	+0·6
	Aug.	17·51*	17074·51	30·2	·440	−0·3
	Dec.	26·90†	205·90	30·9	·453	+0·3
1907	Jan.	13·87†	17588·87	31·7	$\bar{2}$·489	+0·9
	Sept.	21·42*	839·42	30·6	·513	−0·3
1908	Aug.	7·49*	18160·49	31·8	$\bar{2}$·543	+0·8
1909	Dec.	28·12*R	18668·12	32·8	$\bar{2}$·592	+1·7
1910	Aug.	13·50*R	18896·50	32·6	$\bar{2}$·613	+1·5
1919	Aug.	22·52*	22192·52	27·6	$\bar{2}$·926	+1·3
1921	Nov.	2·37‡	22995·37	+24·0	$\bar{1}$·003	+0·2

TABLE I (*continued*)

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O − C) km s⁻¹
1922	Jan.	27·17*	23081·17	+24·8	$\bar{1}$·011	+1·1
1937	Aug.	5·12§	28750·12	29·6	$\bar{1}$·549	−1·4
1969	Sept.	30·05	40494·05	30·2	0·665	−0·8
1970	July	31·14	40798·14	31·4	0·694	+0·5
1971	Feb.	3·82	40985·82	29·9	0·711	−0·9
1972	Nov.	22·96	41643·96	29·7	0·774	−0·5
1973	Feb.	21·78	41734·78	31·6	0·783	+1·5
	Aug.	16·15	910·15	31·3	·799	+1·5
	Sept.	27·11	952·11	28·9	·803	−0·9
1974	Jan.	19·86	42066·86	28·1	0·814	−1·5
	Aug.	7·13	266·13	28·7	·833	−0·5
	Oct.	7·10	327·10	27·3	·839	−1·8
	Nov.	12·03	363·03	31·2	·842	+2·2
1975	Feb.	27·82	42470·82	27·5	0·852	−1·2
	June	26·10	589·10	28·1	·864	−0·3
	Oct.	2·04	687·04	28·4	·873	+0·2
1976	Jan.	3·86	42780·86	26·3	0·882	−1·6
	Aug.	1·13	991·13	26·0	·902	−1·2
1977	Jan.	2·12¶	43145·12	29·2	0·916	+2·5
		29·83	172·83	26·3	·919	−0·3
	Sept.	3·16	389·16	24·7	·940	−1·1
		26·41¶	412·41	25·8	·942	+0·1
	Nov.	24·98	471·98	24·9	·948	−0·6
	Dec.	30·19¶	507·19	26·2	·951	+0·9
1978	Jan.	22·12¶	43530·12	23·2	0·953	−2·0
		22·89	530·89	25·8	·953	+0·6
	Mar.	27·85	594·85	24·5	·959	−0·5
	Aug.	22·11	742·11	25·0	·973	+0·5
	Oct.	7·06	788·06	23·8	·978	−0·6
1979	Jan.	9·86	43882·86	22·2	0·987	−1·9
	Mar.	4·77	936·77	24·1	·992	+0·1
	Sept.	16·15	44132·15	23·8	1·010	+0·1
	Nov.	24·99	201·99	23·6	·017	0·0
1980	Sept.	3·12	44485·12	22·1	1·044	−1·6
	Dec.	8·92	581·92	23·6	·053	−0·2
1981	Sept.	14·08	44861·08	24·1	1·079	−0·2
1982	Jan.	18·82	44987·82	24·5	1·092	−0·1
	Mar.	5·82	45033·82	24·3	·096	−0·4
	Sept.	24·13	236·13	25·1	·115	−0·2
	Nov.	26·16P	299·16	25·8	·121	+0·4
1983	Mar.	15·81	45408·81	27·0:	1·131	+1·3
	Sept.	20·14	597·14	25·6	·149	−0·6
	Oct.	28·37D	635·37	+26·4	·153	+0·1

TABLE I (*continued*)

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
1984	Dec.	11·89	46045·89	+26·5	1·192	−0·7
1985	Feb.	18·13[D]	46114·13	27·0	1·198	−0·4
	Oct.	1·04	339·04	27·8	·220	0·0
1986	Jan.	23·82	46453·82	27·7	1·231	−0·3
	Mar.	6·91	495·91	28·5	·235	+0·4
	Aug.	29·12[H]	671·12	28·7	·251	+0·3
1987	Feb.	27·81	46853·81	28·7	1·269	0·0
	Apr.	2·79[‖]	887·79	26·9	·272	−1·8
	Oct.	18·00	47086·00	28·9	·291	−0·1
1988	Jan.	3·87	47163·87	28·0	1·298	−1·1
	Mar.	8·86[M]	228·86	29·0	·304	−0·2
		15·78	235·78	29·4	·305	+0·2
	Oct.	31·96	465·96	29·5	·327	0·0
	Nov.	29·07[M]	494·07	29·5	·330	0·0
	Dec.	12·92	507·92	29·0	·331	−0·5
1989	Mar.	27·78	47612·78	29·5	1·341	−0·1
	Sept.	7·17	776·17	30·4	·356	+0·6
1990	Jan.	30·90	47921·90	30·3	1·370	+0·3
	Oct.	13·09	48177·09	29·9	·394	−0·3
	Dec.	30·93	255·93	30·2	·402	0·0
1991	Feb.	3·84	48290·84	30·4	1·405	+0·1
	Oct.	30·00	559·00	30·4	·431	−0·1
	Dec.	17·88	607·88	30·5	·435	0·0
1992	Mar.	3·14[D]	48684·14	30·8	1·443	+0·3
	June	26·11	799·11	30·3	·454	−0·3
	Dec.	21·97	977·97	30·7	·471	0·0
1993	Feb.	15·79	49033·79	30·8	1·476	+0·1
	Dec.	26·90	347·90	31·3	·506	+0·4
1994	Aug.	7·12	49571·12	31·0	1·527	+0·1
	Dec.	10·89	696·89	30·7	·539	−0·3
1995	June	7·13	49875·13	31·0	1·556	0·0
	Dec.	31·85	50082·85	31·0	·575	−0·1
1996	Mar.	30·79	50172·79	31·2	1·584	+0·1
	Nov.	20·90	407·90	31·1	·606	0·0
1997	Mar.	6·79	50513·79	30·9	1·616	−0·2
	July	23·12	652·12	31·0	·630	−0·1
	Sept.	11·01	702·01	30·6	·634	−0·5
	Dec.	21·87	803·87	30·9	·644	−0·2
1998	July	15·13	51009·13	31·0	1·663	0·0
1999	Apr.	17·18[D]	51285·18	30·7	1·690	−0·2
	July	8·49[D]	367·49	31·0	·697	+0·1
	Oct.	31·33[D]	482·33	31·2	·708	+0·4
	Dec.	28·91	540·91	+30·8	·714	0·0

TABLE I (*concluded*)

Date (UT)		MJD	Velocity km s⁻¹	Phase	(O − C) km s⁻¹
2000	Jan. 18·86	51561·86	+31·2	1·716	+0·4
	Mar. 25·83	628·83	30·5	·722	−0·2
	Aug. 29·12	785·12	30·5	·737	−0·1
	Dec. 2·04	880·04	30·5	·746	0·0
2001	Mar. 12·87	51980·87	30·7	1·756	+0·3
	Sept. 30·10	52182·10	30·1	·775	−0·1
	Dec. 21·99	264·99	30·0	·783	−0·1
2002	Mar. 7·84	52340·84	30·2	1·790	+0·2
	Sept. 2·16	519·16	29·7	·807	0·0
2003	Jan. 7·08	52646·08	29·5	1·819	0·0
	Mar. 15·86	713·86	29·4	·825	0·0
	Sept. 14·18	896·18	29·2	·843	+0·2
	Dec. 28·93	53001·93	28·7	·853	0·0
2004	Mar. 30·80	53094·80	28·6	1·862	+0·1
	Sept. 6·16	254·16	28·1	·877	0·0
	Dec. 17·91	356·91	27·7	·886	−0·1
2005	Mar. 18·81	53447·81	27·5	1·895	0·0
	Sept. 7·17	620·17	26·9	·911	0·0
	Dec. 26·93	730·93	26·3	·922	−0·2
2006	Mar. 3·80	53797·80	26·1	1·928	−0·1
	Sept. 11·17	989·17	25·4	·946	−0·1
	Dec. 11·98	54080·98	25·2	·955	0·0
2007	Mar. 3·85	54162·85	24·9	1·963	0·0
	Sept. 15·16	358·16	24·5	·982	+0·2
	Dec. 7·97	441·97	24·2	·989	+0·1
2008	Jan. 5·95	54470·95	+24·2	1·992	+0·2

* Lick[6] observation, normal weight 0·15.
† Bonn[95] observation, weight 0.
‡ DAO[96] observation, weight 0.
§ OHP (Tremblot)[97] observation, weight 0.
¶ Ames[99] observation, weight 0.
‖ Moscow[101] observation, weight 0.
P Observed with Palomar 200-inch telescope (wt. 0·5).
D Observed with DAO 48-inch telescope (wt. 0·5).
H Observed with Haute-Provence *Coravel* (wt. 1).
M Observation reported through the CDS[92].
R Rejected observation.

Dealing with an *ensemble* of velocities in which no one source covers the whole range of phases warrants particular circumspection regarding zero-points: alterations of relative zero-points may leave the residuals of all sources agreeably close to zero at the cost of warping the orbital velocity-curve to suit. In the present instance we therefore prefer to be guided by experience rather than (as is quite usual in this series of papers) by the mean residual for each individual source of data. Accordingly, the 'original Cambridge' data (which come from the same instrument as was used in the first place to set up the Cambridge zero-point[108]) are left unchanged, the OHP measurements receive the routine adjustment of

$+0\cdot8$ km s^{-1}, and the new Cambridge measurements have been adjusted downwards by $0\cdot2$ km s^{-1} in accordance with recent experience[109]. A solution of the orbit was first made on the basis of the writer's own observations alone. The measurements from the two *Coravel*s were attributed unit weight, the DAO and Palomar ones $^1/_2$, while the 'original Cambridge' data were mostly weighted $0\cdot15$. In the early years, however, the velocities were particularly ragged, mainly because the star was brighter than the electronics then in use in the spectrometer could really cope with, and they have been weighted only $0\cdot05$. The part of the velocity curve that would otherwise depend upon them is well covered by recent measurements with the Cambridge *Coravel*, but the early measures, though of low weight, do serve to give what seems like quite a definite result for the period, at 10 668 ± 56 days.

Unfortunately the Lick observations create some unease when plotted with that period: the three that are near the steepest parts of the velocity curve and have the most leverage on the period would all like the period to be appreciably shorter. A preliminary orbital solution that was illustrated in the proceedings of a conference[110] as long ago as 1992, when the writer's observations were well short of covering a complete cycle, necessarily relied wholly upon the Lick measurements for the determination of the period, which was given as 10 400 days with a standard error of only two months — formally incompatible with the value obtained here from the new observations alone. The period obtained now if the Lick velocities are given a very heavy and inappropriate weight is about 10 460 days. Whatever course is adopted is bound to appear as a somewhat unsatisfactory compromise, but since according to past experience the Lick radial velocities[6] have proved to have much the same standard errors as those obtained with the original spectrometer[103] at Cambridge, they have been attributed the same weighting ($0\cdot15$) here. The two slightly 'wild' ones near the top edge of the figure, the first of which is actually one of those that meretriciously 'confirmed' the binary nature of κ Per for Campbell & Albrecht[88], have been zero-weighted, but because they are at a phase where the velocity curve is practically level their rejection scarcely affects the resulting period, which is 10 528 days. The formal standard deviation of only 34 days is surely an under-estimate of its real uncertainty, but the true value that will be known in 500 years' time is not expected to differ from the one found here by more than about 100 days, which is, after all, less than 1% of the period itself. None of the published measurements other than those from Lick has been utilized in the final solution, which is plotted in Fig. 1 and whose elements are:

$P = 10528 \pm 34$ days	$(T)_2 = \text{MJD } 54553 \pm 62$
$\gamma = +28\cdot54 \pm 0\cdot04$ km s^{-1}	$a_1 \sin i = 509 \pm 10$ Gm
$K = 3\cdot74 \pm 0\cdot07$ km s^{-1}	$f(m) = 0\cdot0476 \pm 0\cdot0027\ M_\odot$
$e = 0\cdot340 \pm 0\cdot012$	
$\omega = 159\cdot5 \pm 2\cdot2$ degrees	R.m.s. residual (wt. 1) $= 0\cdot23$ km s^{-1}

The period is seen to be marginally longer than that of 58 Per, one of the two stars mentioned in the *Introduction* above as having a good published orbit with a period above 10 000 days, but not quite as long as that of Polaris.

The mass function requires the secondary to have a minimum mass ranging from $0\cdot6$ to $1\cdot0\ M_\odot$ for primary masses ranging from $1\cdot6$ to $3\cdot6\ M_\odot$. A white-dwarf secondary would be unlikely to have escaped detection at κ Per's distance of only about 34 pc, so we can with reasonable confidence expect the companion

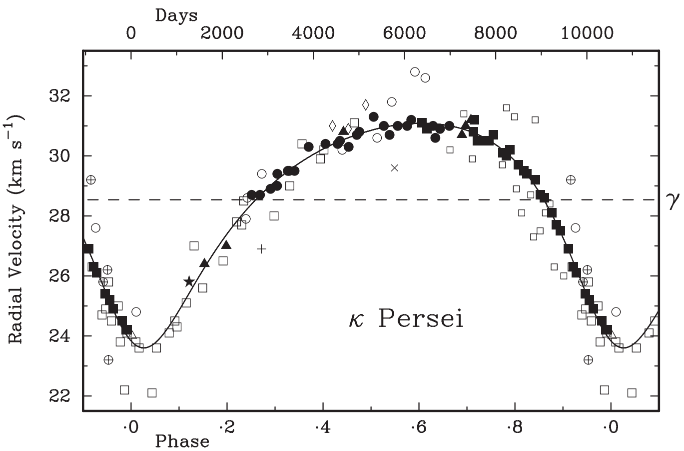

FIG. 1

The observed radial velocities of κ Persei plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The orbit largely depends on measurements made with the OHP and Cambridge *Coravel*s, represented by filled circles and squares, respectively, and the original Cambridge radial-velocity spectrometer (open squares; mostly weighted 0·15 in the solution of the orbit). Other data used in the solution are from the DAO and Palomar spectrometers (filled triangles and star symbol, both weight 1/2) and the early Lick photographic observations[6] (open circles, weight 0·15; the two highest ones, near the top edge of the plot, are rejected). Certain observations that were attributed reduced weight are plotted with smaller symbols; they include, in particular, all the Cambridge observations made before 1977. Other symbols plot published measurements that were not utilized in the solution — Bonn[95] (open diamonds), DAO[96] (open triangle), OHP[97] (cross), Ames[99] (circles with plusses in them), and Moscow[101] (plus).

star to be a main-sequence object. It would adulterate the spectrum if it contributed a significant fraction of the brightness of the system ($M_V \sim 1^{\mathrm{m}}\cdot 1$), so it could be expected to lie in the range of the main sequence between about M_V $3^{\mathrm{m}}\cdot 5$ and a mass of 0·6 $M_{\odot}$ — roughly mid-F to late-K. With a mass probably half that of the giant, or less, it must be twice (or more) as far from the centre of gravity of the system as the primary, so the mean separation of the stars should be at least three times the $a_1 \sin i$ value given above, making it more than 10 AU. Such a separation subtends an angle of about a third of a second of arc at the distance of κ Per. The Δm is at maximum about six magnitudes and could easily be a lot less, so although we do not know the orbital inclination it seems entirely possible that the system could be resolved on the sky, perhaps at the first try with adaptive optics on a large telescope.

The star also features in double-star catalogues such as the *ADS*[111], in which (**1**, 206) it is no. 2368. It is, however, not a genuine double star: the 'companion' is merely a faint star in the background, first noted in 1914 by Espin, whose paper[112] (one of a number of mutually analogous ones) is a numbered list of his latest 'discoveries'. Curiously, it omits to assign any number to κ Per — our star is otherwise listed just like all the other objects, save for being identified by its constellation name instead of a *BD* designation; it comes between numbers 1309 and 1310. In the sky the 'companion' is being steadily left behind by the quarter-second annual proper motion of the primary star, and it is understandable that few measurements have been made of the pair.

β Leonis Minoris (*HR 4100, HD 90537*)

β LMi is a fourth-magnitude star with the spectrum of a late-type giant. It is not only the brightest star in the inconspicuous constellation Leo Minor, which occupies part of the area between Leo and Ursa Major, but also has the distinction of being the only star with a Greek-letter designation in the whole constellation. That obviously begs the question as to what happened to Alpha; it seems relevant to mention that just outside the western border of Leo Minor, in the equally inconspicuous constellation Lynx, lies the third-magnitude α Lyncis, which is the only star possessing a Greek-letter designation in *its* constellation.

Unlike the other stars discussed in this paper, β LMi is well known as a visual binary. Its duplicity was discovered in 1904 with the Lick 36-inch refractor by Hussey[113], who saw it as a very unequal pair (magnitudes 4 and 6·5) with a separation of only 0″·45 and assigned to it his 'discovery number' Hu 879. Naturally the system has been watched ever since then by visual observers, although obviously it is a difficult object visually, and in recent years speckle and other forms of optical interferometry have been brought to bear on it. For a considerable time in each orbital cycle it is not to be seen double in any telescope; around the time of the first periastron passage after discovery, no less an observer than van Biesbroeck[114] failed to resolve it on 14 occasions during the interval 1918–1925. By the time that Aitken compiled his great double-star catalogue[111], where (**1**, 653) β LMi is no. 7780, enough of the orbit had been seen for him to note that "the revolution period of the binary may be comparatively short" — by the standards of visual binaries, of course.

In 1950 Baize[115] was able to recognize the form of the orbit, with a period of about 38 years and high eccentricity (0·61) and inclination (82°); he[116] gave revised elements in 1976. In both versions, periastron was incorrectly placed before the nodal passage — an understandable error, since that whole part of the orbit had been unobservable and the complete range of the observed position angles amounted only to 40°. The need for a major revision of the time of periastron was recognized by Heintz[117,118], perhaps partly in the light of the efforts made by Miss Underhill at the DAO to observe the periastron passage around 1960 spectroscopically. She[119] obtained no fewer than 80 spectrograms, mostly at 11 Å mm^{-1}, with the 72-inch reflector, but she presented the results as 15 mean values over the interval 1952–1962. They show only very sketchily the nodal passage; their author actually commented on the bad scatter and was inclined to attribute it to a 'secondary variation' but said that her results were insufficient to define it. In an abstract[120] of the paper as given at a meeting in Victoria of the Astronomical Society of the Pacific, however, she said much more definitely that there was "evidence of a pronounced secondary variation that has a period of 1850 days and a semi-amplitude of 3·15 km/sec." Her idea received some support in a closely ensuing annual report from the Allegheny Observatory[121], which reads, "A preliminary analysis of the visual binary measurements of β LMi by van Biesbroeck and the parallax residuals of Allegheny confirms the 5-yr period discovered spectroscopically by Miss Underhill. The 5-yr period along with the 38-yr period creates the interesting possibility of orbital perturbations within the system. Observations are being continued." Despite the difficulties caused by the supposed 'secondary variation', by using an unpublished visual orbit by van Biesbroeck Miss Underhill[119] derived a radial-velocity amplitude $K_1 = 3·18$ km s^{-1} in the β LMi orbit, a value which we shall find below to be less than half the true one.

Heintz[117], in notes associated with the publication in 1981 of three radial-velocity measurements of his own, commented that "the 5-yr variation suspected by Underhill (1963) is quite weakly documented, and considerations of stability make it unlikely." In the following year he[118] gave a new orbital solution, shifting the periastron passage from well before the node (Baize's ωs were $327°\cdot0$ and $344°\cdot6$ in his respective orbits[115,116]; coincidence with the node would mean $\omega = 0°$) to $32°\cdot5$, and re-computing the spectroscopic elements as $\gamma = +5\cdot61$ km s^{-1}, $K_1 = 3\cdot82$ km s^{-1}, $\omega = 32°\cdot5$. The value that he found for the 'fractional mass' (the visual observer's way of expressing the mass ratio), in conjunction with the very small K_1 that he obtained, led to an unacceptably small mass for the secondary star.

Further, relatively minor, revisions to what was in very large part a 'visual' orbit (the term is to be understood to embrace interferometry, *etc.*, as well as measures made literally by eye) have been published by Söderhjelm[122] and Mason & Hartkopf[123], and astrometry has recently been added to the considerations by Gontcharov & Kiyaeva[124,125]. We shall return to discuss those results in the light of the spectroscopic ones below.

The apparent magnitude of the system has been measured photoelectrically by Argue[24], Häggkvist & Oja[25], Johnson *et al.*[26], and Eggen[27,126], with accordant results close to $V = 4^{m}\cdot21$, $(B-V) = 0^{m}\cdot91$, $(U-B) = 0^{m}\cdot64$. Inasmuch as the object is a double star, there is a need to apportion the integrated luminosity between the components — a notoriously difficult problem with close binaries. The visual observers have mostly estimated Δm to be near two magnitudes, although the *ADS*[111], perhaps deferring to Hussey[113], gives it as $2^{m}\cdot5$. Actual measurements have resulted in smaller values; it was well appreciated by Baize[127] that there is an increasing tendency to over-estimate Δm as binary separations decrease. Thus Kuiper[128], who produced multiple images of supposedly known Δms by means of wire gratings placed in front of the objective of the Lick 12-inch refractor, obtained $\Delta m = 1^{m}\cdot18$ for β LMi. Muller[129], however, obtained a result nearer to the visual one, $1^{m}\cdot71$, with his double-image micrometer[130]. Much more recently, Horch *et al.*[131] measured Δm by means of speckle interferometry with a CCD detector on the *WIYN* 3·5-m telescope at Kitt Peak. They were able to do that through different filters, and obtained for β LMi values of $1^{m}\cdot47$ at λ 5510 Å, $1^{m}\cdot78$ at 6480 Å, and $1^{m}\cdot77$ at 7010 Å. Probably the most direct and least easily criticized measurement of the magnitude difference was made by *Hipparcos*[29] (7, 1031), as $\Delta Hp = 1^{m}\cdot42 \pm 0^{m}\cdot07$. The *Hp* passband is not very different from *V*, and for present purposes the two bands might be accepted as equivalent to one another. A less carefree attitude would require us to notice (*Hipparcos*[29] **1**, 59) that blue stars are measured slightly brighter than red ones in *Hp* in comparison with *V*, which in the case of β LMi would warrant our adopting a ΔV slightly larger than ΔHp, but since such an increase would be less than the standard error of the initial value, and also because such tampering would bring with it a possible appearance of circularity of argument when we come to consider the natures of the two stars, we will be content to regard the Δm as $1^{m}\cdot42$ in *V*, the same as in *Hp*.

The spectral type of β LMi was given as "I?" in the *Draper Catalogue*[37]; a remark associated with the K0 that is shown in the *Henry Draper Catalogue*[132] indicates that that type was taken from an earlier classification[133] of bright stars, although the type shown there[133] is actually just K. The types initially given at Mount Wilson[40] in 1921 were G6 ('estimated') and G7 ('measured'); the first method was the normal one of comparing the spectrograms visually with those of standard stars, whereas the second involved quantitative measurements of the strengths of hydrogen lines with respect to nearby metallic ones. The absolute magnitude was estimated at $+1^m \cdot 2$. Shortly afterwards, Adams & Joy[134] at Mount Wilson again gave the type as G6; in addition, classifications and spectroscopic luminosity estimates were published from the Norman Lockyer Observatory by Rimmer[45], who obtained results of K0, $+0^m \cdot 8$, and from the DAO by Young & Harper[42], who gave the type as G6 and made independent but identical estimates of $+1^m \cdot 8$ for the absolute magnitude. A later paper from Mount Wilson[41] listed G8, $+0^m \cdot 6$. The first MK classification appears to be Miss Roman's[60] G8 III–IV, which has been widely quoted, often without the 'st-l' [strong-line] suffix which she attached to it and which, as described in the κ Per section above, actually implies *weak* lines. Keenan & Morgan[62] considered β LMi to be of type G9 IIIab and marked it as being one of "the normal stars that we judge to be the best-determined standards of temperature type and luminosity class". It does seem a trifle perverse to adopt, as a particularly creditable standard, an object that is known to be double and could be expected (from the disparity in magnitudes) to involve dissimilar spectra; the spectrum ought to be in some degree composite. Be that as it may, the G9 IIIab type persisted through all of Keenan's re-classifications[63–67] until the very last one[68], in which it appears as plain G9 III and is still flagged as one of the best-determined spectral standards. There is also a classification by Abt[72], of G8 III.

Indices derived from photoelectric photometry in fairly narrow (interference-filter) bands enabled Hansen & Kjærgaard[57] to propose an absolute magnitude of $+1^m \cdot 8$ and a metallicity [Fe/H] of $-0 \cdot 06$; they obtained a value of $0^m \cdot 035$ for a duplicity-sensitive parameter, 'res(k)' — almost up to the $0^m \cdot 04$ that they took as definitely indicating either compositeness or else considerable reddening. In an investigation somewhat analogous to that of Hansen & Kjærgaard, but using a different system, Hartkopf & Yoss[135] found a 'DDO spectral type' of G8 III–IV and an M_V of $+1^m \cdot 5$. Williams[73,75] (and in one case[76] with collaborators) used Cambridge narrow-band indices with model atmospheres rather than empirical correlations, and repeatedly gave a very high metal abundance, [Fe/H] $= +0 \cdot 50$, for β LMi. Gustafsson *et al.*[136], who did not themselves observe β LMi but did have a substantial overlap with Williams' observing list, noted with studied understatement that their abundance determinations "do not agree too well", and considered that part of the trouble was with Williams' temperature calibration — they implied that the model atmospheres were too hot. Since Williams obtained the temperatures from broad-band magnitudes, the admixture in the photometry of a hotter secondary star would certainly falsify the temperature in the case of interest.

Eaton[137] published a spectrophotometric tracing of β LMi in the vicinity of Hα. It is fortuitously juxtaposed with one of the K0 III Hyades giant ε Tau. Comparing the two tracings, the eye of faith may see that the region immediately beyond the core of the line in β LMi is slightly depressed relatively to that in ε Tau — a possible sign of superposition on the late-type spectrum of that of a secondary star of earlier type.

There is one result from the early Cambridge efforts with the narrow-band spectrometer — the most ambitious effort of all in terms of the narrowness of the band, though not the most successful — which is worth relating here. The programme concerned[138] was intended to measure the strength of the λ 6305-Å resonance line of Sc I, which at least in giant stars increases in strength very rapidly through the K types. The measurements showed a very close relationship of the scandium intensity to $(B-V)$, and indicated that the line is of negligible strength for $(B-V) < 0^m\cdot85$, so β LMi, with $(B-V) = 0^m\cdot91$, should have very weak Sc I. In fact the intensity was found to be appropriate to $(B-V) \sim 1^m\cdot08$, corresponding to type K1 III; it could well be argued that β LMi must include a star at least as late as that to produce a line of such strength.

Other investigations, such as that of McWilliam[79], do not support the findings of over-abundances in β LMi, but the whole principle of performing abundance analyses on a raw composite spectrum seems very questionable.

The matter of the absolute magnitude of the system has been settled quite precisely by *Hipparcos*[29] (7, 1030), which found a parallax that corresponds to the distance modulus $3^m\cdot26 \pm 0^m\cdot09$, making the integrated absolute magnitude, *i.e.*, the combined luminosity of the two stars, $+0^m\cdot95$, with the same uncertainty of $0^m\cdot09$.

The probability of the spectrum being composite was evident to Stephenson & Sanwal[139], who were interested in the masses of stars above the main sequence. They listed a total mass for the pair, on the basis of the orbit by Baize[115], of $3\cdot45\ M_\odot$. Seemingly just from a Δm value plus the assumption that the secondary star would be a main-sequence one, they listed the types as G8 III–IV + (F4 V). They evidently did not allow for the fact that the G8 III–IV classification was for the system as a whole and not just for the primary star, so if the companion were hotter than that classification, which must be a weighted average of the components' types, then the primary would have to be cooler. That point occurred to Edwards[140], who 'split' the types of a lot of visual binaries in accordance with their Δm values, and gave K0 III–IV + F8 V for β LMi on the basis that its Δm was $1^m\cdot69$.

Radial velocities and orbit of β Leonis Minoris

As in the case of κ Per, the first radial-velocity observation of β LMi was made in 1899 at Lick. The catalogue[6] of the Lick radial-velocity survey of bright stars includes six measurements of it. Unfortunately they are confined to the interval 1899–1909 when the system was near apastron (the periastron and nodal passages would have been in or about 1922/23) and no variation of velocity was detected. Indeed, the six measures agree well with one another: their mean is $+6\cdot8$ km s^{-1}, with an r.m.s. deviation of $0\cdot7$ km s^{-1} for an individual observation and a standard error of $0\cdot3$ km s^{-1} for the mean. Küstner[141], too, contributed some early observations, as he did for κ Per; there are four of them, obtained in 1910–13. The last one is some 5 km s^{-1} — usually a significant amount in Küstner's work — below the mean of the first three, and drew the comment "*Gute Platte, Abweichung auffällig*" (Good plate, striking discordance); but it is not at a phase when much change should have occurred. In 1923 Adams & Joy[134] published from Mount Wilson an un-dated mean velocity, from three plates, of $+4\cdot7 \pm 0\cdot6$ ('probable error') km s^{-1}; much later, Abt[142] listed the individual values, which do not give exactly the previously quoted mean and uncertainty, and he also found a fourth observation, obtained in 1941 and quite discordant with the first three at $+12\cdot3$ km s^{-1}. Harper[96] published just a single velocity that was obtained at the DAO in early 1922 and is much lower ($-2\cdot7$ km s^{-1}) than other measurements of the star. The discrepancy from previously published velocities clearly did not escape Harper's attention, because the result is flagged to indicate that the plate concerned had been measured a second time; it will be seen from the discussion below that its deviation is actually in keeping with the velocity excursion that we now know to take place near periastron.

It was at that stage, with 16 velocities having been measured altogether, at four observatories, that the *Radial Velocity Catalogue*[91] was compiled. Its author, being at the Mount Wilson Observatory, was privy to the individual data that had been in part published as a mean by Adams & Joy[134]. The mean velocity was catalogued as $+5 \cdot 6$ km s^{-1}, with quality *a*, the highest quality, and no suggestion was made of variability.

There ensued the work, already mentioned above, of Miss Underhill[119] at Victoria, with her 80 spectrograms taken around the time that the periastron/nodal passage of β LMi was expected, from the visual orbit, to occur; they seemed, however, to suggest a 5-year periodicity at least as much as they documented the nodal passage. From the elements that she derived by a combination of her radial velocities with an unpublished visual orbit by van Biesbroeck, and on the basis of a parallax of $0'' \cdot 02$ which is not far from the truth, Miss Underhill deduced values of $2 \cdot 10$ and $0 \cdot 54$ $M_{\odot}$ for the masses of the components. Abt, Sanwal & Levy[143] published 20 velocities obtained at Kitt Peak with the coudé-feed system of the 84-inch telescope. The measurements were made in 1964–71 with one isolated one in 1976. They show a mean of $+6 \cdot 9 \pm 0 \cdot 4$ km s^{-1} with a dispersion of $1 \cdot 8$ km s^{-1} per observation; there is a comment that they are near apastron and "show no long-term variation but may show a small short-period (months) variation". Beavers & Eitter[99] gave two radial velocities photoelectrically measured at Ames in 1977 and 1978. In connection with his work[118] on the visual orbit, Heintz[117] gave three radial velocities, all obtained at Kitt Peak within a few days of one another in 1979. De Medeiros & Mayor[92] have provided, through the CDS, two velocities of β LMi, obtained with the OHP *Coravel* in 1986 and 1987; probably on the basis of the same two observations, de Medeiros, da Silva & Maia[144] have listed a rotational velocity of $4 \cdot 0$ km s^{-1} for the star, considering it to be a single-lined binary.

The writer's own observations of β LMi began, with the original radial-velocity spectrometer on the Cambridge 36-inch reflector, in 1970 January and have been made reasonably systematically ever since, including at least one measurement in each of the 39 successive calendar years. They just about cover one complete cycle of the orbit, whose period according to the most recent visual orbit[123] is $38 \cdot 62$ years. As it happened, the starting date was not particularly auspicious, coming as it did quite soon after a periastron passage, and the velocity was practically constant for the first fifteen years. There then ensued a slow decline, during which the observational programme was largely transferred to the OHP *Coravel*. At length, in 1995 January, after the object had been under observation for 25 years, the radial-velocity trace was recognized as being incipiently double-lined: an indication appeared of a small secondary dip closely blended into the red wing of the primary. As the system moved towards the nodal passage and the periastron that closely followed it, the secondary became increasingly conspicuous. It was a bit frustrating to the writer that after such a long campaign the actual nodal passage, at which the best opportunity would occur to delineate accurately the profile of the secondary dip, was largely missed through lack of access to an instrument with which it could be observed, although other OHP observers kindly assisted: Dr. M. Imbert obtained one observation close to the node, and Drs. J.-M. Carquillat and S. Udry obtained others not very far from it. At that time, the Cambridge *Coravel* was inoperative and great difficulty was being experienced with it. Access to the OHP one ceased in 1998 July; a final observing run was scheduled with it over the following Christmas period, but was negated before it began by instrument failure. Knowledge of the exact profiles of the dips seen in radial-velocity traces enables the reductions of blended observations to be made with confidence by the imposition of fixed dip profiles in the reduction process. As soon as the Cambridge *Coravel* was operational in late 1999 the observations were resumed, but the nodal passage was then past and the velocity separation was rapidly decreasing.

The new velocities of β LMi comprise 44 obtained with the original spectrometer at Cambridge, 37 with the OHP *Coravel*, 51 with the Cambridge *Coravel*, four with the spectrometer at the DAO 48-inch telescope, and two with the Palomar 200-inch — 138 in all. They are listed in Table II. To obtain the best solution of the orbit from the available observations proved to be a difficult problem. First of all, there was the question of what if anything to do with the nine sources of published velocities. In only two cases[119,143] are they at all systematic, with reasonably continuous coverage spanning a number of years. Those of Abt *et al.*[143] were obtained near apastron and show no significant change in velocity at all, while those of Miss Underhill[119], though obtained near periastron, do not seem to document it satisfactorily. So it was decided not to try to utilize *any* of the published observations, but to rely entirely upon the writer's own data.

TABLE II

Radial-velocity observations of β Leonis Minoris

Primary-only measures are retrieved from blends by 'dragging function'; weight 0·2.
Except as noted, the sources of the observations are as follows:
1970–1991 — original Cambridge spectrometer;
1992–1999 — Haute-Provence Coravel; 2000–2008 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	*(O – C)* Sec. km s⁻¹
1970 Jan. 7·03	40593·03	+8·9	—	0·233	−0·9	—
1971 Feb. 4·07	40986·07	+9·5	—	0·261	−0·2	—
15·98	997·98	+9·6	—	·262	−0·1	—
Dec. 10·23	41295·23	+8·8	—	·283	−0·9	—
1972 Nov. 23·27	41644·27	+10·1	—	0·308	+0·6	—
1973 Feb. 23·96	41736·96	+9·7	—	0·314	+0·2	—
1974 Apr. 4·92	42141·92	+8·5	—	0·343	−0·9	—
May 30·18*	197·18	+9·6	—	·347	+0·2	—
Dec. 30·20	411·20	+9·4	—	·362	+0·1	—
1975 Feb. 21·06	42464·06	+9·3	—	0·366	0·0	—
May 3·96	535·96	+10·4	—	·371	+1·2	—
June 11·89	574·89	+9·0	—	·374	−0·2	—
Sept. 19·20	674·20	+8·5	—	·381	−0·7	—
1976 Jan. 14·12	42791·12	+9·3	—	0·389	+0·2	—
May 6·89	904·89	+9·0	—	·397	−0·1	—
Nov. 27·24	43109·24	+9·3	—	·412	+0·3	—
1977 Mar. 13·02	43215·02	+9·1	—	0·419	+0·2	—
May 24·89	287·89	+9·7	—	·424	+0·8	—
Nov. 1·18	448·18	+8·0	—	·436	−0·8	—

TABLE II (*continued*)

Date (UT)		MJD	Velocity Prim. km s^{-1}	Velocity Sec. km s^{-1}	Phase	(O – C) Prim. km s^{-1}	(O – C) Sec. km s^{-1}
1978	Apr. 12·96	43610·96	+10·1	—	0·447	+1·3	—
	June 18·90	677·90	+9·4	—	·452	+0·7	—
	Oct. 12·23	793·23	+8·6	—	·460	−0·1	—
1979	Feb. 25·07	43929·07	+7·6	—	0·470	−1·0	—
	May 13·92	44006·92	+9·4	—	·475	+0·8	—
	Nov. 28·21	205·21	+9·5	—	·489	+1·0	—
1980	May 4·93	44363·93	+8·9	—	0·501	+0·5	—
	Dec. 7·23	580·23	+8·9	—	·516	+0·6	—
1981	Feb. 2·08	44637·08	+8·5	—	0·520	+0·3	—
1982	Jan. 10·13	44979·13	+8·4	—	0·544	+0·4	—
	Mar. 5·03	45033·03	+8·1	—	·548	+0·1	—
	May 25·87	114·87	+8·4	—	·554	+0·4	—
1983	Feb. 23·03	45388·03	+8·5	—	0·573	+0·7	—
	Dec. 11·18	679·18	+7·2	—	·594	−0·4	—
1984	June 9·92	45860·92	+7·7	—	0·607	+0·2	—
	Nov. 9·59†	46013·59	+7·7	—	·618	+0·3	—
1985	Feb. 17·41†	46113·41	+7·3	—	0·625	0·0	—
	May 31·89	216·89	+6·7	—	·632	−0·5	—
	Nov. 12·26	381·26	+6·7	—	·644	−0·4	—
1986	Jan. 4·47*	46434·47	+7·1	—	0·648	+0·1	—
	May 15·88	565·88	+7·7	—	·657	+0·8	—
	Dec. 12·19	776·19	+6·4	—	·672	−0·4	—
1987	Feb. 28·95‡	46854·95	+7·3	—	0·677	+0·6	—
	May 24·89	939·89	+6·6	—	·683	0·0	—
	Oct. 19·22‡	47087·22	+6·8	—	·694	+0·3	—
1988	Jan. 26·40†	47186·40	+6·3	—	0·701	−0·1	—
	Mar. 10·98‡	230·98	+6·5	—	·704	+0·2	—
	May 21·89	302·89	+6·9	—	·709	+0·6	—
	Nov. 3·24‡	468·24	+6·2	—	·721	+0·1	—
1989	Mar. 17·97	47602·97	+5·9	—	0·730	−0·1	—
	May 30·90	676·90	+6·7	—	·736	+0·8	—
1990	Jan. 14·10	47905·10	+6·9	—	0·752	+1·3	—
	Dec. 4·26	48229·26	+5·6	—	·775	+0·4	—
1991	Feb. 4·07‡	48291·07	+5·2	—	0·779	+0·1	—
	June 13·91	420·91	+5·1	—	·788	+0·2	—
	Dec. 21·07‡	611·07	+4·6	—	·802	0·0	—
1992	Feb. 27·36†	48679·36	+4·7	—	0·807	+0·2	—
	June 20·84	793·84	+4·0	+12·1	·815	−0·3	+2·4
	Dec. 17·14	973·14	+4·1	+9·2	·828	+0·1	−1·1
1993	Feb. 11·06	49029·06	+3·7	+10·8	0·832	−0·2	+0·4
	July 6·85	174·85	+2·7	+12·2	·842	−0·8	+1·2
	Dec. 27·13	348·13	+3·4	+13·3	·854	+0·3	+1·7
1994	Feb. 17·07	49400·07	+3·1	+12·6	0·858	+0·1	+0·8
	May 2·85	474·85	+2·7	+10·1	·863	−0·1	−2·0
	Dec. 12·26	698·26	+1·9	+12·6	·879	−0·3	−0·6

TABLE II (*continued*)

Date (UT)		MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O − C) Prim. km s⁻¹	(O − C) Sec. km s⁻¹
1995 Jan.	3·08	49720·08	+1·9	+14·5	0·881	−0·2	+1·2
	7·11	724·11	+2·1	+14·8	·881	0·0	+1·5
May	30·87	867·87	+1·5	+13·5	·891	−0·1	−0·6
June	6·84	874·84	+1·6	+13·7	·892	0·0	−0·5
Dec.	27·10	50078·10	+0·4	+13·0	·906	−0·4	−2·5
1996 Jan.	1·12	50083·12	+0·9	+14·7	0·906	+0·1	−0·8
Mar.	28·91	170·91	+0·6	+15·5	·913	+0·2	−0·7
Apr.	24·88	197·88	+0·4	+14·2	·914	+0·1	−2·2
Nov.	21·18§	408·18	−0·8	+17·3	·929	+0·1	−0·9
Dec.	15·20	432·20	−1·1	+15·0	·931	−0·1	−3·4
1997 Jan.	23·04R	50471·04	+1·5	+20·2	0·934	+2·7	+1·4
Feb.	19·02§	498·02	−1·0	+19·2	·936	+0·4	+0·1
Mar.	27·00§	534·00	−1·3	+18·8	·938	+0·3	−0·7
Apr.	7·91§	545·91	−1·2	+18·6	·939	+0·5	−1·0
	25·90	563·90	−1·6	+18·2	·940	+0·2	−1·6
May	6·93§	574·93	−1·8	+18·5	·941	+0·1	−1·4
July	18·84	647·84	−2·3	+18·5	·946	+0·1	−2·2
Sept.	9·17	700·17	−3·1	+17·1	·950	−0·3	−4·3
Oct.	25·20C	746·20	−3·1	+18·5	·953	0·0	−3·5
Nov.	21·22U	773·22	−3·5	+19·2	·955	−0·1	−3·1
Dec.	22·15	804·15	−3·7	+17·6	·957	−0·1	−5·1
1998 Jan.	22·17C	50835·17	−4·4	+18·2	0·960	−0·5	−4·9
Feb.	28·06U	872·06	−4·4	+18·4	·962	−0·2	−5·2
Apr.	28·88	931·88	−4·4	+19·5	·967	+0·2	−4·9
June	11·90C	975·90	−5·0	+19·2	·970	0·0	−5·8
July	9·85	51003·85	−4·9	+19·5	·972	+0·3	−5·8
	23·84	017·84	−5·4	+18·6	·973	−0·1	−6·9
1999 Apr.	11·90I	51279·90	−5·9	+20·3	0·991	0·0	−6·2
Dec.	20·14§	532·14	−2·3	+19·0	1·009	−0·1	−1·4
2000 Jan.	9·10	51552·10	−2·2	+20·1	1·011	−0·4	+0·4
Feb.	11·07	585·07	−0·6	+19·2	·013	+0·5	+0·7
Mar.	21·97	624·97	−0·1	+16·5	·016	+0·1	−0·7
Apr.	7·91	641·91	+0·4	+17·3	·017	+0·3	+0·7
May	14·90	678·90	+1·0	+14·1	·020	+0·1	−1·3
	25·90	689·90	+1·0	+14·2	·020	−0·1	−0·8
Nov.	13·25	861·25	+3·7	+9·9	·032	−0·2	−0·4
Dec.	9·20	887·20	+4·1	+10·3	·034	−0·2	+0·5
2001 Jan.	16·08	51925·08	+4·1	+10·2	1·037	−0·6	+1·2
Nov.	3·24	52216·24	+7·2	+3·0	·058	0·0	−1·9
Dec.	14·24	257·24	+7·3	+2·0	·061	−0·1	−2·5
2002 Jan.	25·12	52299·12	+7·6	+1·9	1·063	−0·1	−2·3
Feb.	23·06	328·06	+8·2	+3·1	·066	+0·4	−0·9
Mar.	26·98	359·98	+8·3	+0·7	·068	+0·4	−3·0
Apr.	23·95	387·95	+8·2	+2·8	·070	+0·2	−0·7
May	7·89	401·89	+8·4	+2·8	·071	+0·3	−0·7
June	3·91	428·91	+8·4	+2·1	·073	+0·2	−1·2
Sept.	30·21	547·21	+8·0	+5·8	·081	−0·6	+3·1
Oct.	24·23	571·23	+8·4	+2·3	·083	−0·2	−0·3
Dec.	5·21	613·21	+8·6	−0·1	·086	−0·2	−2·5
2003 Jan.	6·15	52645·15	+8·9	+1·3	1·088	+0·1	−1·0
Feb.	18·05	688·05	+8·9	+0·2	·091	0·0	−1·9
Mar.	16·96	714·96	+9·2	+1·3	·093	+0·2	−0·7

TABLE II (*concluded*)

Date (UT)	MJD	Velocity Prim. km s⁻¹	Velocity Sec. km s⁻¹	Phase	(O − C) Prim. km s⁻¹	(O − C) Sec. km s⁻¹
2003 Apr. 17·89	746·89	+9·1	+0·9	1·095	+0·1	−1·0
May 25·89	784·89	+9·2	+1·2	·098	+0·1	−0·6
June 20·90	810·90	+9·1	+1·3	·100	−0·1	−0·4
Nov. 4·25	947·25	+9·3	+1·6	·109	−0·1	+0·2
Dec. 8·24	981·24	+9·3	+0·9	·112	−0·1	−0·4
2004 Jan. 9·16	53013·16	+9·3	+0·3	1·114	−0·1	−1·0
Feb. 9·11	044·11	+9·6	+1·5	·116	+0·1	+0·3
Mar. 30·97	094·97	+9·5	+0·2	·120	0·0	−0·9
Apr. 21·94	116·94	+9·4	+0·8	·121	−0·1	−0·3
May 18·88	143·88	+9·3	+0·1	·123	−0·3	−1·0
Nov. 14·27	323·27	+9·7	+0·7	·136	0·0	−0·1
2005 Jan. 9·12	53379·12	+9·7	+1·4	1·140	0·0	+0·6
May 2·92	492·92	+9·7	0·0	·148	−0·1	−0·7
Nov. 19·26	693·26	+10·0	+0·4	·162	+0·2	−0·2
2006 Jan. 29·13	53764·13	+9·7	+1·5	1·167	−0·1	+0·9
Mar. 4·04	798·04	+9·7	−0·4	·170	−0·1	−1·0
May 10·94	865·94	+9·8	+0·5	·175	−0·1	−0·1
Nov. 19·28	54058·28	+9·9	−0·3	·188	0·0	−0·8
2007 Feb. 7·09	54138·09	+9·9	−0·3	1·194	0·0	−0·8
Apr. 28·90	218·90	+10·1	+0·6	·200	+0·2	+0·1
Nov. 24·24	428·24	+9·8	+0·8	·214	−0·1	+0·2
2008 Jan. 6·22	54471·22	+10·0	+1·9	1·218	+0·1	+1·3

* Observed with Palomar 200-inch telescope.
† Observed with DAO 48-inch telescope.
‡ Observed with OHP *Coravel*.
§ Observed with Cambridge *Coravel*.
C Observed by Dr. J.-M. Carquillat.
U Observed by Dr. S. Udry.
I Observed by Dr. M. Imbert.
R Rejected observation.

Even with those data, a major difficulty had already become apparent some years ago. The Cambridge observations, though having a serious gap in the critical vicinity of the periastron passage, appear to give a tolerably satisfactory representation of the velocity curve for the secondary component (as well as that of the primary), *but the OHP velocities do not fit it*. The secondary dip is not extremely weak — both *Coravel* instruments, Cambridge and OHP, agree in putting its strength (area) at 18% of that of the primary — and enough of its profile is distinct from that of the primary (*cf.* Fig. 2) that there ought to be no great uncertainty concerning its width. Yet the OHP traces show its velocity as rising only gently, and finally hardly at all, during the approach to the nodal passage of 1999, when the primary velocity was falling regularly. Dr. S. Udry and Mr. P. Figueira have made repeated efforts to see if any other interpretation can be placed on the OHP radial-velocity traces, by the imposition of different dip profiles (within the very limited range of plausibility) or none at all, but those experiments have made little more than cosmetic changes to the results; the fundamental difficulty remains inescapable, and the orbit can be presented only with the shortcoming included.

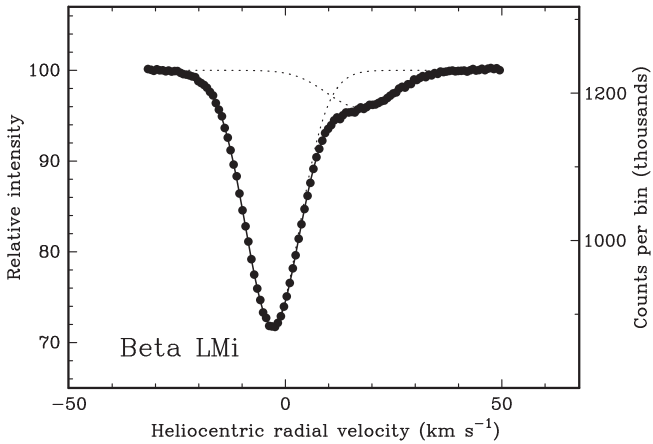

FIG. 2

Radial-velocity trace of β Leonis Minoris, obtained with the Cambridge *Coravel* on 1997 May 6 and illustrating the double-lined nature of traces made near the nodal passage. The projected rotational velocities are considered to be 'too small to measure' for the primary and 9·5 km s^{-1} for the secondary. The latter is a mean value from the best-resolved traces and has been imposed on the reductions of all of the observations. Such adoption of a fixed profile for the secondary dip is essential if meaningful results are to be obtained from heavily blended traces (*cf.* Fig. 3), which are in a majority. The procedure does, however, provide a possible source of systematic error in some of the orbital elements, particularly in K_2, although not on anything like the scale needed to explain the discrepancies between the Cambridge and OHP velocities of the secondary component near the nodal passage.

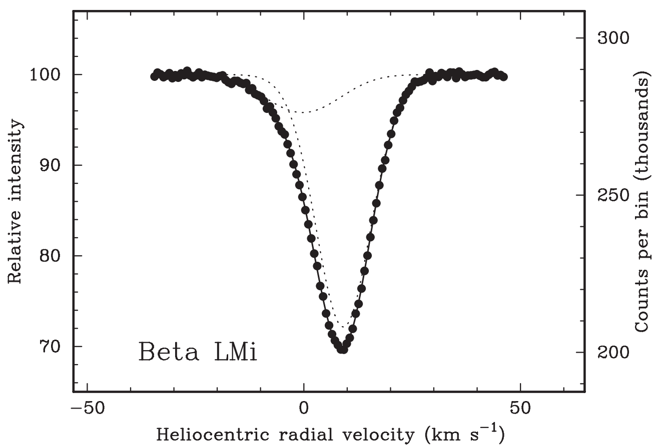

FIG. 3

Radial-velocity trace of β Leonis Minoris, obtained with the Cambridge *Coravel* on 2007 February 7. It shows that there is an asymmetry that is noticeable (if the S/N ratio is good enough), revealing the double-lined nature of the system, even on the apastron side of the orbit when the velocity difference between the components is relatively small. It is believed that, with prior knowledge of the profiles of the two dips, it is possible to obtain tolerably reliable velocities for the individual components even from such heavily blended traces.

It is not just a question of the OHP secondary velocities demanding a quite different value for K_2 from the Cambridge ones: in any simple binary system, the velocity curve for the secondary has got to be a mirror image of that of the primary, apart from its amplitude being a disposable parameter. In the present instance, where we are dealing with a bright star whose radial-velocity traces show a magnificently deep dip with extremely high S/N ratio, the form of the primary's velocity curve is pretty well set in stone.

Another problem is that the system appears single-lined except near periastron, and almost two-thirds of the cycle had been witnessed before the observer was even aware that the secondary star was observable at all. Especially now that its signature is known, it is observable in traces made on the apastron side of the orbit, such as the recent one reproduced in Fig. 3, as a slight blunting of the blueward shoulder of the 'dip'; but the original spectrometer, with which so much of the orbit was observed, could not give the S/N ratio necessary to disclose such a minor asymmetry. That difficulty has been overcome by appeal to the 'dragging-function' method, which has been used in this series of papers a number of times and was explained most recently in the section on HR 4964 in the paper[145] immediately preceding *this* one. The resolved observations were first used to obtain a preliminary orbit and in particular a γ-velocity. The exact relationship between the offset from the γ-velocity and the error incurred in adopting the velocity of the blend as if it were the velocity of the primary was established explicitly by

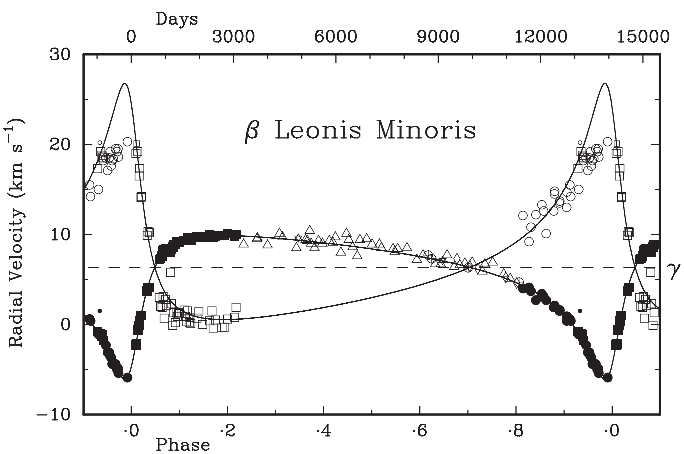

FIG. 4

The observed radial velocities of β Leonis Minoris, plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The diagram plots only the author's observations plus a few made by others with the OHP *Coravel*. Filled and open circles and squares represent primary and secondary measurements from the OHP and Cambridge *Coravel*s, respectively. The velocity curve for the secondary is fitted to the Cambridge observations (open squares) alone. Other open symbols are used to plot velocities of the primary star that were obtained before the system was recognized as double-lined; the traces were reduced as single but have been doctored by the 'dragging-function' procedure (see text) to recover as well as possible the primary's velocities. All have been given weight 1/5 in the solution of the orbit. The triangles denote observations made with the original Cambridge spectrometer, stars Palomar, circles with plusses in them DAO, and diamonds OHP. One pair of OHP observations, clearly if unaccountably in error, are plotted with very small symbols and were ignored in the calculation of the orbit.

reducing the double-lined traces a second time, as if they were single-lined, and then that relationship was used to obtain unbiassed primary velocities from blended traces. The secondary velocities cannot be recovered with useful accuracy by such a process. Much of the power of the *Coravel*s for measuring F-type stars comes from the violet and near-ultraviolet parts of the spectrum, where F stars are bright and have many strong lines. The other instruments operated in the blue and were much less efficient in measuring F-type spectra, giving much smaller dips for such stars than the *Coravel*s. For that reason the dragging corrections made to their observations, all of which appeared single-lined, were halved in comparison with those that would have been made to analogous *Coravel* measurements; the maximum was 0·4 km s^{-1}.

In an effort to make the various sources of data as homogeneous as possible, the usual offset of +0·8 km s^{-1} has been applied to the OHP velocities, and one of −0·3 km s^{-1} has been made to the Cambridge *Coravel* ones. As always in these papers, the adjustments have already been made to the tabulated entries. The primary velocities from double-lined traces from both *Coravel*s have been accorded unit weight; all of those derived from blends by means of the dragging function — the 'original Cambridge', DAO, and Palomar velocities, and the first six OHP ones — have been given weight 0·2. The Cambridge secondary measurements have received weight 0·025; those made at OHP are being omitted from the initial solution, not because they are considered to be worse than the Cambridge ones but simply because, for reasons unknown, they cannot be made to match the same — or indeed any Keplerian — solution. The orbital period is not very accurately determined by the radial velocities, and has been fixed at 14 100 days, a slightly rounded version of the value (38·62 years) given by the recommended visual orbit. Fig. 4 shows the data and the computed velocity curves; the orbital elements are as follows:

P	=	14100 days (fixed)	$(T)_1$	=	MJD 51404 ± 6
γ	=	+6·34 ± 0·03 km s^{-1}	$a_1 \sin i$	=	1126 ± 10 Gm
K_1	=	7·98 ± 0·06 km s^{-1}	$a_2 \sin i$	=	1852 ± 58 Gm
K_2	=	13·1 ± 0·4 km s^{-1}	$f(m_1)$	=	0·287 ± 0·008 $M_\odot$
q	=	1·64 ± 0·05 (= m_1/m_2)	$f(m_2)$	=	1·28 ± 0·12 $M_\odot$
e	=	0·685 ± 0·004	$m_1 \sin^3 i$	=	3·30 ± 0·26 $M_\odot$
ω	=	215·6 ± 0·5 degrees	$m_2 \sin^3 i$	=	2·01 ± 0·09 $M_\odot$

R.m.s. residual (unit weight) = 0·23 km s^{-1}

Since the orbital inclination is known from the 'visual' orbit to be about 79°·1, the actual masses of the stars must be larger than the minima indicated spectroscopically by the inverse sine-cubed of that angle — a factor of 1·056. The exact factor is, however, academic, because it must be said at once that the values of K_2 and of all the quantities derived from it (including, in particular, the masses) are quite uncertain, and the formally computed standard errors given in the table above are far smaller than the true uncertainties of those quantities. The reason is, of course, the uncertainty created by the non-statistical residuals of the secondary velocities. We see from Fig. 4 that, if the OHP velocities of the secondary are disregarded altogether, the fit to the Cambridge ones alone is reasonable, although it is not too good to those made in the early years on the apastron side of the orbit (phases around ·1). The computed velocity curve for the secondary star peaks at +26·8 km s^{-1}, but it seems almost certain from the OHP measurements that in reality the velocity scarcely rose above +20 km s^{-1} at the node.

It would be a mistake to yield to the temptation to reject the OHP measurements *en bloc* just because we cannot understand them; if we rejected everything we could not understand, science would progress very slowly, if at all. We hesitate to offer any suggestion as to the reason for the misbehaviour of the secondary velocities, recalling with unease how Miss Underhill's postulation of a subsidiary oscillation in the velocity of the *primary* has not been borne out by the observations of the ensuing cycle. The misbehaviour in the present case is, however, rather certainly real, and the only excuse for it would seem to be in terms of multiplicity of the secondary; but no significant change in the profile of the secondary dip, to the extent that that profile has been observable, has been witnessed, and there is no obvious periodicity in the velocity residuals.

If, instead of disregarding the OHP velocities, we try rejecting the Cambridge ones, the fit to the OHP data is still poor and the fit to the Cambridge ones is terrible. K_2 is reduced to 9·9, q to 1·24, and the minimum masses to 1·81 and 1·45 $M_\odot$, which actually seem rather plausible. The secondary's velocity curve peaks at 21·7 km s^{-1}, still a bit higher than was actually observed. It is of course a matter for regret that an observational campaign lasting nearly 40 years has not, in the end, provided a more definite conclusion; a comparison of Fig. 4 above with Fig. 3 of ref. 119 does, however, suggest that *some* progress has been made in the documentation of β LMi. In order to provide at any rate a modicum of hard information that is scarcely compromised by the difficulties over the secondary star, we can solve the observations given in Table II for the primary alone, to give a single-lined orbit whose elements are:

P = 14100 days (fixed)	$(T)_1$ = MJD 51400 ± 7
γ = +6·38 ± 0·04 km s^{-1}	$a_1 \sin i$ = 1129 ± 10 Gm
K = 7·97 ± 0·06 km s^{-1}	$f(m)$ = 0·289 ± 0·008 $M_\odot$
e = 0·683 ± 0·004	
ω = 215·2 ± 0·5 degrees	R.m.s. residual (wt. 1) = 0·25 km s^{-1}

Those elements, which owing to the great relative weight of the measurements of the primary in the double-lined orbit are very close to the corresponding ones in the double-lined set given above, are nevertheless recommended as supplanting the latter, since they are almost free from the uncertainties surrounding the secondary. The qualification 'almost' in that assertion arises because about 12% of the total weight of the data that underpin the primary's elements is represented by measurements that have been slightly doctored by the dragging-function procedure, whose validity could be impaired if the secondary does not act in the implicitly expected fashion. The primary star's velocity curve and the fit of the points to it look quite good in isolation from the problems of the secondary! The curve itself, but not the points, is illustrated, to the same vertical scale as in Fig. 4, in Fig. 5. Since the curve is indistinguishable from the corresponding one in Fig. 4, there would be no purpose in presenting it anew were it not for the fact that it provides an opportunity to illustrate the relative unserviceability, for purposes of orbit determination, of the nine sets of published radial velocities of β LMi, which were described at the beginning of this section of the paper. None of them was measured as double-lined; they are not even entered in Table II let alone incorporated in the solution of the orbit. It is thought that Fig. 5 justifies that decision.

In considering the nature of the secondary (and indeed of the primary) star, we are handicapped by not being in possession of an unambiguous value of q or even being certain that the mass attributed to the visual secondary represents only one

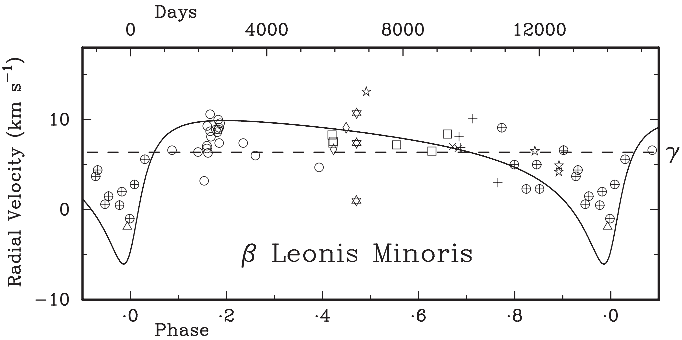

FIG. 5

The orbital velocity curve for β Leonis Minoris A, computed from the writer's velocities for the primary star alone, with the radial velocities published by others plotted for comparison. All of the published velocities have been increased by 0·8 km s^{-1} with a view to placing them as nearly as possible on the same zero-point. In no case were they measured as double-lined, so they represent means for the two components, heavily weighted towards the brighter primary star. The correspondence between the symbols and their sources is as follows: squares — Lick[6]; plusses — Bonn[141]; five-point stars — Mount Wilson[134,142]; triangle — DAO (Harper)[96]; circles with plusses in them — DAO (Underhill)[119]; circles — KPNO[143]; diamonds — Ames[99]; six-point stars — Heintz[117]; crosses — OHP[92].

star. Moreover, the spectrum has never been recognized as composite. In a paper that seems to have been published twice[124,125], Gontcharov & Kiyaeva have plotted astrometric positions of the photocentre on a scaled plot of the visual orbit of β LMi with a view to determining the optimal scale factor and thus the mass ratio. They obtained masses of 1·3 ± 0·4 and 1·7 ± 0·4 $M_\odot$ for the primary and secondary components, respectively; those values are not regarded here as being sufficiently precise and reliable to serve as a basis for discussion.

We can still rely on the known value of ΔHp (very nearly the same as ΔV), 1^m·42, which corresponds to a luminosity ratio of 3·61 in approximately the V band, and the ratio of dip areas in radial-velocity traces, of 1 to 0·18 or 5·65 to 1. The difference between those two ratios shows that the visual secondary is a star that, magnitude for magnitude, gives a much smaller dip than the primary, which we know is a late-type giant. The obvious conclusion, which has already been reached on less-compelling grounds by authors such as Stephenson & Sanwal[139] and Edwards[140], is that the secondary is an F-type main-sequence star. In that case the Δm between the components must be substantially smaller in the spectral region ($\sim B$) in which the *Coravel* operates than it is in V, thereby making the intrinsic dip strength of the secondary smaller still with respect to the primary's, as indeed would befit an early-F main-sequence star. An effort to produce a photometric model that meets all the criteria proved to succeed readily, and is shown here as Table III. It starts by attributing to the components absolute magnitudes that accord with the *Hipparcos* total M_V and Δm, and with the colour indices tabulated by Schmidt-Kaler[146] for types K2 III and F0 V. The absolute magnitude that has had to be assigned to the primary is fainter by 0^m·7 than the value listed for K2 III by Schmidt-Kaler; reference to Keenan & Barnbaum's post-*Hipparcos* discussion[69] shows that such a star is perfectly possible, on the red edge of the 'clump', and would best be assigned a type of K2 IIIb.

The absolute magnitude attributed in the model to the F0 V model secondary is extremely close to the tabulated[146] value of $2^m \cdot 7$; the mass of such a star is more consonant with the value of q suggested by the OHP velocities of the secondary than by the Cambridge ones.

TABLE III

Photometric model (absolute magnitudes, colour indices) for β LMi

	Star	M_V	$(B-V)$	$(U-B)$	M_B	M_U
		m	*m*	*m*	*m*	*m*
Model	K2 IIIb	1.21	1.16	1.16	2.37	3.53
	F0 V	2.63	0.30	0.03	2.93	2.96
	K2 IIIb + F0 V	0.95	0.91	0.60	1.86	2.46
β LMi (observed)		0.95	0.91	0.64		

At this point the author laments — not for the first time in this series of papers — that people who have access to large telescopes that offer spectrographs behind adaptive optics, or indeed to the *Space Telescope*, do not take an appropriate interest in binary stars. They could accomplish such a lot in exposures of mere seconds. In particular they could take spectra of the individual components of systems like (and including) β LMi, with separations of half a second of arc or more, and then there would be no need for inference or argument about the exact natures of the components.

Although the orbit for β LMi has a considerably longer period than any previous spectroscopic orbit observed completely round a cycle, it cannot be claimed to be the longest-period good orbit determined entirely spectroscopically, because (*a*) the situation regarding the velocity curve of the secondary star prevents the classification of this orbit as 'good', and (*b*) in any case we have appealed to the visual orbit to fix the period.

56 Ursae Majoris (HR 4392, HD 98839)

In presenting here an orbit for 56 UMa, the writer is taking a second bite at a cherry that he found rather unpalatable at a first sampling[147] in 1996. The star was observed early in the very first season (1966) in which stellar velocities were ever measured by cross-correlation, but only two observations of it were made before it was lost in the evening twilight — too few to warrant its featuring among the 'proof' measurements in the initial paper[103] on the technique. The velocity in 1966 was about $+2$ km s^{-1}; it was sensibly the same in 1971/2, but in 1979 it was found close to zero, and subsequent systematic measurements documented a further very gradual decline to near -3. Meanwhile MK spectral-type assignments, which started[148] at G8 II, were moderated somewhat as regards luminosity class when it was found that the star had an over-abundance of barium and other *s*-process elements, confusing the normal spectroscopic luminosity criteria, as well as unusually strong lines of other elements. There is a chequered history of classifications in which barium indices were first associated with the classified type of 56 UMa and then omitted again; it largely parallels the corresponding history of ζ Cyg, in whose case the vacillations were helpfully explained[149] by Keenan. It seems a little unsatisfactory that in Keenan's final classification[68] 56 UMa appears as one of the best-determined standards for type G8 IIIa, although its standing as a 'mild barium star' is believed still to be valid.

In 1996, after 30 years' radial-velocity observations had shown nothing more than a very slow monotonic decline amounting in total to less than 5 km s^{-1}, the writer despaired of ever seeing the object round a cycle. At the same time he thought that the mere fact of that particular mild barium star being a spectroscopic binary with a period which, though indeterminate, was evidently a great deal longer than the longest so far documented for barium stars (those of 16 Ser[150] and ζ Cyg[149], of 15 and 18 years, respectively), was significant enough to merit publication of a note[147] about it. Speculation in the note about the orbital period was informed only by the slender evidence of three Lick measurements[6], made around 1920 and having a mean of $+2 \cdot 9$ km s^{-1}. If they represented a phase one cycle or so before the start of the Cambridge observations then the period would be some 40 years, but if there were no intervening cycle then the period could not be much less than a century. The shorter period (the note said) "would be ruled out if the star does not return to positive velocities within the next decade or so."

56 UMa promptly acted as though it saw the note as throwing down the gauntlet! The note was published in 1996 December. In that very year and month, on Christmas Day, the velocity was found to have begun a significant rise, and by 1999 it was up to $+3$ km s^{-1}, higher than it had been in 1966 when observations began! For a few years after that, there still remained an ambiguity as to whether the period was about 17 000 or about 30 000 days (47 or 82 years), but after the node was passed (at a velocity of nearly $+6$ km s^{-1}, in 2002) there was no doubt that the 'short' period is the correct one.

From places that suffer less from philistine artificial lighting than Cambridge, 56 UMa, at fifth magnitude, may still be visible to the naked eye. In any case it is to be found about 2½° following, 1° south of the third-magnitude ψ UMa, which is readily identified, being near the intersection of the lines that constitute the sides of the 'Dipper' asterism of Ursa Major, produced in the direction away from the Pole. The V and B magnitudes, and in some cases U, have been measured by Häggkvist & Oja[25], Johnson *et al.*[26], Eggen[27], Bouigue[151], Argue[152], Moffett & Barnes[153], Fernie[154], and apparently by Jennens & Helfer[155], who are believed to have included it in an unpublished listing that was "available on request" some 30 years ago. The agreement is not perfect, but there can be little doubt that the best values are very near to $V = 4^m \cdot 99$, $(B-V) = 0^m \cdot 99$, $(U-B) = 0^m \cdot 80$, which are the values listed in the *Bright Star Catalogue*[90].

The writer's 1996 note[147] includes a fairly comprehensive but succinct summary of the salient literature on 56 UMa up to the time when it was written. Perhaps the most significant contribution to such literature since then has been the measurement by *Hipparcos*[29] (7, 1116) of the parallax, which has demonstrated that the absolute distance modulus is $5^m \cdot 89 \pm 0^m \cdot 21$, so the absolute magnitude (neglecting interstellar absorption, which is not expected to be significant at the star's Galactic latitude of 65°; in fact both Eggen[156] and Bersier[157] have specifically listed $E(B-V)$ as $0^m \cdot 00$) is $-0^m \cdot 9 \pm 0^m \cdot 2$. That is a higher luminosity than normal giants were previously supposed to have, and higher, in particular, than had been found for 56 UMa previously. The early spectroscopic-parallax work had yielded M_Vs of $-0 \cdot 1$[40] and $+0 \cdot 1$[41] at Mount Wilson and $0 \cdot 0$[45] at the Norman Lockyer Observatory, and the K-line method had produced values of $-0 \cdot 2$[83] and $0 \cdot 0$[82] — all very consistent with one another, but differing appreciably from the new trigonometric value. The reason probably lies in a systematic under-estimation of the luminosities of giant stars before many reliable parallaxes were known, so the calibrations upon which the former estimates were based were in error. Particularly informative are Figs. 1 and 3 of Keenan & Barnbaum[69], which plot absolute magnitudes against spectral types for giant stars that have adequately accurate parallaxes, first individually and then in the mean: stars of luminosity class III are now seen to be a full magnitude brighter generally than was formerly believed.

Other post-1996 papers have added a little to the previously existing literature on the elemental abundances in 56 UMa and on the radial velocity. Abundances up to and including the 'iron peak' have usually been found to be close to solar or slightly enhanced. The 'critical appraisal' by Taylor[158] in 1991 summarized the situation up till that time, finding a corrected mean of [Fe/H] = $+0 \cdot 09$, and subsequent investigations such as those of Liang *et al.*[159] and Lebre *et al.*[160] have agreed with it. The barium over-abundance first noticed by Williams[75], who obtained [Ba/H] = $+0 \cdot 47$ but admitted to uncertainties of about $\pm 0 \cdot 3$ and did not consider his result significant enough to warrant calling 56 UMa a barium star, has been corroborated at about that level by subsequent investigators, most recently by Liang *et al.*[159] who obtained [Ba/H] = $+0 \cdot 56$. Those authors also derived *via* stellar models a mass for the star — rather high for a normal giant — of about $3 \cdot 8 \pm 0 \cdot 2$ $M_{\odot}$; the same value was found by Gondoin[161].

Rotational and radial velocities have been given for 56 UMa, among many other stars, by de Medeiros & Mayor[92], who obtained a mean radial velocity of $-2 \cdot 36$ km s^{-1} from just two observations and a $v \sin i$ of $4 \cdot 0$ km s^{-1}. They listed the spectral type as G8 II, and although they flagged many of the stars in their listing as spectroscopic binaries even where their own observations (as in the present case) had shown no change, they omitted to flag 56 UMa as a binary. Exactly the same data, with the same omission, have very recently been re-stated by Lebre *et al.*[160] in what they describe as "an unprecedented list of precise stellar parameters", which includes other known binaries not flagged as such. Those authors were particularly interested in lithium abundances, and found no detectable lithium in 56 UMa; in a previous investigation of lithium lines Brown *et al.*[162] had found $\log \varepsilon(\mathrm{Li}) = +0 \cdot 7$: on the scale where $\log \varepsilon(\mathrm{H}) = 12$.

Radial velocities and orbit of 56 UMa

56 UMa seems not to have been a popular object with radial-velocity observers. Apart from the present author's observations[147], the only ones that have been published or are otherwise available appear to be those already mentioned — three made at Lick[6] in 1918–23 and two at OHP[92] in 1986/7 — plus one at Moscow[101] in 1987. Two of the Lick velocities are noted as half-weight, leaving only one 'good' observation. The two OHP measurements, published[92] initially as a mean value, were subsequently made available individually through the Centre de Données Stellaires, though with a mean then of $-1 \cdot 89$ instead of $-2 \cdot 36$ km s^{-1}, and they have been included in Table IV below with the writer's own observations made with the same instrument (the OHP *Coravel*[105]). The table lists 111 measurements made by the author; they extend over a total interval of 42 years, but only over the last 30 years have they been made in every year because the variability of 56 UMa's radial velocity was not recognized for a long time. The frequency of observations was modestly increased from 1999, taking its cue from the rate of variation. Of the new observations, 33 were obtained with the original spectrometer at Cambridge, 25 at OHP, six at the DAO, and 47 with the Cambridge *Coravel*. Not all of them are actually new in the sense of being unpublished, because those made up until early 1996 were listed in the earlier note[147], but the velocities attributed here to the OHP *Coravel* differ slightly from those previously

Table IV

Radial-velocity observations of 56 UMa

Except as noted, the sources of the observations are as follows:
1966–1989 — original Cambridge spectrometer (weighted 1/5 in orbital solution);
1990–1998 — Haute-Provence Coravel; 1999–2008 — Cambridge Coravel

Date (UT)			*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O − C)* *km s⁻¹*
1918	May	21·22*	21734·22	+4·9	$\bar{1}$·190	+1·3
1919	Jan.	30·54*	21988·54	+2·3	$\bar{1}$·206	−1·1
1923	Feb.	21·46*	23471·46	+5·4	$\bar{1}$·296	+3·1
1966	Apr.	13·91	39228·91	+2·0:	0·253	−0·8
		23·88	238·88	+2·3:	·254	−0·5
1971	Dec.	10·27	41295·27	+1·4	0·379	−0·1
1972	Mar.	29·94	41405·94	+1·7	0·385	+0·2
1979	May	19·87	44012·87	−0·2	0·544	−0·4
	Dec.	31·17	238·17	+0·3	·558	+0·2
1980	May	13·90	44372·90	−0·9	0·566	−0·9
		15·89	374·89	+0·8	·566	+0·8
1981	Feb.	2·10	44637·10	+0·1	0·582	+0·2
	Mar.	12·99	675·99	−1·0	·584	−0·9
	Apr.	17·91	711·91	+1·1:	·586	+1·3
	July	4·90	789·90	−0·2	·591	0·0
1982	Jan.	10·14	44979·14	+0·1	0·603	+0·4
	Mar.	2·04	45030·04	+0·7	·606	+1·0
	Apr.	14·92	073·92	−0·4	·608	−0·1
	May	11·89	100·89	−1·5	·610	−1·2
1983	Feb.	15·40†	45380·40	+0·2	0·627	+0·7
	June	12·91	497·91	0·0:	·634	+0·5
1984	Jan.	2·18	45701·18	+0·4	0·646	+1·0
	Apr.	2·95	792·95	−0·3	·652	+0·4
		15·91	805·91	−1·3	·653	−0·6
1985	Jan.	15·14	46080·14	−1·4	0·669	−0·6
	June	1·89	217·89	−0·6	·678	+0·3
1986	Jan.	25·08	46455·08	−0·2	0·692	+0·8
	Mar.	6·03	495·03	−1·2	·695	−0·2
		25·99‡M	514·99	−1·1	·696	−0·1
	May	13·89	563·89	−1·1	·699	−0·1
	Dec.	12·22	776·22	−1·3	·712	−0·2
1987	Jan.	31·12	46826·12	−1·9	0·715	−0·8
	Feb.	28·95‡	854·95	−0·9	·717	+0·3
	Mar.	10·88§	864·88	−1·4	·717	−0·2
		18·94	872·94	−2·7	·718	−1·5
		30·01‡M	884·01	−1·1	·718	+0·1
	June	24·91	970·91	−0·6	·724	+0·6
1988	Jan.	26·47†	47186·47	−1·5	0·737	−0·2
	Mar.	11·00‡	231·00	−1·4	·739	−0·1

TABLE IV (*continued*)

Date (UT)			MJD	Velocity km s^{-1}	Phase	(O – C) km s^{-1}
1988	June	12·91	47324·91	–2·2	0·745	–0·8
	Nov.	7·23‡	472·23	–1·4	·754	+0·1
1989	Jan.	18·13	47544·13	–2·0	0·758	–0·5
	Feb.	11·06	568·06	–1·7	·760	–0·2
	Mar.	17·98	602·98	–3·8	·762	–2·3
	Apr.	29·89‡	645·89	–1·5	·765	0·0
	Oct.	31·22‡	830·22	–1·9	·776	–0·3
1990	Jan.	31·05	47922·05	–1·2	0·781	+0·5
1991	Jan.	29·07	48285·07	–2·0	0·803	–0·2
	Dec.	19·14	609·14	–2·1	·823	–0·1
1992	Feb.	28·33†	48680·33	–2·2	0·827	–0·2
	Apr.	30·91	742·91	–1·9	·831	+0·1
1993	Feb.	15·06	49033·06	–1·9	0·849	+0·2
	Mar.	24·95	070·95	–1·9	·851	+0·2
	July	8·84	176·84	–2·0	·858	+0·2
	Dec.	30·16	351·16	–1·9	·868	+0·3
1994	Feb.	21·03	49404·03	–1·8	0·871	+0·4
	May	2·89	474·89	–2·3	·876	–0·1
	Dec.	14·18	700·18	–2·5	·889	–0·3
1995	Jan.	8·15	49725·15	–2·1	0·891	+0·1
	June	4·84	872·84	–2·2	·900	0·0
	Dec.	27·12	50078·12	–2·4	·912	–0·3
1996	Mar.	31·98	50173·98	–2·2	0·918	–0·2
	Nov.	21·22¶	408·22	–2·1	·932	–0·3
	Dec.	25·20	442·20	–1·7	·934	0·0
1997	Apr.	13·92¶	50551·92	–1·3	0·941	+0·3
	July	20·84	649·84	–1·5	·947	–0·2
	Dec.	22·17	804·17	–0·9	·956	0·0
1998	Apr.	28·90	50931·90	–0·2	0·964	+0·2
	July	8·85	51002·85	–0·2	·969	–0·1
1999	Apr.	2·30	51270·30	+1·2	0·985	0·0
	July	8·22	367·22	+2·0	·991	+0·2
	Nov.	4·54	486·54	+2·3	·998	–0·2
	Dec.	20·16	532·16	+2·7	1·001	–0·1
		27·30	539·30	+3·3	·001	+0·5
2000	Jan.	9·12	51552·12	+3·1	1·002	+0·2
	Feb.	11·09	585·09	+3·2	·004	+0·1
	Mar.	4·03	607·03	+3·2	·005	0·0
	May	25·89	689·89	+3·5	·010	–0·2
	July	7·89	732·89	+3·1	·013	–0·8
	Nov.	13·26	861·26	+4·4	·021	0·0
2001	Jan.	11·15	51920·15	+4·8	1·024	+0·1
	Mar.	12·05	980·05	+5·3	·028	+0·5
	May	12·05	52041·05	+5·2	·032	+0·2
	July	25·89	115·89	+5·0	·036	–0·2
	Nov.	1·23	214·23	+5·3	·042	0·0

TABLE IV (*concluded*)

Date (UT)			*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O − C)* *km s⁻¹*
2002	Jan.	1·17	52275·17	+4·9	1·046	−0·5
	Mar.	2·06	335·06	+5·5	·049	+0·1
	May	1·95	395·95	+5·6	·053	+0·1
	July	3·90	458·90	+5·4	·057	−0·1
	Oct.	19·22	566·22	+5·3	·064	−0·2
	Dec.	5·22	613·22	+5·4	·066	−0·1
2003	Jan.	7·19	52646·19	+5·6	1·068	+0·2
	Mar.	3·02	701·02	+5·6	·072	+0·2
	May	6·91	765·91	+5·5	·076	+0·1
	July	12·88	832·88	+5·3	·080	0·0
	Nov.	4·25	947·25	+5·2	·087	−0·1
2004	Jan.	9·17	53013·17	+5·2	1·091	0·0
	Mar.	17·11	081·11	+5·1	·095	0·0
	May	16·92	141·92	+5·4	·098	+0·3
	July	6·89	192·89	+5·0	·102	0·0
	Oct.	19·23	297·23	+4·9	·108	0·0
	Dec.	26·23	365·23	+4·9	·112	0·0
2005	Mar.	19·00	53448·00	+4·9	1·117	+0·1
	May	4·94	494·94	+4·9	·120	+0·2
	July	9·89	560·89	+4·5	·124	−0·2
	Nov.	5·24	679·24	+4·4	·131	−0·2
2006	Jan.	27·26	53762·26	+4·6	1·136	+0·1
	Mar.	22·96	816·96	+4·6	·140	+0·2
	May	10·96	865·96	+4·4	·142	0·0
	July	2·92	918·92	+4·3	·146	0·0
	Nov.	19·27	54058·27	+4·2	·154	0·0
2007	Jan.	14·20	54114·20	+4·0	1·158	−0·1
	Mar.	22·03	181·03	+4·0	·162	−0·1
	May	18·93	238·93	+4·2	·165	+0·2
	July	7·90	288·90	+3·9	·168	−0·1
	Nov.	9·24	413·24	+3·3	·176	−0·6
2008	Jan.	6·24	54471·24	+3·9	1·179	+0·1

* Lick observation[6], weight 0.
† Observed with DAO 48-inch telescope.
‡ Observed with Haute-Provence *Coravel*.
M Observation reported through the CDS[92].
§ Moscow observation[101], weight 0.
¶ Observed with Cambridge *Coravel*.

given owing to a change in the basis of their reductions[163]. The same change may account for the discrepancy noted above between the OHP measurements obtained from the CDS and their previously published mean.

The published velocities, and the writer's OHP ones, have been adjusted by the usual amount of +0·8 km s^{-1}, whereas the Cambridge *Coravel* measures have been adjusted by −0·2 km s^{-1} in the light of experience[109] with stars of similar colour. In the solution of the orbit, the *Coravel* observations were given unit weight, the 'original Cambridge' ones 1/5, and the Lick and Moscow ones were not included at all. Certain velocities in the table are followed by a colon to indicate that they were considered uncertain and were arbitrarily attributed half the weight that they would otherwise have received. The resulting orbit is illustrated in Fig. 6 and has the following elements:

$P = 16460 \pm 631$ days	$(T)_1 = \text{MJD } 51521 \pm 35$
$\gamma = +1{\cdot}01 \pm 0{\cdot}05$ km s^{-1}	$a_1 \sin i = 724 \pm 30$ Gm
$K = 3{\cdot}84 \pm 0{\cdot}04$ km s^{-1}	$f(m) = 0{\cdot}056 \pm 0{\cdot}004\, M_\odot$
$e = 0{\cdot}555 \pm 0{\cdot}016$	
$\omega = 286{\cdot}8 \pm 2{\cdot}0$ degrees	R.m.s. residual (wt. 1) = $0{\cdot}26$ km s^{-1}

Fig. 6 shows that the Lick observations (the open circles), made one cycle before the start of the Cambridge ones but not used in the calculation, fall at an acceptable phase in the orbit, but it would clearly be unwise to try to use them to arbitrate as to the exact period.

It will be seen that the orbit, whose period expressed in years is $45{\cdot}1 \pm 1{\cdot}7$, has been seen round very nearly a complete cycle, but owing to the sparse coverage in the early years the period is not determined very accurately. The precision of the early observations, also, is not as good as that of the more recent ones, although at the time it was the best that had ever been routinely available. That fact is readily illustrated by the statistical summary provided in the *Introduction* to the *Radial Velocity Catalogue*[91], which was published in 1953 but still represented the situation that obtained until the general adoption of the cross-correlation method of measurement in the 1980s. Of the 15 000-odd stars whose radial velocities were listed in the *Catalogue*, only about 10% were attributed quality class *a*, for which a 'mean probable error' of $0{\cdot}5$ km s^{-1} (standard error $0{\cdot}75$ km s^{-1}) was claimed — and that was for the mean of at least four observations made at quite high dispersions (exceptionally three, if they were made with the 114-inch camera

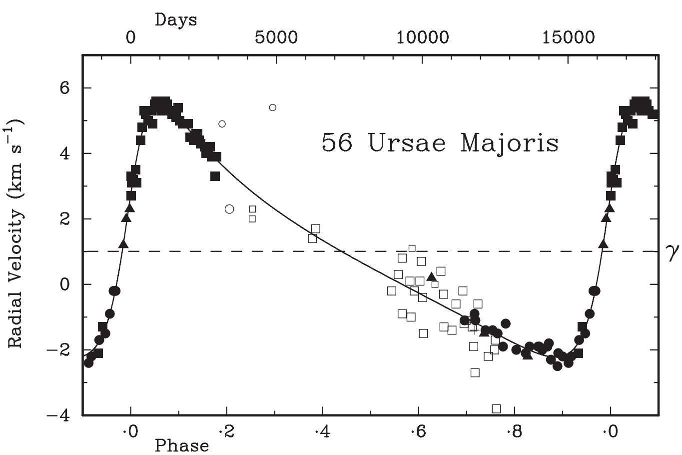

FIG. 6

The observed radial velocities of 56 Ursae Majoris plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The correspondence between plotting symbols and sources of observations is the same as in Fig. 1. All the points plotted with filled symbols received unit weight in the solution of the orbit; the open squares ('original Cambridge' observations) were weighted 1/5, apart from four velocities which were given half the normal weighting, marked with colons in Table IV and plotted with smaller symbols.

of the Mount Wilson 100-inch coudé). Almost all of the stars with *a*-quality velocities were naked-eye stars; scarcely any of them were as faint as 7^m. Even the original spectrometer, in the first few months of its operation, gave velocities[103] almost as good as the *a*-quality standard, and only three years later[164] that standard was surpassed ($\sigma \sim 0 \cdot 64$ km s^{-1}) for 7^m stars — and that was for individual observations, not the means of several or many as in the *Radial Velocity Catalogue*. Of course it could truthfully be represented that the current observations are not of state-of-the-art precision, either, and will be seen as reducing the precision with which the orbital elements are determinable after the *next* cycle of the variation of 56 UMa has been witnessed (if indeed posterity and/or its robotic servants bother to watch it); but clearly that type of problem is endemic in an age when technology is developing on a time-scale shorter than that of the natural phenomena to which it is being applied.

The mass function of 56 UMa is in the range usual for barium stars and is entirely consistent with the hypothesis that the secondary star should be a white dwarf. If indeed the mass of the primary is near to the $3 \cdot 8\ M_{\odot}$ suggested by Gondoin[161] and Liang *et al.*[159], then the minimum mass of the secondary must be about $1 \cdot 1\ M_{\odot}$, and since that is not very far below the Chandrasekhar[165] limit to the mass of white dwarfs it suggests that the orbital inclination must be high. Since $a_1 \sin i$ is nearly 5 AU and the mass ratio is presumably at least 3, the mean separation of the stars in the binary system must be something like 20 AU, which at the 150-pc distance of 56 UMa would subtend an angle of $0'' \cdot 13$. Since we see the orbit nearly end-on, however, ($\omega \sim 270°$ to a zero-order approximation), our view of the orbit on the sky is of its width rather than its length, so its projected size is reduced by nearly the factor $(1 - e^2)^{1/2}$, about $0 \cdot 83$. Also, we do not benefit, as we would if the orbit were presented more cooperatively, from the increased separation (by the factor $(1 + e)$) at apastron, because from our perspective the maximum angular separation does not occur at that phase. All the same, the separation can be expected to reach above $0'' \cdot 1$, so we can conclude that it will be the enormous magnitude difference between the components, rather than their angular proximity, that will make it difficult to resolve them on the sky.

HR 4593 (HD 104438)

HR 4593 is a $5^1/2^m$ late-type giant star in the vicinity of the North Galactic Pole. It accordingly featured in the Cambridge project to measure the radial velocities of all the late-type *Henry Draper Catalogue* stars within 15° of the Galactic Pole; that programme was combined with a complementary photometric one undertaken by Yoss, and its principal results were published[166] some ten years ago. In respect of the object of interest, the results were the discovery of its spectroscopic-binary nature* and the listing of its γ-velocity as $+25 \cdot 6 \pm 0 \cdot 6$ km s^{-1}, the magnitudes $V = 5^m \cdot 57$ and $(B - V) = 1^m \cdot 02$, and the deductions from *DDO*-style[168] photometry that the spectral type is K0 III, the absolute magnitude $+1^m \cdot 2$, and [Fe/H] $-0 \cdot 16$. The star is bright enough to be shown in *Norton*[169]; it is far from any really bright stars and its position can best be described as being just halfway between β Leo and δ UMa.

*In the *Bright Star Catalogue*[90] the radial velocity of HR 4593 is suffixed with 'V?', meaning 'may be variable', and *Simbad* records a paper[167] in *PDAO*, **2**, 189, 1921, as saying that the velocity is variable. Actually the paper, whose date is really 1923, makes no such claim and would not support one, but it has a misprint: for HR 4593, there are two velocities, given as $+27 \cdot 6$ and $+38 \cdot 0$ [*sic*] km s^{-1} whose mean is given (no doubt correctly) as $+27 \cdot 8$, showing that the second entry was intended to be $+28 \cdot 0$.

Schild[170] gave the broad-band magnitudes of HR 4593 as $V = 5^m \cdot 59$, $(B-V) = 1^m \cdot 01$, $(U-B) = 0^m \cdot 79$; Häggkvist & Oja[171] found $V = 5^m \cdot 58$, $(B-V) = 1^m \cdot 018$. The *Draper Catalogue*[37] had the first classification of HR 4593, as type I (the letter, between G and K, not to be read as a Roman 'one'!); in the *Henry Draper Catalogue* the type appears as K0. In the early flurry of spectroscopic-parallax determinations, at Mount Wilson[40] the star (identified as Boss[172] 3141) was classified in the normal way as G9 and 'measured' (the term is explained in the section on β LMi above) as G8, and its absolute magnitude was given as $+0^m \cdot 9$. At the DAO, Young & Harper[42] gave it as G8, with absolute magnitudes that they determined independently as $+1^m \cdot 3$ and $+1^m \cdot 5$, respectively, and at the Norman Lockyer Observatory Rimmer[46] found it to be $+0^m \cdot 7$. A decade later, a new effort[41] at Mount Wilson listed it as K1, $+0^m \cdot 5$. The MKK system[43] had not yet been described when Keenan[173], in 1940, gave the first two-dimensional classification, as K0 III, with $M_V = 0^m \cdot 0$. The same type of K0 III was found both by Upgren[174] and by Schild[170]. Other estimates of the absolute magnitude have been published by Hansen & Kjærgaard[57] ($+1^m \cdot 0$), da Silva & Grenier[175] ($+1^m \cdot 1$, corrected, seemingly before it was even published, to $+0^m \cdot 6$ or $+0^m \cdot 1$ under different assumptions[176]), Hansen & Radford[177] ($+1^m \cdot 5$), and Eggen[178] ($+0^m \cdot 8$, apparently from Geneva photometry[179] transformed to *DDO*). The *Hipparcos*[29] (7, 1178) parallax translates to an absolute distance modulus of $5^m \cdot 23 \pm 0^m \cdot 19$, and so fixes the absolute magnitude with that accuracy at $+0^m \cdot 35$, interstellar absorption being supposed negligible. The lower luminosities usually found previously are probably due again more to calibration errors than to 'accidental' ones. Some of the narrow-band photometric investigations, like the one[166] with which the author was associated, have allowed metal abundances to be estimated, and there has been good agreement that the abundances in HR 4593 are just slightly less than solar. An observation of the lithium line by Brown *et al.*[162] yielded a result of $\varepsilon(\mathrm{Li}) = 0 \cdot 0$ on the scale with $\varepsilon(\mathrm{H}) = 12$.

Radial velocities and orbit of HR 4593

HR 4593 is just too faint to feature in the Lick catalogue[6] of radial velocities, whose nominal cut-off was $5^m \cdot 5$. The velocity was first measured from three plates taken in 1918/19 with the Mount Wilson 60-inch Cassegrain at 36 Å mm^{-1} at Hγ by Adams *et al.* for their spectroscopic-parallax work[40]; it was published[134] simply as an undated mean value of $+31 \cdot 1 \pm 0 \cdot 7$ ('probable error') km s^{-1}. Abt[142] much later provided the individual dates and velocities. Published at almost the same time was the paper[167] referred to in the footnote on the last page, giving velocities from two plates both taken in 1922 March. No more measurements of HR 4593 appear to have been published until in 1986 Barnes, Moffett & Slovak[180] put the star forward as a velocity standard (with others equally doubtful) on the basis of six measurements made photoelectrically at the 82-inch McDonald coudé with an instrument that gave an r.m.s. accuracy of $2 \cdot 8$ km s^{-1} per observation; the mean value was $+24 \cdot 2 \pm 0 \cdot 9$ km s^{-1}, but the separate observations are not listed and so cannot be included in the discussion here — not that the star promises to be a good velocity standard, in the sense of having a known and constant velocity, in any case. Fehrenbach *et al.*[181], on the other hand, listed individually four velocities obtained for HR 4593 by their objective-prism method that gives still larger errors. De Medeiros & Mayor[92] referred to two measurements (subsequently made available through the Centre de Données Stellaires) made with the OHP *Coravel*; they gave a rotational velocity of $1 \cdot 1 \pm 1 \cdot 0$ km s^{-1}.

The first measurement with the Cambridge spectrometer, +25·0 km s^{-1}, was made by G. A. Radford (then a student whose Ph.D. thesis was concerned with stars in the North Galactic Pole field) in 1973. Subsequent observations did not agree very well with it, but could not be said to be definitely discordant, either with it or with one another, until the one of 1984 April 30 gave a result of +18·4 km s^{-1}. Unfortunately (and it must be confessed that a drawback of the original spectrometer[103] was that the chart record that it drew had to be read by eye and the result reduced by hand) the observations made on that night were not reduced until 1987 December 4 — the observer had got rather behindhand with his reductions, despite the awful example presented by Challis[182]! — whereupon a fresh observation was taken at once, on the night of 1987 December 7, and showed that the velocity had returned to about its 1973 value. The star was then promptly transferred to the binary programme and has been observed systematically ever since, but a consequence of the delayed discovery of the variation is that the descending node of the orbit and the early part of the rise in velocity were not observed as well as could be wished. It was fortunate that the rise was discovered when it was, when it was less than half completed, because the rest of it could be carefully followed. It is much the most significant phase of the orbit from the observational point of view: the orbit is very eccentric, and the rate of change for much of the rise is about 5 km s^{-1} per year, whereas the fall takes place at a leisurely and nearly constant 0·4 km s^{-1} per year. The total number of measurements made on the Cambridge programme for HR 4593 stands at 63: 21 of them were made with the original spectrometer, 19 at OHP, 18 with the Cambridge *Coravel*, two at the DAO, two at ESO, and one at Palomar. All of the available observations are set out in Table V. The OHP and ESO *Coravel* measurements have been increased by 0·8 km s^{-1} as usual, and the Cambridge *Coravel* ones decreased by 0·2 km s^{-1}. The Mount Wilson[134,142] and DAO[167] photographic observations are increased by 0·8 km s^{-1} plus the corrections noted for those sources (+0·5 and +1·0 km s^{-1}, respectively) on p. vi of the *Radial Velocity Catalogue*[91].

TABLE V

Radial-velocity observations of HR 4593

Except as noted, the sources of the observations are as follows:
1973–1991 — original Cambridge spectrometer (weighted 1/4 in orbital solution);
1992–1998 — Haute-Provence Coravel; 1999–2008 — Cambridge Coravel

Date (UT)			MJD	Velocity km s^{-1}	Phase	(O – C) km s^{-1}
1918	June	24·16*	21768·16	+33·3	$\bar{1}$·045	+1·2
	Dec.	14·55*	941·55	32·8	·058	+0·4
1919	Jan.	14·52*	21972·52	29·6	$\bar{1}$·060	–2·9
1922	Mar.	7·31†	23120·31	29·4	$\bar{1}$·148	–1·6
		15·38†	128·38	29·8	·149	–1·2
1973	Apr.	24·96R	41796·96	25·0	0·578	+0·8
1975	Feb.	25·01	42468·01	22·5	0·630	–1·0
1977	Apr.	2·00	43235·00	24·2	0·688	+1·5
1978	Mar.	30·96	43597·96	22·4	0·716	+0·1
	May	23·24‡	651·24	+22·5	·720	+0·3

TABLE V (*continued*)

Date (UT)			*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O − C)* *km s⁻¹*
1979	May	14·96	44007·96	+22·0	0·748	+0·2
1980	Jan.	13·12	44251·12	21·4	0·766	−0·1
1984	Jan.	29·13[§]	45728·13	24·0	0·879	+4·4
	Apr.	30·85	820·85	18·6	·886	−0·9
1985	Dec.	16·14[§]	46415·14	30·0	0·932	+10·6
1986	Feb.	12·09[§]	46473·09	18·0	0·936	−1·5
	Mar.	14·98[§]	503·98	13·0	·939	−6·5
	Apr.	4·87[¶]	524·87	19·5	·940	−0·1
1987	Mar.	1·94[¶]	46855·94	21·0	0·966	−0·1
	Dec.	8·24	47137·24	24·4	·987	+0·3
1988	Jan.	26·48[‖]	47186·48	24·7	0·991	−0·1
	Mar.	11·02[¶]	231·02	25·6	·994	+0·2
		16·98[¶]	236·98	25·5	·995	0·0
	Apr.	12·89	263·89	25·8	·997	−0·1
	May	6·87	287·87	26·3	·999	0·0
	June	5·92	317·92	27·5	1·001	+0·8
		6·88[‡M]	318·88	27·0	·001	+0·3
	Nov.	5·20[¶]	470·20	28·6	·013	−0·3
1989	Jan.	18·11	47544·11	29·7	1·018	0·0
	Feb.	5·14[‡M]	562·14	29·5	·020	−0·4
		23·25[**]	580·25	29·7	·021	−0·4
	Mar.	12·02	597·02	30·3	·022	0·0
	Apr.	28·01[¶]	644·01	31·0	·026	+0·3
	May	19·90	665·90	32·2	·028	+1·3
	June	18·92	695·92	31·9	·030	+0·8
	Oct.	31·21[¶]	830·21	31·5	·040	−0·4
1990	Jan.	14·11	47905·11	33·1	1·046	+1·0
	Feb.	12·27[**]	934·27	32·0	·048	−0·2
	Mar.	13·38[‖]	963·38	32·1	·050	−0·2
		26·90	976·90	32·6	·051	+0·3
	Apr.	29·02	48010·02	32·5	·054	+0·1
	May	26·90	037·90	32·0	·056	−0·4
	Dec.	4·24	229·24	33·0	·071	+0·5
1991	Jan.	26·08[¶]	48282·08	32·2	1·075	−0·2
	Feb.	6·09[¶]	293·09	33·1	·076	+0·7
	June	10·90	417·90	32·1	·085	−0·2
	Dec.	19·15[¶]	609·15	31·8	·100	−0·3
1992	Apr.	29·87	48741·87	32·5	1·110	+0·6
1993	Feb.	16·08	49034·08	31·4	1·132	0·0
1994	Jan.	5·22	49357·22	31·1	1·157	+0·3
	Aug.	5·84	569·84	30·0	·173	−0·5
1995	Jan.	8·13	49725·13	30·3	1·185	+0·1
	June	5·91	873·91	29·9	·197	−0·1
1996	Apr.	3·00	50176·00	+29·4	1·220	−0·1

TABLE V (*concluded*)

Date (UT)			*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1997	May	12·92††	50580·92	+29·0	1·251	+0·1
1998	May	2·91	50935·91	28·3	1·278	−0·2
	July	11·86	51005·86	28·7	·283	+0·3
1999	Dec.	17·22	51529·22	27·8	1·323	+0·1
2000	May	13·95	51677·95	27·2	1·335	−0·4
	Dec.	14·23	892·23	26·9	·351	−0·4
2001	June	7·97	52067·97	27·2	1·365	+0·1
2002	Jan.	2·18	52276·18	26·0	1·381	−0·9
	June	3·93	428·93	27·0	·392	+0·3
2003	Jan.	28·17	52667·17	26·6	1·411	+0·1
	June	10·91	800·91	26·4	·421	+0·1
2004	Mar.	30·99	53094·99	26·0	1·443	0·0
	June	5·92	161·92	26·4	·448	+0·5
2005	Jan.	9·16	53379·16	25·6	1·465	−0·1
	May	30·97	520·97	25·5	·476	−0·1
2006	Jan.	29·16	53764·16	25·3	1·495	0·0
	June	2·97	888·97	25·1	·504	−0·1
	Dec.	9·30	54078·30	25·0	·519	0·0
2007	May	29·97	54249·97	24·8	1·532	0·0
2008	Jan.	6·25	54471·25	+24·6	1·549	0·0

* Mount Wilson[134,142] observation, weight 0·025.
† DAO[167] observation, weight 0·025.
R Observed by Dr. G. A. Radford.
‡ Observed with Palomar 200-inch telescope.
§ Objective-prism[181] observation, weight 0.
¶ Observed with Haute-Provence *Coravel*.
‖ Observed with DAO 48-inch telescope.
M Observation reported through the CDS[92].
** Observed with ESO *Coravel*.
†† Observed with Cambridge *Coravel*.

Initial orbital solutions utilizing the author's own observations (plus the two extra OHP ones to which attention was drawn by de Medeiros & Mayor[92]) showed that all of them could be attributed the same weight apart from those made with the original spectrometer, which merited weight 1/4. The period was found to be 12 737 ± 286 days. In fact the orbit looked extremely similar to the one that was deduced and illustrated at a conference[110] 16 years ago, at a time when the observations had only just reached as far as the ascending node (the velocity maximum). The period that was then suggested was 12 000 days, and it was correctly noted that "the orbit cannot really be well determined until after the next periastron passage, which will involve a wait of about thirty years." In that quotation, "wait" is clearly a euphemism for "work"! — but at least we can remark that half the time has now elapsed and *some* progress has been made.

In the preliminary solution just described, the old Mount Wilson radial velocities, made two cycles previously, fall just about at the level of the maximum but slightly too early; owing to the relatively rapid rate of change at that phase their residuals can be greatly reduced by making an acceptably small change (of the order of one standard error) to the initially determined period. That reduces the residuals of the early DAO measurements too. Quantitatively, the amount of the change depends upon the weighting attributed to the old velocities. There are too few of them for it to be safe to assess their weighting upon the evidence of the residuals from this one orbit; reliance is therefore placed upon experience, which suggests that velocities from both the old sources could be attributed uncertainties of about 2 km s^{-1} per observation, implying in the present context weightings of 0·025. The orbit was therefore re-computed with the old observations included at that weighting. The new period of 13 060 days was just over 1σ longer than the preliminary one, and none of the other elements was changed by as much as its standard error; the result was considered satisfactory and was adopted as the best characterization that can presently be made of the orbit. The elements are:

P = 13060 ± 187 days	$(T)_1$ = MJD 47305 ± 24
γ = +25·51 ± 0·07 km s^{-1}	$a_1 \sin i$ = 960 ± 22 Gm
K = 6·59 ± 0·11 km s^{-1}	$f(m)$ = 0·207 ± 0·012 $M_\odot$
e = 0·586 ± 0·009	
ω = 275·6 ± 1·6 degrees	R.m.s. residual (wt. 1) = 0·30 km s^{-1}

The orbit is illustrated in Fig. 7. Among the elements, one that calls for attention (apart from the long period) is the mass function, which mandates a minimum mass of 1·3 $M_\odot$ for the secondary if we suppose the primary to be 2 $M_\odot$;

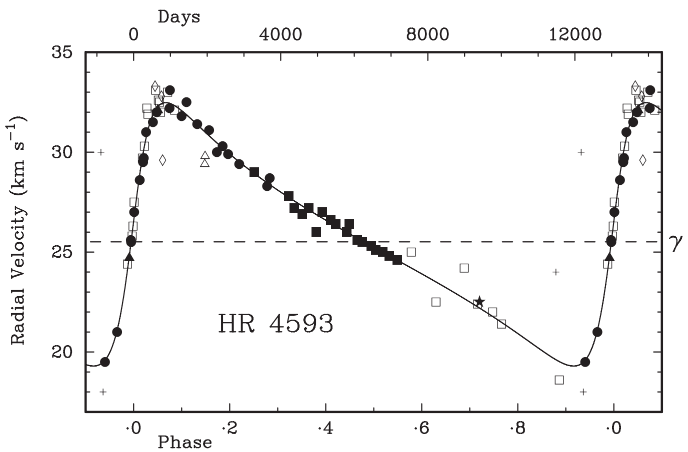

FIG. 7

The observed radial velocities of HR 4593 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The same conventions as in Figs. 1 and 6 regarding plotting symbols are maintained, except that here the open diamonds represent the early Mount Wilson observations[134,142] (weighted 0·025, like the early DAO ones plotted by open triangles) and the small plusses indicate the zero-weighted objective-prism[181] measurements, one of which would fall far below the bottom of the diagram. Two velocities from the ESO *Coravel* are treated as if they were OHP ones.

even a 1-$M_\odot$ primary would still have to have a secondary of at least 0·9 $M_\odot$. If, in the light of probability and for the sake of discussion, we adopt the former case, the secondary is almost too massive to be a white dwarf and would have to be a main-sequence star no later than about F5 and thus no fainter than about $M_V = 3^m \cdot 4$, *i.e.,* three magnitudes fainter than the primary. The Copenhagen-style[54] photometry performed by Hansen & Radford[177] yielded a res(k) of $0^m \cdot 025$, not up to the official level ($0^m \cdot 04$) of significance for duplicity but nevertheless larger than the values for the vast majority of stars. The $(U-B)$ colour index is also quite blue in its relationship to the $(B-V)$ one, although the combination is not unexampled. Experience of analogous systems, and in particular of 73 Leo[183], which almost certainly consists of a late-type giant plus a main-sequence star in the F0–F5 range, has brought home the difficulty of recognizing the compositeness of such objects spectroscopically; the mass function of 73 Leo is the same as that of HR 4593, within their uncertainties. No great hope is felt, therefore, of identifying the secondary star spectroscopically. On the other hand, the long orbital period and concomitant large orbit imply that the angular separation must not be very small. On the assumption (which is not very critical) that the secondary has 2/3 of the mass of the primary, the minimum mean separation of the pair is 2½ times the $a_1 \sin i$ value in the elements above, or about 16 AU, which at the 111-pc distance found by *Hipparcos* would subtend an angle of some $0'' \cdot 14$ if viewed 'square-on'. In such an eccentric orbit as that of HR 4593, the system spends most of its time near apastron, where we are about *now* and where the linear separation is increased by the factor $(1 + e)$; but because $\omega \sim 270°$ we see the orbit almost end-on, and so get a view only of its minor axis, and if the inclination is close to 90° the angular separation at apastron could be very small.

39 Cygni (HR 7806, HD 194317)

39 Cygni is a 4½-magnitude star near the southern border of Cygnus; in connection with the long-period nature of the stars treated in this paper, it is of interest to remark that 39 Cyg is about 5° south-preceding ε Cyg, for which the author[21] risked proposing in 1994, on the basis of dangerously skimpy information, an orbit with a period of 55 years. 39 Cyg was also mentioned in Paper 103[184] of this series as being only half a degree from one of the two stars whose orbits were given in that paper, HD 193891; by a remarkable coincidence, the other subject of the paper, HD 19942, was pointed out as being only a degree or so from κ Per, whose orbit is given above.

When the radial-velocity spectrometer[107] that J. E. Gunn and the writer made for the coudé focus of the 200-inch Palomar reflector was new, there was an evening that was overcast but not quite opaquely so: no stars could be seen with the naked eye, but they were visible all right through the big one, so we amused ourselves observing bright stars, whose exact then-current positions were immediately available from the *Ephemeris*[185], just to see what the traces looked like on stars of different types. One of the stars thus observed was 39 Cyg. Shortly afterwards the same set of stars was re-observed with the Cambridge spectrometer[103] for comparison purposes. Two of the 32 stars observed in that way were not known to be binaries but gave velocities distinctly different from those found[6] at the Lick Observatory about half a century previously. Those stars were 39 Cyg and 23 Vul, which gave discrepancies from Lick of $+4 \cdot 1$ and $-2 \cdot 2$ km s^{-1}, respectively. Although really we knew that they must be binaries, in writing the work up[107] we refrained from making such a claim, because at that time most astronomers seemed not to *believe* photoelectrically measured radial velocities and it did not seem necessary to stir up extra trouble for ourselves over a matter that was not germane to what we were discussing. We therefore contented ourselves by remarking that those two stars exhibited "uncharacteristically large" discrepancies, and by omitting them from the calculation of the r.m.s. discordances on grounds that were merely statistical. There was a large discrepancy (+5·1 km s^{-1}) also in the case of ε Aql, but that star had already been identified[141,91] as a spectroscopic binary although six Lick measurements[6] provided no confirmation.

It later occurred to the writer that the discrepant stars ought really to have their velocities monitored and their orbits determined, so in 1975 he placed 39 Cyg and ε Aql on the Cambridge observing programme. The 23 Vul discrepancy was so small that it promised to be too difficult a task (and could well take too long) to obtain its orbit, so *that* star was not observed; some satisfaction was gained in due course, however, from the demonstration[186] by speckle interferometry in 1983 that it is a close 'visual' binary, after it had already been suspected[187] of having a composite spectrum; subsequently a still closer companion, still more likely to produce macroscopic radial-velocity variations in its primary, was discovered[188]. The orbit of ε Aql was readily determined and was written up as early as Paper 44[189] of this series of papers. It may be mentioned that there have come to light just two other cases of unexplained discrepancies between Lick velocities and the writer's; the stars concerned are HR 6388[190] and χ Gem[191]. In an effort to cast light on the case of HR 6388, that star's orbit was recently discussed anew[104] on the basis of fresh observations after a lapse of more than a quarter of a century, but no (further) change in velocity could be documented; reprehensibly, the χ Gem case has not been followed up.

The *UBV* magnitudes of 39 Cyg have been measured by Miss Roman[192] (her list of 'high-velocity stars', in which 39 Cyg seems to sit rather uneasily since it is not such a star), Argue[152], Häggkvist & Oja[25], and Johnson *et al.*[26], with results near to $V = 4^{m}\cdot43$, $(B-V) = 1^{m}\cdot33$, $(U-B) = 1^{m}\cdot50$; Miss Roman made the star slightly brighter than the other observers, increasingly so towards shorter wavelengths, such that her U measurement is as much as $0^{m}\cdot09$ brighter than either of the other two[152,26] (Häggkvist & Oja measured only V and B).

The spectral classification and luminosity estimates of 39 Cyg were made by many of the same authors as those of the other stars described here. The initial *Draper Catalogue*[37] class was 'H?'; the *HD* type is K2. At Mount Wilson it was first[40] given an 'estimated' type of K5 but 'measured' as K2, with $M_V = +0^{m}\cdot5$; in the later revision[41] it was called K5, $+0^{m}\cdot7$. At the DAO, Young & Harper[42] called it K5 and assessed its absolute magnitude at $+0^{m}\cdot4$ and $+0^{m}\cdot2$, respectively, while at the Norman Lockyer Observatory Rimmer[45] found it to be K2, $+0^{m}\cdot5$. The first MK classification appears to be that by Miss Roman[60], K3 III, which was repeatedly reinforced by Keenan and his collaborators[62–64] until 1988, when it was slightly revised[66,67] to K2·5 III Fe −0·5. Photometric and spectroscopic estimates of the absolute magnitude of 39 Cyg include $+0^{m}\cdot2$ by Hansen & Kjærgaard[57], $+0^{m}\cdot17$ by Gottlieb & Bell[77], $-0^{m}\cdot26$ by Egret, Keenan & Heck[70], $+0^{m}\cdot05$ by McWilliam[79], and $+1^{m}\cdot09$ (from *DDO* photometry) by Flynn & Mermilliod[80]; Wilson[82] obtained $+0^{m}\cdot4$ by his *K*-line method. All of the estimates apart from the *DDO* one are reasonably corroborated by the trigonometrical one from *Hipparcos*[29] (**9**, 2020), which obtained a parallax that corresponds to $(m - M) = 4^{m}\cdot47 \pm 0^{m}\cdot10$, and thus, with the apparent magnitude $4^{m}\cdot43$, to an absolute magnitude very close to zero. Several of the same authors also estimated the metal abundances [M/H] in 39 Cyg; they too were always close to zero, except that of Gottlieb & Bell[77], whose whole scale of abundances seemed to be much too high, as they themselves recognized. Boyarchuk *et al.* devoted a whole (if short) paper[193] to 39 Cyg and its elemental abundances, which they again found to be close to solar. Lithium is, of course, always a special case. Brown *et al.*[162] found ε(Li) to be $+0 \cdot 6$; Boyarchuk *et al.*[194] (not exactly the same syndicate as in ref. 193) initially obtained $\varepsilon(\text{Li}) = +0 \cdot 7$, but when they had taken careful account of non-LTE effects their result was increased to $+1 \cdot 15$.

Radial velocities and orbit of 39 Cygni

As in the cases of so many bright stars, the earliest radial-velocity measurements were made at Lick and Bonn. Six plates were taken at Lick[6] in 1906–09 and three at Bonn[141] in 1908–11. All were in good accord with one another; each series gave the same mean, $-13 \cdot 5$ km s^{-1}, about which the r.m.s. scatter was $1 \cdot 2$ km s^{-1} for the whole ensemble. Then there was a long interval lacking observations before two more were made at Lick, bringing the tally of the early measurements there to eight. The last two were in 1924, and did not give exactly the same results as before: they were $-17 \cdot 0$ and $-16 \cdot 2$ km s^{-1}, which can be seen to differ from the earlier set by much more than those measures disagreed amongst themselves. Evidently that fact was not lost upon the Lick astronomers, because the last two plates were measured a second time by a different measurer ('J', whose identity is not listed among those of the 58 measurers whose names and acronyms are set out in the table on p. xlii of the catalogue[6]). In each case the second measurement supported the first, but (even so) both plates were noted as being given half-weight in the formation of the mean of all eight; it seems possible that that was done because they did not agree well with the majority, although that is not admitted as a normal reason for half-weighting (ref. 6, p. xliii, paragraph (*h*)), because if anything had been objectively wrong with them they would hardly have been considered worth re-measuring. We can see now that the reason for the disagreement was an actual change in the star's velocity, but the change was sufficiently slight that Campbell & Moore, the compilers of the catalogue, would naturally have hesitated to assert that the star was a binary on the basis of such slender evidence. That evidence — for an unusually low velocity around 1924 — was greatly reinforced ten years later, when Harper[96] published two mutually accordant velocities obtained at the DAO in 1923 and giving a mean of $-19 \cdot 4$ km s^{-1}. One velocity, of $-14 \cdot 1$ km s^{-1}, was published from the Crimea by Albitzky[195]; no date was given, so it cannot be taken into account here. De Medeiros & Mayor[92] referred to eight measurements of 39 Cyg, obtained with the OHP *Coravel* and later made available individually through the Centre de Données Stellaires. As well as measuring the radial velocity, they gave the rotational velocity as $1 \cdot 2 \pm 1 \cdot 0$ km s^{-1}; Dempsey *et al.*[196] listed a rotation of 10 km s^{-1} on the basis of observations of the Ca II triplet region in the near-infrared, but they must have been mistaken. No fewer than sixteen radial velocities of 39 Cyg were obtained by Beavers & Eitter[99] with the Ames spectrometer[100] in 1976–83. All the published velocities are included in Table VI, in which they have been increased by $0 \cdot 8$ km s^{-1}; the DAO ones[96] have been additionally corrected by the $+0 \cdot 5$ km s^{-1} advised in Table 3 on p. vi of the *Radial Velocity Catalogue*[91], but the suggested correction to the Bonn[141] velocities has not been applied.

It has been explained above how 39 Cyg, having first been observed casually, was placed on the Cambridge spectroscopic-binary programme owing to the 4-km s^{-1} discordance between the velocity observed at Palomar in 1971 and the

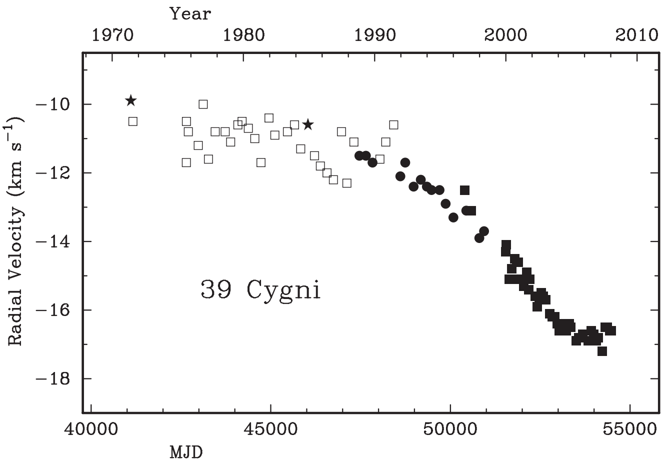

FIG. 8

The author's radial-velocity measurements of 39 Cygni, plotted directly against time. As before, the open squares indicate measurements made with the original Cambridge spectrometer, and filled circles, squares, and stars represent OHP *Coravel*, Cambridge *Coravel*, and Palomar, respectively.

TABLE VI

Radial-velocity observations of 39 Cygni

Except as noted, the sources of the observations are as follows:
1971–1991 — original Cambridge spectrometer (weighted 1/5 in orbital solution);
1992–1998 — Haute-Provence Coravel; 1999–2008 — Cambridge Coravel

Date (UT)			*MJD*	*Velocity km s^{-1}*	*Phase*	*(O − C) km s^{-1}*
1906	July	6·36★	17397·36	−11·5	$\bar{1}$·837	+0·5
1907	July	16·42★	17772·42	−13·8	$\bar{1}$·849	−1·6
1908	Oct.	29·19★	18243·19	−12·5	$\bar{1}$·864	0·0
	Nov.	7·74†	252·74	−11·8	·864	+0·7
1909	July	7·48★	18494·48	−13·2	$\bar{1}$·872	−0·5
		14·41★	501·41	−13·4	·872	−0·7
		22·29★	509·29	−11·7	·872	+1·0
1910	Aug.	19·96†	18902·96	−14·9	$\bar{1}$·885	−1·9
1911	Oct.	6·83†	19315·83	−11·4	$\bar{1}$·898	+2·0
1923	Aug.	30·26‡	23661·26	−17·6	0·037	−1·5
	Sept.	14·32‡	676·32	−18·6	·038	−2·5
1924	July	15·45★	23981·45	−16·2	0·047	−0·5
	Nov.	26·15★	24115·15	−15·2	·052	+0·3
1971	June	2·49§	41104·49	−9·9	0·594	+0·5
	July	29·99	161·99	−10·5	·596	−0·1

TABLE VI (*continued*)

Date (*UT*)			*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	($O-C$) *km s*$^{-1}$
1975	Aug.	25·99	42649·99	−11·7	0·644	−1·2
		26·94	650·94	−10·5	·644	0·0
	Oct.	18·85	703·85	−10·8	·646	−0·3
1976	July	11·28¶	42970·28	−9·3:	0·654	+1·3
		22·05	981·05	−11·2	·654	−0·6
	Aug.	5·22¶	995·22	−10·9	·655	−0·3
	Sept.	22·15¶	43043·15	−8·9	·656	+1·7
	Dec.	5·74	117·74	−10·0	·659	+0·6
1977	May	1·11	43264·11	−11·6	0·664	−1·0
	Aug.	5·93M	360·93	−10·3	·667	+0·3
		25·19¶	380·19	−7·4:	·667	+3·2
	Sept.	10·22¶	396·22	−5·5:	·668	+5·1
		12·84M	398·84	−9·4	·668	+1·2
		13·86M	399·86	−10·4	·668	+0·2
		14·83M	400·83	−10·9	·668	−0·3
	Oct.	14·11¶	430·11	−11·2:	·669	−0·6
	Nov.	1·76M	448·76	−10·5	·669	+0·1
		3·83	450·83	−10·8	·669	−0·2
1978	July	23·99M	43712·99	−10·9	0·678	−0·2
		27·25¶	716·25	−12·4	·678	−1·7
	Aug.	9·94	729·94	−10·8	·678	−0·1
1979	Jan.	9·72	43882·72	−11·1	0·683	−0·4
	July	30·00	44084·00	−10·6	·690	+0·1
	Nov.	24·76	201·76	−10·5	·693	+0·2
1980	May	15·12	44374·12	−10·7	0·699	0·0
	Aug.	15·21¶	466·21	−9·6	·702	+1·2
	Nov.	12·76	555·76	−11·0	·705	−0·2
1981	May	3·14	44727·14	−11·7	0·710	−0·9
	Dec.	12·72	950·72	−10·4	·717	+0·4
1982	May	24·06	45113·06	−10·9	0·723	0·0
	June	17·37¶	137·37	−11·1	·723	−0·2
	July	13·28¶	163·28	−12·0	·724	−1·1
	Aug.	17·21¶	198·21	−11·4	·725	−0·5
1983	May	10·14	45464·14	−10·8	0·734	+0·1
	July	11·31¶	526·31	−10·3	·736	+0·6
		19·29¶	534·29	−10·1	·736	+0·8
		26·28¶	541·28	−9·0	·736	+1·9
	Sept.	8·19¶	585·19	−10·9	·738	+0·1
		11·18¶	588·18	−11·7	·738	−0·7
	Nov.	23·76	661·76	−10·6	·740	+0·4
1984	May	13·11	45833·11	−11·3	0·746	−0·3
	Nov.	30·12§	46034·12	−10·6	·752	+0·5
1985	May	31·09	46216·09	−11·5	0·758	−0·4
	Nov.	11·78	380·78	−11·8	·763	−0·7
1986	May	14·12	46564·12	−12·0	0·769	−0·8
	Nov.	8·75	742·75	−12·2	·775	−1·0
1987	June	23·04	46969·04	−10·8	0·782	+0·5
	Aug.	14·99M	47021·99	−11·0	·784	+0·3
	Nov.	20·76	119·76	−12·3	·787	−0·9

TABLE VI (*continued*)

Date (UT)			*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
1988	June	3·09	47315·09	–11·1	0·793	+0·3
	Aug.	7·96[M]	380·96	–11·6	·795	–0·2
	Nov.	2·85‖	467·85	–11·5	·798	0·0
1989	May	2·16‖	47648·16	–11·5	0·804	0·0
	Nov.	1·80‖	831·80	–11·7	·809	–0·1
1990	May	27·11	48038·11	–11·6	0·816	+0·1
	Nov.	5·80	200·80	–11·1	·821	+0·7
1991	June	14·07	48421·07	–10·6	0·828	+1·3
	Dec.	16·73‖	606·73	–12·1	·834	–0·1
1992	May	1·16	48743·16	–11·7	0·839	+0·3
	Dec.	20·75	976·75	–12·4	·846	–0·2
1993	July	8·15	49176·15	–12·2	0·852	+0·1
	Dec.	24·74	345·74	–12·4	·858	0·0
1994	May	3·16	49475·16	–12·5	0·862	0·0
	Dec.	10·74	696·74	–12·5	·869	+0·1
1995	May	30·06	49867·06	–12·9	0·875	–0·2
	Dec.	31·75	50082·75	–13·3	·881	–0·4
1996	Nov.	15·73**	50402·73	–12·5	0·892	+0·7
	Dec.	25·71	442·71	–13·1	·893	+0·1
1997	May	12·09**	50580·09	–13·1	0·897	+0·2
	Dec.	22·74	804·74	–13·9	·904	–0·3
1998	May	3·14	50936·14	–13·7	0·909	0·0
1999	Dec.	28·73	51540·73	–14·3	0·928	+0·1
2000	Jan.	8·74	51551·74	–14·1	0·928	+0·3
	Apr.	7·18	641·18	–15·1	·931	–0·6
	June	18·03	713·03	–14·8	·934	–0·2
	Aug.	28·93	784·93	–14·5	·936	+0·2
	Oct.	30·78	847·78	–14·6	·938	+0·2
	Dec.	10·71	888·71	–14·6	·939	+0·3
		28·71	906·71	–15·1	·940	–0·2
2001	Mar.	12·24	51980·24	–15·1	0·942	–0·1
	May	13·14	52042·14	–15·3	·944	–0·2
	July	31·07	121·07	–14·9	·947	+0·3
	Sept.	25·90	177·90	–15·4	·948	–0·2
	Nov.	1·80	214·80	–15·1	·950	+0·2
2002	Mar.	27·20	52360·20	–15·6	0·954	–0·1
	May	17·11	411·11	–15·9	·956	–0·3
	July	14·05	469·05	–15·7	·958	–0·1
	Sept.	9·01	526·01	–15·5	·959	+0·2
	Nov.	7·85	585·85	–15·6	·961	+0·2
2003	Jan.	11·72	52650·72	–15·7	0·963	+0·2
	May	6·10	765·10	–16·1	·967	–0·1
	July	14·05	834·05	–16·2	·969	–0·1
	Sept.	22·94	904·94	–16·2	·972	0·0
	Nov.	27·77	970·77	–16·4	·974	–0·1

TABLE VI (*concluded*)

Date (UT)		*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O − C)* $km\ s^{-1}$
2004	Jan. 24·72	53028·72	−16·6	0·976	−0·3
	Apr. 22·17	117·17	−16·5	·978	−0·1
	June 15·12	171·12	−16·4	·980	+0·1
	Aug. 13·03	230·03	−16·6	·982	−0·1
	Oct. 25·86	303·86	−16·4	·984	+0·2
	Dec. 16·74	355·74	−16·5	·986	+0·1
2005	May 8·15	53498·15	−16·9	0·991	−0·2
	July 17·10	568·10	−16·8	·993	0·0
	Sept. 14·92	627·92	−16·8	·995	0·0
	Nov. 9·84	683·84	−16·7	·996	+0·1
2006	Apr. 9·16	53834·16	−16·9	1·001	−0·1
	July 3·10	919·10	−16·6	·004	+0·2
	Sept. 8·02	986·02	−16·7	·006	+0·1
	Nov. 18·84	54057·84	−16·9	·008	−0·1
2007	Jan. 14·73	54114·73	−16·8	1·010	0·0
	May 2·14	222·14	−17·2	·014	−0·5
	July 25·06	306·06	−16·5	·016	+0·2
	Sept. 29·94	372·94	−16·5	·019	+0·2
	Dec. 5·83	439·83	−16·6	·021	0·0
2008	Jan. 7·75	54472·75	−16·6	1·022	0·0

* Lick[6] observation, weight 1/10.
† Bonn[141] observation, weight 0·025.
‡ DAO[96] observation, weight 0·01.
§ Observed with Palomar 200-inch telescope, wt. 1/2.
¶ Ames[99] observation, weight 0·025.
M Observation reported through the CDS[92], wt. 1/4.
‖ Observed with Haute-Provence *Coravel.*
** Observed with Cambridge *Coravel.*

mean value that had been obtained[6] at Lick. Between 1971 and 2008 the writer obtained 30 measurements of its velocity with the original spectrometer, 15 with the OHP *Coravel*, 45 with the Cambridge *Coravel*, and two at Palomar. Those observations (only) are plotted directly against time in Fig. 8. It will be seen that the initial Palomar measurement appears to be something like 1 km s^{-1} 'high', but after that the velocity (if viewed without the benefit of seeing what happened later) appeared to continue constant near −11 km s^{-1} for 20 years. During all that time the writer maintained not only an observing cadence of about two measurements a year but also his faith that 39 Cyg had once exhibited a minimum of velocity and would do so again when the proper time for it came round. Only in 1992 — when, as it happened, the programme was transferred to the OHP instrument, which gave more accurate velocities, the original spectrometer having been de-commissioned — did the velocity start to change significantly in the manner that had all along been expected, declining towards the Lick level. In the fullness of time it passed that level, and in recent years has clearly reached the descending node and is just beginning to rise again.

Fig. 9 plots all the radial velocities listed in Table VI, not only the author's measurements but also all the others that have been obtained over the past 102 years. Those that overlap in time with the author's cannot be said to contribute anything fresh, but the early ones[6,141,96] show rather convincingly (no matter how sketchily)

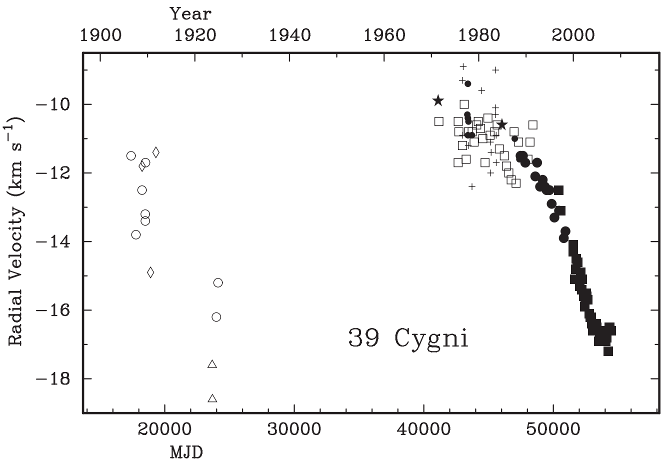

FIG. 9

All available observations of 39 Cygni. Here the measurements shown in Fig. 8 are supplemented in the early years by those from Lick[6] (circles), Bonn[141] (diamonds), and the DAO[96] (triangles), and around 1980 by velocities from Ames[99] (small plusses; two of them, both of 'B' quality, fall off the top of the diagram), and by some measurements (small filled circles) made with the OHP *Coravel* by people[92] other than the writer. It is considered that the early observations indicate a velocity minimum analogous to that taking place at the time of writing.

another velocity minimum in about 1920, analogous to the one that is taking place now. An orbit is readily computed on that basis; it has a period of about 86 years, with a standard deviation of less than one year, although it would be wise not to put unbounded faith in formal standard errors computed from such patchy and inhomogeneous data. The orbit is illustrated in Fig. 10, and its elements are:

$P = 31292 \pm 324$ days	$(T)_2 = \text{MJD } 53794 \pm 174$
$\gamma = -12{\cdot}01 \pm 0{\cdot}19$ km s^{-1}	$a_1 \sin i = 1207 \pm 46$ Gm
$K = 3{\cdot}23 \pm 0{\cdot}11$ km s^{-1}	$f(m) = 0{\cdot}072 \pm 0{\cdot}008\, M_\odot$
$e = 0{\cdot}495 \pm 0{\cdot}023$	
$\omega = 177 \pm 7$ degrees	R.m.s. residual (wt. 1) = $0{\cdot}23$ km s^{-1}

In that solution, the writer's *Coravel* measurements have been given unit weight, the Palomar ones weight 1/2, the OHP ones[92] from the Centre de Données Stellaires 1/4, the 'original Cambridge' ones 1/5, Lick[6] 1/10, Bonn[141] 0·025, Ames[99] 0·025 (halved for the four measurements accorded category B by their authors), and DAO[96] 0·01.

Between 1924 and 1971 there is a gap that is longer than any more-or-less continuous run of observations. In principle there is room to fit another minimum into it, but that looks very unlikely. An analogous gap of something like half a century in observational coverage has caused a lot of trouble in other cases of long-period spectroscopic binaries, of which 56 UMa, treated above, is one. It is only about now, when the duration of quasi-continuous coverage of such binaries is becoming comparable with the length of the observational hiatus in the mid-20th Century, that it is becoming possible to resolve the ambiguities. An orbital solution of 39 Cyg with the period halved does not fit the data as well as the

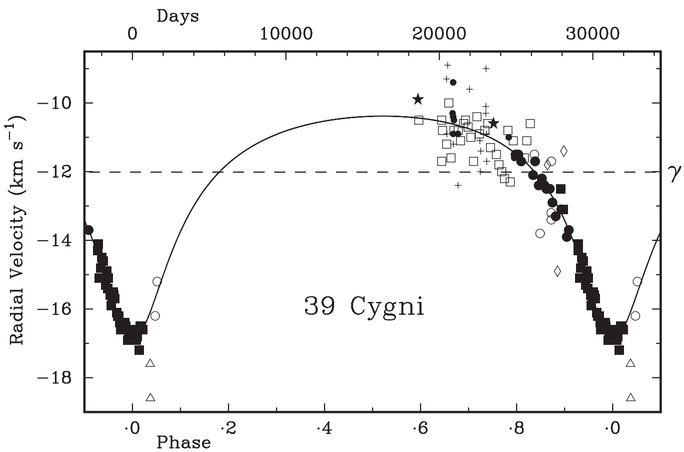

FIG. 10

The same data as in Fig. 9, but plotted folded on the proposed orbital period of about 31 000 days and with the corresponding velocity curve superimposed.

86-year solution: the sum of the squares of the weighted deviations rises from the 7·12 of the solution in the above table to 9·18 (km s^{-1})^2. Since the 7·12 arises from 123 degrees of freedom, it is obvious that the extra 2·06 (km s^{-1})^2 is an exorbitant cost for the imposition of just one parameter and assures us that that parameter is erroneous.

For readily plausible masses of the 39 Cyg primary, the mass function demands a secondary whose minimum mass is 0·7–1·0 $M_{\odot}$. It could be a white dwarf, but is more likely to be a main-sequence star with a type somewhere between F and mid-K, several magnitudes fainter than the primary. If we arbitrarily suppose that its mass is 2/3 of that of the primary, then the mean linear separation of the pair would be 2½ times the $a_1 \sin i$ value shown in the informal table above, about 3 Tm or 20 AU. Since the orbit is presented to us with its long axis in the 'plane of the sky' ($\omega \sim 180°$), we should see the separation at apastron, where it is enhanced by the factor ($1 + e$) to 30 AU, as nearly 0″·4 at the *Hipparcos* distance of about 78 pc. It is presumably only the magnitude difference that has enabled 39 Cyg to avoid being recognized as a visual double star already.

The 86-year orbit proposed here for 39 Cyg has much the longest period of any orbit derived wholly from radial velocities, but clearly there is a long way to go before it could be described as 'good' and thereby supplant the orbit of Polaris as the longest good spectroscopic orbit. The other orbits derived in this paper also fail, κ Per because its period is not long enough, β LMi because there are anomalies in the velocity curve of the secondary star, and 56 UMa and HR 4593 because their orbits are too sparsely observed over considerable ranges of phases. Thus Polaris[10,11] still retains the palm! Nevertheless, an overview of the sizes and periods of orbits that are now quite commonly determined spectroscopically demonstrates how considerably the subject has expanded since the introduction of cross-correlation methods. As examples of limitations that are now routinely surpassed, we might quote Walters[197], "The separation of a spectroscopic binary is not likely to be as large as 2×10^{13} cm., which is 1·29 [actually 1·34 — R.F.G.] astronomical units." And in 1977 — when the series of papers of which this is a member was already in full swing — there was published the sixth edition of

Smart's textbook[198], which is still being sold (or at least offered for sale) today and asserts, "The orbital periods of spectroscopic binaries are generally several days only, and as observations may be carried on over several months, or even years, the orbital periods can be found with great accuracy."!

Acknowledgements

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This series of papers continues to rely heavily on observations made in the previous century at other observatories; I am pleased to thank once again the Geneva, Palomar, Dominion Astrophysical, and European Southern observatories for their hospitality. I am most grateful to Dr. R. E. M. Griffin for the excellent reduction software that she has written for treating the data from the Cambridge *Coravel*, as well as from Palomar and the DAO. I am pleased also to acknowledge the use of *Simbad* and other facilities made freely available by the Centre de Données Stellaires, and to mention particularly Dr. W. P. Bidelman's stellar data file, which was long ago made available by the Centre and many of whose entries pre-date *Simbad*. For stars that have long been known to be spectroscopic or visual binaries I have also benefitted greatly from the bibliographies provided by Korytnikov *et al.*[199] and Burnham[200], respectively. Mr. M. Hurn, the Departmental Librarian, has been very helpful over bibliographical matters.

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COPERNICUS AND IBN AL-SHATIR: DOES THE COPERNICAN REVOLUTION HAVE ISLAMIC ROOTS?

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I review first the main similarities and differences between the planetary models of Ibn al-Shatir (14th-Century Muslim astronomer) and of Copernicus. I show that important similarities reside in the technical aspects of the orbits constructed by the two astronomers but that fundamental differences are: (*a*) Copernicus adopted a heliocentric model while Ibn al-Shatir (and all Muslim astronomers) assumed a geocentric model, as strictly as possible; (*b*) Copernicus followed a clear inductive method while Ibn al-Shatir remained within the *Zij* (astronomical tables) tradition. On the question of the extent to which Copernicus had benefitted from the 'transmission' of those models and critiques of Ptolemy, I insist that neither Ibn al-Shatir nor any Muslim astronomer accepted, let alone proposed, a heliocentric model. I then briefly discuss the Copernican Revolution and try to assess the extent to which the Polish astronomer might have been influenced by earlier Muslim discussions on the centrality (or not) and immobility (or possible motion) of Earth.

The problem

The Arab Muslim 14th-Century Damascene astronomer `Ala al-Din Ibn al-Shatir (1304–1375) and his important works were only discovered in the 1950s by Kennedy & Roberts[1]. It was quickly realized that he was an accomplished astronomer, able to make sophisticated calculations based on old and new models, construct instruments, and perform detailed observations; he also worked as *muwaqqit* (timekeeper) in the Mosque of Damascus.

Kennedy & Roberts found that Ibn al-Shatir's planetary models, while being firmly geocentric, eerily resembled those of Copernicus, at least in their geometrical construction. This led to a reexamination of the possible transfer to — and influence on — European Renaissance scientists (particularly Copernicus) by late-Muslim-era astronomers; Gingerich, a leading contemporary historian of astronomy, has thus written[2], "Scholars are currently divided over whether Copernicus got his method for replacing the equant by some unknown route from the Islamic world or whether he found it on his own." He added, "I personally believe he could have invented the method independently."

Unfortunately, this discovery and new perspective has also produced a less scholarly discourse of claims that Copernicus took his theory from Muslim astronomers, a discourse which has lately got louder and bolder. Such claims range from the moderate to the extreme. One can find writers and speakers who state that Copernicus "took" his planetary geometry from Al-Tusi and Ibn al-Shatir (although the historical route by which this would have occurred is far from established) and only "added" his heliocentric innovation; one can also find published works where Arab/Muslim authors boldly state that Copernicus "plagiarized" from his Islamic predecessors, and sometimes that Muslim astronomers like Al-Biruni and Ibn al-Shatir did propose Sun-centred cosmologies.

Among the group of moderate authors, those who stress Copernicus' full knowledge of (and thus deep influence by) Ibn al-Shatir and other Muslim astronomers, one may cite Fernini[3]; he writes, "[Ibn al-Shatir's] planetary models *refuted* those of Ptolemy and with the reservation that they are geocentrics, they are the same as those of Copernicus" (emphasis mine). Fernini, an astronomer, does understand the intricacies of the planetary models under discussion, but he does play down the crucial heliocentric–geocentric model difference and tends to ignore the fact that their new geometric model turned out to be completely wrong. He states[3], "This planetary model most likely influenced Copernicus through Byzantine intermediaries and, with the work of Al-Tusi's followers, contains all the novelties of Copernicus' astronomy except the heliocentric hypothesis."

In the second, much more extreme category of authors, one glaring example is Nas E. Boutamina, who titles the sixth chapter of his recent book[4] 'The era of plagiarism and looting', in which he presents in tabular form dozens of (totally unsubstantiated) claims of plagiarism of Muslim works by Renaissance authors and scientists; for example, according to him, Copernicus and Kepler plagiarized their great works from Al-Biruni, Al-Farghani, Al-Bitruji, and Ibn Yunus; Tycho Brahe would have plagiarized from Al-Buzjani, *etc.*

The serious problem of misrepresentation that has developed around the 'similarities' between the models of Ibn al-Shatir and of Copernicus stems from a hasty reading and misunderstanding of the commentaries made by some contemporary historians of astronomy regarding the works of Ibn al-Shatir. For instance, George Saliba, one of today's main experts on Islamic astronomy, writes[5], "Aristarchos [and] Biruni … did acknowledge that the same phenomena could either be explained by a fixed Earth at the center or by a moving one." He quickly adds, however, "but that did not change the Aristotelian cosmological conditions one bit."

The Ptolemaic and the Muslim astronomers' models

Although the idea that the Sun could be the centre of the world was proposed more than once in ancient times, it was the geocentric model (in various versions) that prevailed in most of astronomy's history until Copernicus. Indeed, Aristarchus of Samos (310–*ca.* 230 BC) is often cited for having formally proposed a Sun-centred model of the Universe, but historians know that others before and after him had suggested the same; Copernicus himself acknowledges Hicetas (as cited by Cicero) and others he does not name (though he gives Plutarch as a reference); we now also know that the 15th-Century Cardinal Nicholas of Cusa advanced the idea of a moving Earth by assuming a plurality of the worlds, and it is also quite well established* that Abu al-Rayhan Al-Biruni (973–1048) did consider and discuss the heliocentric hypothesis he had found in Indian books, but ended up rejecting it after noting that the Earth's speed would be so large that effects in nature would be very easily noticeable.

The centrality of Earth in the world was unanimously accepted by Muslim scientists and philosophers. There are two main reasons for that: (*a*) although the Quran does not impose or even clearly promote a geocentric view of the cosmos, it is, however, resolutely anthropocentric, stating clearly that everything in the world was created for Man's benefit; (*b*) the Muslim intellectual tradition was largely Hellenistic in its assumptions and outlook — except for cases like Al-Biruni, who was equally Indian in his background knowledge — hence the adoption of the Ptolemaic geometry and of the Aristotelian physics and (geocentric) cosmology.

The model of planetary motion developed by Ptolemy (*ca.* 90–168 AD) can be found in any textbook of introductory astronomy. Its basic features, illustrated in Fig. 1, are: (*a*) its geocentric nature; (*b*) the circular motions of all objects; (*c*) the addition of 'epicycles', 'deferents', and an 'equant' (a point symmetrically opposite to the Earth with respect to the centre of the deferent), in order to account better for the observations.

The Muslim astronomers worked largely within the Ptolemaic framework but sometimes adopted different values for the orbital parameters or even proposed significant modifications to his scheme, such as imposing a fully geocentric and/or uniform orbital motion for the planets. For example, Muhammad Al-Battani (*ca.* 853–929), one of the greatest Muslim astronomers of the whole era, one who was cited[2] 23 times by Copernicus in *De Revolutionibus*, produced his *Zij* (astronomical tables) strictly within the Ptolemaic model, but with new values for some of the parameters.

It was not unusual for Muslim astronomers to formulate 'doubts' (*shukuk*) or critiques regarding the Ptolemaic system. For example, Ibn al-Haytham (965–1039), the Muslim polymath who is widely considered as the founder of modern optics and who also made important contributions in astronomy and several other sciences, was one of the first to voice such doubts/critiques, but these were often (though not always) made on philosophical rather than observational bases, and they never called into question the geocentric nature of the model. The equant concept was particularly bothersome to the Muslim astronomers, who much preferred a system with the Earth exactly at the centre, and with perfect circles and uniform motions. Ibn Rushd (Averroes, 1126–1998), the great Andalusian philosopher and scientist, strongly objected to the whole system on precisely such grounds and declared it thus to be no more than a mathematical construct that is "contrary to nature." Another Andalusian astronomer, Al-Bitruji

* On the medieval Muslims' views on heliocentrism, the *Encyclopedia of Chemistry* states the following: "In 1030, Abu al-Rayhan al-Biruni discussed the Indian heliocentric theories of Aryabhata, Brahmagupta, and Varahamihira in his *Ta'rikh al-Hind* (*Indica* in Latin). Biruni stated that the followers of Aryabhata consider the Sun to be at the centre. In fact, Biruni casually stated that this does not create any mathematical problems."

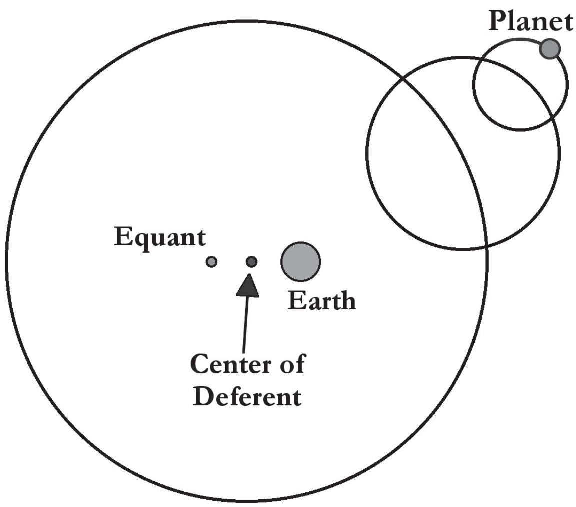

FIG. 1

A Ptolemaic model of a planetary orbit, where in addition to epicycles around a deferent, an 'equant' has been introduced: the Earth is shifted away from the centre of the deferent, and the point symmetric to Earth with respect to the centre is the equant; the planet is then constrained to move uniformly about the equant.

(died *ca.* 1204), whose different order of the planets (placing Venus above the Sun) was cited by Copernicus, went so far as to construct a strictly geocentric model, which led to enormous disagreements with the observations. Al-Tusi (1201–1274), another great astronomer, philosopher, and scientist (for whom Ulugh Khan built the famous Maragha observatory in Southern Azerbaijan, present-day Iran), also got rid of the equant in his model; he did so by adding two small epicycles to each planet's orbit, the famous 'Tusi couple'. Al-Tusi thus launched an influential new 'school' of astronomy; other important members were Mu´ayyad al-Din al-`Urdi and Qutb al-Din al-Shirazi.

Ibn al-Shatir's and Copernicus' models

It is thus in this context that Ibn al-Shatir appeared, barely a century after the radical model of Al-Bitruji and a mere 30 years after the formulation of Al-Tusi's mathematical constructs and theorems. As Ragep stresses[6,7], Ibn al-Shatir must be viewed fully in continuity of the eastern school of Ptolemaic reformulation, *i.e.*, Al-Tusi's, and not the western, Andalusian school, *i.e.*, Ibn Rushd's and Al-Bitruji's, which was much more radical but failed in its programme. To put it succinctly, the trend was to push back for truly geocentric models and to try to stick to perfect (as opposed to eccentric) circles and uniform motions as much as possible.

Ibn al-Shatir, and Copernicus at first (in his *Commentariolus* but not in his *De Revolutionibus*), thus inherited from the astronomy of the times the tendency to get rid of the equant. So Ibn al-Shatir constructed a completely concentric arrangement of the planets by adopting a system very similar to that of Al-Tusi;

he remained fully geocentric and retained the deferent and epicycle circles. He introduced certain changes and new concepts or details, for instance, making a distinction between the 'apparent epicyclic apogee' (*al-dhirwat al-mar´iya*) and the 'true epicyclic apogee' (*al-dhirwat al-haqiqiya*). But none of these modifications can be considered as truly fundamental, much less revolutionary.

Kennedy & Roberts hail the work of Ibn al-Shatir and show remarkable similarities with specific aspects of Copernicus' orbits but insist that neither Ibn al-Shatir nor Copernicus was first in introducing some of the features we find in common between them. It is thus clear that Ibn al-Shatir was influenced by the Maragha School, although he was indeed somewhat novel and ingenious in the system he proposed. It is further possible that Copernicus did not know of Ibn al-Shatir's model and works but rather learned the same lessons from their common predecessors and made a similar development in model construction, which resulted in an analogous system, albeit with some differences. Indeed, as Gingerich tells us[2], "some of the Al-Tusi material is known to have reached Rome in the 15th Century…" but he does add that "there is no evidence that Copernicus ever saw it." One huge mystery that has contributed to the debate (and sometimes suspicions) of whether Copernicus took important elements of his planetary model from his Islamic predecessors is that while he does cite Al-Battani, Al-Bitruji, Az-Zarqali, Ibn Rushd, and Thabit Ibn Qurra in *De Revolutionibus*, he does not mention Al-Tusi or any of the Maragha astronomers, although it has been shown that some of the diagrams and mathematical tools (*e.g.*, the Tusi couple) that Copernicus used are almost literally copied from Arabic sources. Why would Copernicus cite some — many times — and ignore others? Perhaps the answer is that some of the Maragha material that he used reached him without proper reference and/or *via* medieval western sources.

Kennedy & Roberts do find important differences between Ibn al-Shatir and Copernicus. First they find evidence that the Polish astronomer understood what today we would call "the commutativity of vector addition", while there is no evidence of that in the Damascene astronomer's work. Most importantly, the main work of Ibn al-Shatir that has been found and studied, *Nihayat al-Sul fi tashih al-usul* ('The Final Quest concerning the Rectification of Principles'), resembles Copernicus' *Commentariolus* more than *De Revolutionibus* in that the first one contained only quantitative descriptions of the systems, whereas Copernicus' latter (final) book presented a full analysis leading from the observational data to the theory. Kennedy & Roberts give Ibn al-Shatir the benefit of the doubt in stating that he "may have done the same in his *Ta`liq al-arsad* … but no copy is known to have survived", and so does Saliba, who states[5] that the Damascene astronomer "devoted a whole book (*Ta`liq al-arsad*, meaning 'Accounting for Observations') to this particular relationship between observations and the construction of predictive models", but since the book "seems to be unfortunately lost … we may never know the extent of his theorizing in this regard." Kennedy & Roberts, however, do not lose from their sight the essential difference between the two models, namely that there is no trace of a heliocentric view in the works of Ibn al-Shatir.

The relative importance of Copernicus' two moves, namely his adoption of the Al-Tusi–Ibn al-Shatir geometric orbit constructs on the one hand and his decision to shift the centre of the world from the Earth to the Sun, is not always made very clear. Indeed, Saliba, for instance, tends sometimes to underscore the second idea (of heliocentrism), referring to it as a "genius idea", yet at other times he tends to suggest that Copernicus' adoption of the Maragha School's (and Ibn al-

Shatir's) orbital geometry is extremely important and that it perhaps even somehow paved the way to the heliocentric idea.

Thomas Kuhn, the historian and philosopher of science, has carefully analyzed the Copernican Revolution, which he refers[8] to as an "upheaval in astronomical and cosmological thought". Kuhn remarks that while the opus itself may have been drafted in a very traditional and conservative manner, without realizing it the Polish astronomer had produced "a revolution-making rather than a revolutionary text", and that "within [the] general classical framework [of *De Revolutionibus*] are to be found a few novelties which shifted the direction of scientific thought in ways unforeseen by its author and which gave rise to a rapid and complete break with the ancient tradition."

The roots of the revolution

So then how did Copernicus arrive at this revolutionary idea? And most importantly for us, was he influenced by the Muslim works that preceded him?

Let me first note that it is usually accepted that what led Copernicus to move to a Sun-centred cosmology had as much to do with philosophical and aesthetic reasons as with the mathematics (geometry) of the planetary orbits; indeed, Copernicus criticized the Ptolemaic system as "not sufficiently pleasing to the mind". The Polish astronomer had insisted (in the letter to the Pope with which he prefaced *De Revolutionibus*) that his decision to adopt a moving-Earth model was primarily driven by the fact that "the mathematicians" (his predecessors, the ancient and medieval astronomers) were inconsistent and unable to account for such features as the constant length of the seasonal year (see reproduction of letter in ref. 8). But the man is also known to have been a strong adherer to Neo-Platonism (Kuhn writes[8] on page 141: "a man without Copernicus' Neoplatonic bias might have concluded merely that the problem of the planets could have no solution that was simultaneously simple and precise.") and a lover of the Sun as a quasi-divine object.

A few researchers have tried to retrace the genesis of the 'revolution' in Copernicus' mind. Some have adopted the 'independent proposition' paradigm (that Copernicus came up with it without any hint from his predecessors), and others have tried to find ideas in the predecessors' works that may have pointed him to the new system. For example, Swerdlow is convinced[9] that Copernicus must have been fully aware of the Islamic tradition of reforming the Ptolemaic system, in particular the move to rid the model of the equant. He believes that when Copernicus attempted to remedy to the main anomalies of the 'classical' model, he found that if one tried to avoid eccentric models or mixed (geo/heliocentric) ones where orbits ended up intersecting, the only satisfactory solution was to put the Sun firmly at the centre.

Jamil Ragep[6] has gingerly argued for the existence of some latent concepts of non-geocentric astronomy in the ideas developed by the Maragha School, particularly those of Al-Tusi and of `Ali al-Qushji (died 1474). Ragep recalls that Al-Tusi, contrary to Ptolemy, believed it impossible to decide either theoretically or observationally whether the Earth was fixed or moving. Earlier, both Aristarchus and Al-Biruni had realized that celestial phenomena could be explained either way. Ragep also emphasizes Copernicus' usage of Al-Tusi's arguments against Ptolemy's view of the Earth's immobility, noting that Al-Tusi's position was adopted by the later Maragha astronomers, including Al-Qushji, thus making this "a conceptual revolution that was going on in Islamic astronomy". He adds, "The fact that we can find a long, vigorous discussion in Islam of this issue intricately tied to the question of the Earth's movement should indicate that such a conceptual foundation was there for the borrowing."

It is a rather stretched conclusion to go from "there is no objection to having the Earth rotate or even move" (and Al-Qushji declared[6,7] that "nothing false follows from the assumption of a rotating Earth") to "one may start constructing a heliocentric cosmology". But one aspect of this viewpoint is important, namely the insistence that many non-Ptolemaic ideas (though nothing heliocentric) had circulated among Islamic astronomers, contrary to the situation in Europe.

A crucial question that one should ask in this context, especially considering Saliba's remarks[5] that Ibn al-Shatir's 'geometrically unified model' paved the way for Copernicus' heliocentrism, is simply: why didn't any of the Muslim astronomers ever show any interest in such a model? Saliba answers that the physics of the time (up to and including Copernicus) was Aristotelian, therefore it made no sense for Muslim astronomers to propose such a schizophrenic system as Copernicus did. Saliba "raise[s] the question of the scientific legitimacy of heliocentrism itself in a pre-Newtonian universe, where no alternative cosmology was yet available." He insists that "without the benefit of the Newtonian law of universal gravitation, how could [anyone] have hoped to maintain the system together?" But that amounts to rejecting any non-deductivist development in the history of science, when in fact there are numerous examples of laws that were constructed in a totally empirical way without the slightest understanding of the first principles that led to them; Newton's law of universal gravitation is the perfect example, since it was largely inferred from Kepler's laws, which were completely empirical and for which neither Kepler nor anyone could propose any explanation. In fact Newton's law was long regarded as mysterious by Newton himself, who saw the instantaneous action at a distance as essentially occult. This did not stop Kepler, Newton, and the multitudes of scientists after them from recognizing the validity of those and other laws, which were left to be explained later. To excuse Muslim astronomers and even applaud them for staying true to the physics of their times is not, in my view, a reasonable position.

Finally, we may mention Seyyed Hossein Nasr, the Iranian philosopher of science who has tried to answer the question of why, if a "conceptual foundation" for non-geocentric models had indeed developed in the later periods of Islamic astronomy, the Muslim astronomers didn't come up with anything resembling Copernicus' heliocentric model, let alone Kepler's? Nasr's view[10] is that the Muslim astronomers in effect saw the (full) revolution that was in store should any non-geocentric cosmology develop, and that they simply held back in order to prevent any philosophical and religious upheaval from occurring. It is a purely speculative retrospective view of history, one that has little evidential support.

Summary and conclusions

Let me now briefly summarize the main ideas and findings from this general review of the story of Copernicus, Ibn al-Shatir, and the Copernican Revolution.

(*i*) There are important similarities and differences in the works of Ibn al-Shatir and Copernicus: (*a*) The first main similarity is in their common attempts to make use of 'vectors' (Tusi-type epicycles) that could produce uniform motions. (*b*) Another similarity is in Ibn al-Shatir's insistence on discarding the equant, and Copernicus' acceptance of that. (*c*) The obvious fundamental difference between the two models is that Copernicus' was heliocentric while Ibn al-Shatir's was firmly geocentric. (*d*) The second important difference between the works of the two astronomers is that Copernicus adopted a clear inductive approach (from data to theory) while Ibn al-Shatir seemed to remain within the *Zij* tradition.

(*ii*) Ibn al-Shatir never so much hinted, let alone declared, that the Earth or any planet moved at all (around the Sun or even around itself); on the contrary, the modifications he brought to Ptolemy's model aimed at making it more strictly geocentric.

(*iii*) Copernicus did construct a model that was in its technical (geometric) aspects very similar to that of Ibn al-Shatir, especially for the Moon, but also to a large extent for Mercury and Venus, but that whole model was later shown (by Kepler) to be utterly wrong and replaced by a model of simple elliptical orbits with the Sun at one of the foci.

(*iv*) There is no evidence that Copernicus read Ibn al-Shatir and chose never to mention him. If Copernicus acknowledged and cited Al-Battani 23 times and mentioned Al-Bitruji and other Muslim astronomers, it would really be an amazing, deliberately ill-intentioned act on his part to leave out Ibn al-Shatir altogether. The more likely explanation is that both men were influenced by the Maragha School's models and made modifications and developments that led to very similar models (technically), although conceptually very different. In this regard, one should note that Hartner[11] has convincingly shown that Copernicus essentially copied (with someone else's help, since he didn't know any Arabic) Al-Tusi's proof of his 'couple', replacing the Arabic symbols with Latin ones, without duly crediting the Muslim astronomer; again, perhaps Copernicus never read any of Al-Tusi's works, that he found the Maragha astronomer's theorem somewhere else and used it without correct referencing.

Still an important underlying question remains to be addressed: since it has become quite clear that Copernicus was indeed influenced by the Maragha School's 'new astronomy' and since there was in Europe up to that point no tradition of criticism toward the Ptolemaic planetary system, how could a classically-minded medieval European come up with a theory that would truly revolutionize astronomy and usher in modern science and a new world view? I explained that in the absence of sufficient historical data, researchers have adopted one of two positions: (*i*) a 'conceptual foundation' had started to be built among the Islamic astronomical community that substantial modifications to the Ptolemaic system needed to be made, and Copernicus became fully aware of that; his subscription to Neo-Platonism and the special place of the Sun in his philosophy led him to formulating the heliocentric hypothesis; (*ii*) Copernicus remained firmly within the classical astronomical tradition (as the *Almagest*-like structure of his book shows) and only proposed a Sun-centred model because it looked simpler and more aesthetically pleasing than the cumbersome and thoroughly imperfect Ptolemaic system.

Finally, I would like to conclude by commenting on the contemporary problem of the continued erroneous presentation of the 'similarities' between Copernicus' model and those of his Islamic predecessors. Indeed, there has been a growing insistence in the Arab/Muslim world (at least among the non-expert but educated society) that the Copernican revolution was explicitly or implicitly "in the works" of Muslim astronomers and that Copernicus at best dotted the 'i's and crossed the 't's.

I believe that despite many efforts at unearthing and presenting to the (Muslim and western) public the wealth of scientific works that the golden-era Muslim scholars produced, there is still serious ignorance as to exactly what was done. Most of the discourse on the 'Islamic civilization' has remained superficial and ill-informed. And that is why we hear academics proclaiming that Al-Biruni and Ibn al-Shatir proposed that planets orbit the Sun and that Copernicus took his theory from the Damascene astronomer. Gingerich could not have put it more clearly when he wrote, "The Islamic astronomers would probably have been astonished and even horrified by the revolution started by Copernicus."

Gingerich, Kennedy, Kuhn, Kunitzsch, Ragep, Saliba, and such researchers do their work with great care and attention, reading and analyzing the sources, following the historic trail and crediting each scientist with due measure. It is hoped that all writers on such topics adhere to such rigorous methodology, making only measured and substantiated claims instead of extrapolating from superficial readings of secondary and tertiary sources.

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REVIEWS

Thomas Hardy's Novel Universe: Astronomy, Cosmology and Gender in the Post-Darwinian World, by P. Gossin (Ashgate Publishing, Aldershot), 2007. Pp. 300, 24 × 16·5 cm. Price £50 (hardbound; ISBN 978 0 7546 0336 8).

[The Editors have elected to publish two reviews of this work: the first, by Allan Chapman whose knowledge of Victorian astronomy is unsurpassed, emphasizes the scientific context; the second, by David Wright — who drew our attention in these pages (**118**, 301) to Hardy's astronomical interest in astronomy — concentrates on Hardy's writing.]

Over the past few decades the novels of Thomas Hardy have come to enjoy a new lease of life, with TV adaptations of some of the most popular, being as they are full of passion and conflict, and set within a magically-evoked world of Victorian Wessex. Yet what is less well known about Hardy is his lifelong passion for science, especially for the then new science of astrophysics, and the way in which it, geology, and Darwinian evolution impacted upon religion, philosophy, and the seemingly traditional certainties of life. And Hardy's *Two on a Tower* (1882), as Pamela Gossin rightly reminds us, is probably the only novel in the English language in which astronomy constitutes a major theme and drives much of the action.

Though I must confess to being wary of books which display the word "gender" in their title and begin by discussing their "critical methodology", I nonetheless felt that, having paid her homage to contemporary literary critical fashions, Dr. Gossin goes on to write a fascinating and deeply illuminating book. She supplies excellent background material on the key intellectual issues of the day, as well as providing a very fine potted history of astronomy from Greek times to the rise of astrophysics, to set Hardy and his contemporaries firmly into context. Thomas Hardy's own early life-story was similar to that of many of the characters that populate his novels, not to mention the lives of so many of the real-life astronomers of the day (that pioneer of astrophysics, Sir William Huggins, springs to mind as an example): men coming from an essentially autodidact tradition, at least in regard to their fields of endeavour and distinction in adulthood.

Central to Pamela Gossin's book is the awareness of how fast scientific ideas were changing by the late 19th Century. Geology was opening up a new history of the Earth, Darwinian evolution our understanding of living things, and astrophysics the vastness, complexity, and physical unity of the cosmos. Two factors which were utterly fundamental to these new discoveries and the ideas which flowed from them were (*a*) the inconceivable vastness of both cosmological timescales and of space itself, and (*b*) the apparent evolutionary character of everything, for did not the spectroscope suggest that even stars were born, developed, and burned out, with their elemental matter being recycled into new stars? Hardy read avidly in the sciences, and kept detailed notebooks, and one Victorian astronomical interpreter who had a formative influence upon him was Richard A. Proctor. Dr. Gossin's work on these notebooks forms a major primary source for *Thomas Hardy's Novel Universe*. And what many Hardy readers may not have realized is that these 'big ideas' of the day permeated most of what he wrote, in one way or another, as his star-crossed characters discuss their passions, predicaments, and place in the vastness of creation.

I warmly recommend this work, not only to lovers of Hardy's novels, but also to anyone interested in the history of 19th-Century astronomy and its place within the wider schema of the sciences as they were then understood, for Pamela Gossin's book is a well-written, jargon-free history of scientific and literary thought, as expressed through the writings of Thomas Hardy. — ALLAN CHAPMAN.

Pamela Gossin has attempted to examine how "Thomas Hardy made sense of life in the universe" and succeeded in making an interesting contribution to Hardy scholarship. Philosophically written, there is much discussion of the astronomically themed *Two on a Tower* (1882). In this novel, the young amateur astronomer Swithin St. Cleeve meets the mature lady of the manor Lady Viviette Constantine, who becomes his patroness. Their romance is skillfully played out against a backdrop of astronomical observation, and ends tragically. Meticulously researched (Hardy studied R. A. Proctor's *Essays in Astronomy* and also obtained permission to visit the Greenwich Observatory), it presented its reader with an amateur's progression through the field of observation and deduction. Rushed in order to conform to American serialization, the novel benefitted by its brisk pace and the topicality of the subject matter. The fictional comet was probably inspired by Tebbutt's Comet, and variable-star theory and the transit of Venus are also included. Swithin's interactions with the professional community later in the story take him to the observatories of Marseilles, Vienna, Poulkowa, Harvard, Yale, and Chicago. In South Africa he is described as attempting to enlarge on Sir John Herschel's charting work.

Astronomical imagery also appears notably in *A Pair of Blue Eyes*, *The Woodlanders*, *Far From The Madding Crowd*, *Tess of the D'Urbervilles*, and *Jude the Obscure*, all of which are referred to in this volume along with a few other works. The novel previous to *Two on a Tower* had been *A Laodecian* (1881), and could perhaps have merited a note here, as it gave a more accurate reflection of the real Hardy's professional abilities as an architect, and his interests in engineering and technology. This was acknowledged by Hardy himself as late as 1900, and would have framed the novelist as a quasi 'Man of Science', but this is only a minor point.

Gossin's idea that "Conjoining Darwinism with astrophysics … provided Hardy with a more universal view of evolutionary theory" is an excellent insight into how the novelist embraced humanity's place in the cosmos. This book is strongly commended to anyone interested in Thomas Hardy's writings and 19th-Century astronomy generally. — DAVID WRIGHT.

The Herschels of Hanover, by M. Hoskin (Science History Publications, Cambridge), 2007. Pp. 182, 24 × 15·5 cm. Price £35/\$70/€50 (plus \$10 or €5 postage) (hardbound; ISBN 978 0 905 19307 6).

And Abraham (born *ca.* 1650) begat Isaac (1707–1769), who begat Jacob (1734–1792), but the ones you have heard of are Jacob's younger brother Friedrich Wilhelm (1738–1822), who grew up to be William Herschel, and, I hope, their sister Carolina Lucretia (1750–1848), the middle half of whose very long life was lived as Caroline Herschel. They and their eight siblings, four of whom died before age 8, all appear in this latest contribution of Michael Hoskin to the literature on the lives and astronomical work of the Herschels of Hanover, Bath, Slough, and all. William's son John (1792–1871) appears primarily as his aunt's confidante. He was an 'only', but himself sired 12 offspring. These carried on the surname in England down, at least, to a John Herschel-Shorland, who is thanked for help with the book, while the youngest daughter, under the name Constance Lubbock, edited *The Herschel Chronicle* (CUP, 1933) and died just 200 years after her grandfather's birth.

A very great deal is said in the 154 pages of text and 25 of notes, and most of it you must simply read for yourself. I had two questions when the book arrived, and here you will get answers (well, partial answers) only to them. The first was triggered by another less-scholarly volume; the second by a CD.

Astronomy first. Herschel is quoted here and elsewhere as having said he had seen light that has been on its way for two million years. Hoskin, as you would expect, provides a proper citation, so we know it's true. But, then, what light had Herschel seen, or thought he had seen, coming from 637 kpc away, a distance modulus of 24 magnitudes? Surely not individual stars. But the quote dates from 1802 and belongs to the era when he thought he had seen to the edge of the Milky Way in all directions, by means of 'stargaging' with his 20-foot (focal length) telescope, and also thought that some of the nebulae he was unable to resolve into stars, including Andromeda and Orion, might be star systems of size comparable with the Milky Way as he then perceived it. On that basis, 637 kpc is not at all a bad distance for M31! (Yes, he and Caroline possessed and used Messier's catalogue and were pleased when they found nebulous objects not in it.) But then came the 40-foot telescope, described by Hoskin as ill-judged. Indeed, little new astronomy resulted from its use, but Herschel did come to realize that there were stars extending far beyond the edges of his famous diagram, perhaps to infinity, so that nothing could be said for or against other comparable star systems. Well, even the *Great Melbourne Telescope* eventually became MACHO before burning down.

And then the musical-question set. Peripheral information suggests widespread talent in the family. Isaac was a self-taught professional. The brothers (even the one dead at 7) seem all to have been violin prodigies. And Caroline, untaught till she arrived in England, was singing within a year or two the lead soprano rôle in *The Messiah* (apparently as much a war horse then as now, with Herschel conducting something that sounds a bit like a sing-along at Bath). And she had learned enough English in six weeks to do the family shopping, suggesting she had a very good ear (though the only other — formerly — young German speaker I know who was shopping for his family in English within weeks of meeting the language doesn't even own a piano, so you never can tell).

But what about William's compositions? Was he any good? Hoskin doesn't tell us, though he provides many details of which musical positions Herschel held and when. My first-hand experience is limited to a short organ recital by Lady Suzi Jeans in connection with the 1970 Brighton General Assembly of the IAU. And there is a CD with six of his 24 symphonies in a set called *Contemporaries of Mozart* (recorded by the London Mozart Players). Mozart is obviously not a fair standard of comparison for anybody, but I at least found more satisfaction in the discs of Michael Haydn, Stammetz, and, yes, Salieri.

Herschel also wrote concert music for various solo and ensemble instruments, dance music, sacred music (most of which has not survived), and madrigals, catches*, and glees. Is there a list of his compositions somewhere? Yes, though not here, and even then it is said to reproduce only the first lines of some of the music. I would have paid twice the price I did for this volume (oh, all right, it is a review copy) for an extra 10 or 20 pages reproducing some of the keyboard (organ, harpsichord, and fortepiano in his day) solo music and some of the madrigals or other vocal ensemble pieces.

I am indebted to author Hoskin, Editor Stickland, and Robin Golding (writer of the notes for the symphony CD) for some factoids not found in the present book, and have asked the Editor's permission to return to the subject of Herschel the composer if I can track down more of his music. — VIRGINIA TRIMBLE.

The Haunted Observatory: Curiosities from the Astronomer's Cabinet, by Richard Baum (Prometheus Books, Amherst, NY), 2007. Pp. 416, 23·5 × 16 cm. Price $28 (about £14) (hardbound; ISBN 978 1 59102 512 2).

The word 'haunted' in the book title whetted my expectations. Was I to be treated to ghostly apparitions in the telescope dome — spectral ladies wandering from the warmth of the marital bed in search of inattentive husbands glued to the eyepiece — or deeply disappointed astronomical ghouls slinking around the observatory corridors bemoaning their inability to find such chimeras as dark matter, the edge of the Universe, or life on Mars? The answer is "no". Richard Baum's thesis is that all observatories, and astronomers, are haunted by disappointment and doubt, those unwelcome bedfellows being brought on by over-expectation and a too-common reliance on observations taken at the very limits of telescopic capability. Astronomy is (or, at least, should be) permanently at the frontiers of

*A catch is like a round, typically with humorous intent, the idea being that the voices catch each other up. Think of "Row, row, row your boat, Underneath the Stream. Ha, ha, I fooled you. I'm a submarine."

knowledge. And frontiers are exciting but also confusing, mysterious, and rather scary places. Astronomers, being merely human, are haunted by the fact that their observations, and the inferences they make from those observations, are too often coloured by their expectations.

Astronomical research is meant to be imaginative, but rather too often astronomers overdo it. Baum's book concentrates on those historical occasions. Normal books on the history of astronomy are rather too kind. They tend to highlight the successes and overlook the mistakes. Baum does the opposite: he revels in the out-takes that litter the cutting-room floor of our past. We are treated to tales of mis-observed snow-capped mountains on Venus, cities and volcanoes on the Moon, strange stars that purport to be Planet X, mysterious bright star-like bodies near the Sun, unusual lunar occultations, and a host of suggested satellites and rings.

Astronomers also seem to be too fond of the conventional. There are many historical examples. Because the Sun was meant to be a pure unsullied golden globe, early observations of sun-spots were 'converted' into sightings of transiting inter-mercurial bodies. Because Earth has a large Moon, similar satellites were purported to be seen around Mercury, Venus, and the Moon itself.

Baum revels in the strange. He also has a commendable love of detail. I greatly enjoyed this book; there is not a dull page in it. I loved the fact that every nook and cranny of each case study was explored; notes abound and references are many. I revelled in the multitude of 'mistakes' that astronomers have made in the past, but I was also greatly impressed by the way that Baum stresses that nothing has changed. We are inevitably making similar mistakes today and still finding it hard to divorce emotion and expectation from observation. — DAVID W. HUGHES.

Von Sonnenuhren, Sternwarten und Exoplaneten — Astronomie in Jena, by R. E. Schielicke (Verlag Dr. Bussert & Stadeler, Jena-Quedlinburg), 2008. Pp. 364, 15·5 × 22·5 cm. Price €24·90 (about £17) (hardcover, ISBN 978 3 932906 80 0).

Here is an impressive historical work that we can recommend right away to people who have mastered German and are interested in the history of European astronomy — in this case *via* an astronomical 'hot spot' that shared the fate of 20th-Century Germany.

Jena is the second largest town of Thuringia (after Erfurt, the state capital). It counts today some 100 000 or more inhabitants. First mentioned in an 1182 document, Jena underwent different dependencies (the Margraves of Meissen in the 14th Century, the Saxon Elector to the German Holy Empire in the 15th Century, *etc.*) and became a strong focus of resistance to the Napoleonic occupation in the early 19th Century. Jena was incorporated into the German Democratic Republic (GDR) in 1949. Since 1990, it has been part of the Free State of Thuringia in the united Federal Republic of Germany. A list of famous citizens includes the poets Johann Wolfgang Goethe and Friedrich Schiller, the reformer Martin Luther, the philosopher Wilhelm Schlegel, as well as, closer to our interests here, Otto Schott, inventor of the fireproof glass, Carl Zeiss, founder of the Zeiss Company, and the physicist Ernst Abbe, a partner of both Schott and Zeiss.

Schielicke's book covers, throughout several centuries, the astronomy-linked activities in Jena, going from the late Middle Ages (foundation of the university in 1558) to the current investigations on exoplanets, *via* the manufacturing of optical instruments at Zeiss and more public facets such as sundials from the Middle Ages and the popular local observatory.

The titles of the successive chapters give a more precise idea of the book's contents: 'The beginnings of astronomy in Jena and the first century of the university'; 'Erhard Weidel and his works (second half of the 17th Century)'; 'Astronomy in Jena during the 18th Century'; 'The foundation of the ducal observatory in 1813 and the first directors'; 'The directorship of Ernst Abbe (1877–1900)'; 'Astronomy at Zeiss from 1897 to 1945'; 'The university observatory in the first decades of the 20th Century (directorship of Otto Knopf)'; 'The beginnings of astrophysical research in Jena (1929)'; 'The popular observatory Urania'; 'Astronomy at VEB Carl Zeiss Jena during GDR times'; 'The Karl Schwarzschild Observatory in Tautenburg'; and 'The development of astronomy at the Friedrich Schiller University (second half of the 20th Century)'.

Several appendices gather together quite interesting data (an historical profile, successive designations of the professional institutions, list of directors, schematic chronology, and astronomical objects linked to Jena, such as small planets, and craters of the Moon, of Mercury, and of Mars). Then follow a dozen pages full of bibliographic sources. An index of names concludes the book. Probably some readers would have liked to benefit from a general index — and this could be a suggestion for a possible second edition. The book contains many illustrations (over 370), unfortunately all in black and white; one would certainly have appreciated the publisher using colour for the most recent pictures. But those two reservations do not remove anything from the intrinsic interest of this formidable historical work. One would wish similar compilations could be undertaken with the same care and the same luxury of details for all major astronomy centres of the world. — A. Heck.

James Van Allen: The First Billion Miles, by Abigail Foerstner (University of Iowa Press, Iowa City), 2007. Pp. 376, 16 × 24 cm. Price \$37·50 (about £19) (hardback; ISBN 0 87745 999 1).

During the last 50 years, we have sent robotic spacecraft to explore the region near the Earth, all of the planets except Pluto, and craft that are still heading toward the outer edge of the Solar System. The contributions of one man, James Van Allen of the University of Iowa, set him apart from all of the other early space pioneers as the father of spacecraft instrumentation. This biography of astrophysicist and space pioneer James Van Allen, by science writer Abigail Foerstner, places him in his times and beautifully tells us the history of the man and his scientific accomplishments. If you know anything about space exploration, you probably know of the Van Allen Radiation Belts that encircle the Earth, but you may not know that Van Allen is also an unsung hero of World War II.

Before I read this book, I was unaware that James Van Allen had helped to develop the proximity fuses used in anti-aircraft shells. Proximity fuses cause a shell to explode when it gets near an aircraft, so it does not have to hit the target in order to bring down the enemy plane. Shortly after thousands of these shells were delivered to the American troops in the South Pacific in 1943, the shells began failing to explode. Van Allen was sent out to the Pacific to find out what was the problem. He discovered that the batteries in the shells were deteriorating. Van Allen and a crew of navy gunner's mates worked around the clock in the heat and sultry humidity at Tillage to replace thousands of shell batteries. The secret proximity-fuse-armed shells were then very effective in shooting down hundreds of Japanese fighters in defence of Allied naval forces.

James Van Allen's greatest achievements centred around his teaching physics and astronomy at the University of Iowa, which in turn supported his efforts to explore the source of cosmic rays and his discovery of the radiation belts that bear his name. Foerstner gives life to what otherwise might be a dull reading of a scientist's life. She takes us to Van Allen's early attempts using weather balloons with instruments and a combination of weather balloon with an instrument package inside a rocket attached to the balloon. This was called a "rockoon" and was used to lift his instruments to higher elevations than the balloons alone could go. Van Allen worked with the German scientists who were brought to America after World War II to teach us how to build and launch the V-2 rockets that we had captured. These German rocket scientists were led by Wernher von Braun. Van Allen was able to insert various packages of Geiger counters and telemetry instruments into the rocket nose cones in furtherance of his search for the source of cosmic rays.

In addition to his teaching assignments, Van Allen also served as the head of the physics department and oversaw the construction of his instruments in the laboratory and workshops located in the basement of the physics building. A number of his graduate students worked on the instruments at Iowa, then went on to lead other spacecraft-instrumentation efforts at private and government facilities. Those former colleagues kept in touch with Van Allen and many of them came to the 90th-birthday scientific-colloquium celebration for him in 2004 October. One of the photographs in the book that I really like shows Van Allen at the colloquium holding up a T-shirt that states: "Actually I am a Rocket Scientist".

The subtitle of the book refers to the fact that when James Van Allen died at the age of 92 in 2006, his radiation detectors on board the *Pioneer 10* spacecraft were still working after 30 years in space and sending back data from a distance of over 8 billion miles from Earth. For her compelling and informative biography, Foerstner has combined the drama of early spaceflight failures and successes, Cold War politics that led to the 'Space Race', Van Allen's dealings with his numerous graduate students and their efforts to create the instrument packages for many space flights, and the events in Van Allen's personal life. She was able to interview her subject over a number of years before he died and was given access to his personal journals and papers. I enjoyed my look into the life and times of one of America's greatest rocket scientists and highly recommend this fascinating book. — ROBERT A. GARFINKLE.

Exploding a Myth: Conventional Wisdom or Scientific Truth?, by J. Dunning-Davies (Horwood, Chichester), 2007. Pp. 256, 18 × 12·5 cm. Price £22 (paperback; ISBN 978 1 904275 30 5).

The thesis of this book is that some (perhaps more than we care to acknowledge) basic scientific facts are cloaked in a sanctity which deep probing demonstrates that they do not merit. Such sanctity arises partly through popularism, and furthermore appears to be protected from investigations which could produce unsavoury verdicts. Thus daring to take on the Establishment almost single-handedly, the author attempts a brave stance, and one with which I have, or had, some sympathy. The cover blurb speaks of "many scientific theories that are not treated as theories but are being perceived as already proven" though without reference to specific scientific fields; and since the Foreword mentions invitingly the critical importance of learning "the Truth" about such diverse matters as medicine and the environment I was led to expect discussions on far-ranging topics, some of which have already entered my own limited realm of experiences. There I was disappointed.

The author has studied aspects of mathematics, physics, and cosmology, but the range of the subject materials in the book is almost exclusively associated with cosmology. Although it was beyond my own skills to judge competently if the criticisms are right or wrong, that was largely irrelevant. The task of a book reviewer is to assess whether a persuasive case is made; whether or not one agrees with the author is another matter.

In presenting the evidence for his thesis, Dunning-Davies delves in some detail into aspects of relativity, cosmology, black holes, hadronic mechanics, and entropy, quoting instances in which the originators (mostly illustrious) issued caveats that became glossed over as publicity swept a theory into vogue, and citing cases in which journals refused articles that pointed out errors or inconsistencies. Inasmuch as applications for grants to pursue research that might prepare alternative theories were likewise cast into outer darkness, the author could then rightfully complain that the Establishment was biassing scientific *results*.

That may all be correct, and what the author says may be true; he has a right to freedom of speech, and it is up to his scientific peers to study the specifics, in a spirit of honest scientific investigation, and examine their correctness. For my part, I was put off by numerous aspects of presentation, content, context, and powers of persuasion. I concluded (rather regretfully) that the author did not in fact do his subject the justice it may well deserve.

The publisher's reviews on the back cover admire the achievements of this book in exposing the naïve notion that all scientists are principally motivated by a deep desire to pursue knowledge for its own sake. One calls it "a beautifully written plea for the pursuit of research that is trustworthy and honest." I could not agree with those comments, nor could I imagine how someone not a cosmologist could accept such sweeping conclusions; there are quite a number of fields besides cosmology, and they are not even mentioned in this book, let alone considered. It is one thing to dwell on evidence of unproven hypotheses in cosmology, but quite another to postulate that since such things occur in that field they probably occur in many others, including medicine with its implications for humankind. That is unwarranted extrapolation; of all fields, cosmology must be the most unconstrainable when it comes to hands-on experimentation and control data.

Certain inconsistencies weaken the author's thesis. The Establishment is collectively accused of bias in not awarding a Nobel Prize to Sir Fred Hoyle, partly (it is suggested) because Hoyle was blunt enough to denounce formal religion in public (how Crick would have chuckled!). But the Nobel Prize is intended to reward scientific *discovery*, and since one of the author's strongest arguments is that a theory is not a fact, it is hard to reconcile awarding the development of a cosmology theory with a prize for a factual discovery.

Another curiosity is the quotation in full of the first 19 verses of the first chapter of the *Book of Genesis* to illustrate the rôle which fundamentalist religions may play in shaping our prejudices. In questioning whether *Genesis* was intended to be believed at face value, the author is presupposing it to have been written by people with the full benefits of modern scientific training, rather than as an account of creation in the poetry and symbolism of its time. The fact that *Genesis* adopts what has been translated as 'day' as the unit of evolution cannot be construed as 'evidence' that events happened as rapidly as Big Bang cosmology suggests; that is bending poetic licence to an extreme.

I would not describe the writing as "beautiful", and though it is definitely better than some, it is irrational with semi-colons, and sometimes the wording is rather turgid. The author often allows his pen to meander into broader generalizations about science, and too frequently has to commence a paragraph with "Returning to ....". Since he argues for the need to be completely open in scientific attitudes, it was all the more surprising to uncover a gender bias, and not exactly a subconscious one. In a passing reference to pulsars, the author mentions that they were detected accidentally by student Jocelyn Bell and that her supervisor Anthony Hewish was awarded the Nobel Prize for "his decisive role" in the discovery, with no comment on the selection of the Prize's recipients. And when later one reads that those who oppose "conventional wisdom" would probably not have survived were it not for the unswerving support of their wives, one starts to blink. Those courageous wives, we are told, "should be saluted and thanked by the entire scientific community, for their quiet moral support has to be recognized as a major factor in helping their husbands continue with their work in the face of so much hostile, scientifically unwarranted — indeed bigoted — opposition." Oh boy!

A book such as this would have the potential for substantial influence were it written from a more dispassionate angle, and if it included a survey of the situation in dissimilar fields of science. Enough of the cited examples of perceived bias by journal editors refer to the author's own papers (or rejections) that one senses a personal grievance rather than an altruistic examination of the situation. The book's unusually small format tends to reinforce the perception that it is one person's stand — albeit a brave one — against the Establishment. As mentioned earlier, it is not the reviewer's task to adjudicate on the correctness of the author's thesis, but to consider how well a book lives up to its claims, and how persuasive are its arguments. There is undoubtedly a lot of energy here, and the author spares no pains to offer explanations and examples. Where I felt unsatisfied was in the breadth of generalizations extrapolated from a very narrow field of experience (and presumably a male-dominated one at that), and from attitudes that seem too polarized to be fully realistic. Nevertheless, the book is a stimulating, sometimes passionate, challenge and I would be interested to learn how other scientists, *not* cosmologists, react. — ELIZABETH GRIFFIN.

One Small Step: The Great Moon Hoax and the Race to Dominate Earth from Space, by G. Wisnewski (Clairview Books, Forest Row), 2007. Pp. 390, 23·5 × 15·5 cm. Price £14·99 (paperback; ISBN 978 1 905 57012 6).

Health Warning: This book can seriously disturb your equilibrium.

The stated purpose of this book is to examine numerous pieces of "evidence" associated with the Apollo series of missions, and to draw conclusions as to whether they do or do not support what they claim. The book examines first what has been revealed concerning Russian manned flights, and dismisses all of it, before turning to the American scene and dealing a similar blow (but at much greater length).

Wisnewski starts out fairly mildly, and writes rather fluently — or at least, his translator from the original German has a fluent touch. However, after only a few pages it becomes clear that he has already formed his own opinions on these matters (indeed, the title itself says as much), and that his purpose is to demolish any shreds of doubt and to damn everyone and everything associated with positions of power, be they American or Russian but particularly NASA. As the narrative progresses the tone becomes increasingly cynical and more heavily sceptical and the opinions more drastically one-sided; nothing on the US side of the 'Pond' has an iota of credibility, all those mentioned are liars and fibbers, and anything, however porous, that could comprise the positive "evidence" that he wants is pounced upon gleefully, so much so that one doubts whether even first-hand experience would be sufficient to negate any of the theses he puts forward.

As an article of scientific research, the book does not answer at all well. The author does not appear to understand the rudiments of a scientific investigation, nor to appreciate the force, and the necessity, of scientific research as a quest for pure knowledge — not to mention the importance of not filtering facts. Some of the items of "evidence" which are laid before us are no more than subjective impressions, and events are portrayed as fabrications largely because the author deems them "unlikely". Right near the start we are assured that NASA would not have publicized such a risky undertaking as a manned Moon landing in case it went wrong; however, those risks have not vanished with time, and some 16 years later thousands of school-children tuned into public TV watched aghast as the school teacher on-board *Challenger* was blown very publicly to pieces.

The arguments that are set out might be more convincing if one could shake off the feeling that the evidence was hand-picked and frequently over-scrutinized. To base a judgement upon a nuance of words translated from Russian is as meritorious as declaring new science based upon noise spikes in one's data, and to denounce the photographs taken by astronauts on the lunar surface as "highly improbable" simply because this relatively young author has no experience of hand-held film-cameras and does not believe that anyone can be skilled enough to take good-quality films without umpteen practices, is scraping for crumbs. Yet in another place we are told that "the Apollo missions were simulated in every detail", so one may conclude that in fact they did indeed practice such obvious things ahead of time.

The book contains nothing about astronomy, and it is when the author fleetingly refers to astronomers that his naïvety is exposed through the assumption that his own interests should be central to the agendas of everyone else. A journalist by trade, the author has ample experience in asking the sort of "Have you stopped beating your wife?" questions whose responses can be twisted to suit any formula. He castigates ESO for not making the detection of the lunar module's landing-gear the early objective of the operations of the *VLT*, and similarly scoffs at CHARA's inability — *and unwillingness to try* — to detect and resolve objects on the Moon that have low surface brightness. He fails to understand ESA's recent scientific statement that "detailed data about the composition of the Moon's crust are lacking" by his asking if 382 kg of Moon rocks from six sites were not enough; how many astronomers would agree that if you've seen a few stars or galaxies you've seen them all and need no further telescope time? We are assured that the placing of retro-reflectors on the lunar surface was all a hoax too, because (so one source told him) the likelihood that any photons shot in their direction will be reflected back is minuscule. Nevertheless, lunar ranging results detected 2400 photons in a 30-minute experiment in 2005; why was that report overlooked?

Whatever the truth about the situations which the book purports to analyze, there is too much bias and prejudice here to make convincing reading. Much emphasis is given to the "proof" that the photographic and TV records were fabricated — matters of non-parallel shadows, additional sources of light, and lack of footprints dog the narrative from page to page. However, one has also to ask: if NASA was so intent on fraud, and spent so much money on a major fake, would it not have taken the trouble to make sure that its fakes stood up to close examination? And what about earthshine as a source of secondary lighting on the Moon? It gets no mention here. Rather, we are told with increasing venom how the American political machine has fabricated just about every success and catastrophe in history for its own ends. So, users of *HST*, beware! What you download from its website are just organized kaleidoscopic images. Not even that 700-million-dollar 'fix' can be believed; I expect they merely repaired the web interface. Still, all this would have come a little better from someone whose own glass house was a little less vulnerable.

This book is undisguised, unashamed, unapologetic anti-American propaganda. It has no place in an astronomical research library. — ELIZABETH GRIFFIN.

The Telescope: Its History, Technology, and Future, by Geoff Andersen (Princeton University Press, Woodstock), 2007. Pp. 256, 23·5 × 16 cm. Price £18·95/$29·95 (hardbound; ISBN 0 691 12979 7).

I love the opening line of this book: "If you live in a big city, you have no doubt heard talk of stars." A nice touch, placing the story of the telescope immediately into its astronomical context, while simultaneously highlighting one of the ills of our woebegone planet — light pollution. You might guess from this that the book's author is an astronomer, but you'd be wrong. And you don't have to read much further down the page to find a tell-tale slip that proves it — namely, a 40-fold underestimate in the number of stars in our Galaxy.

In fact, Geoff Andersen is an optical physicist, with a physicist's view of the telescope, and in the context of this book, that is a good thing. Although astronomy lurks in the background throughout — from the historical discussion in the first few chapters to the stunning up-to-date images in the colour section — the telescope itself is the star of the story. And, for the most part, it is comprehensively dealt with.

The book begins with an historical introduction, which is adequate despite the repetition of a few well-worn myths. ("[It] is called a Cassegrain telescope after the French priest Laurent Cassegrain who made the first working model." No he didn't — he only suggested the design, and, as it turned out, James Gregory had already attempted to make one almost a decade before Cassegrain's 1672 proposal. The first working Cassegrain telescope was probably made by Jesse Ramsden, more than a century later.) From there, the book goes on to introduce the basic forms of telescope, and discusses how aberrations and atmospheric seeing conspire to degrade performance. Chapters on image analysis, interferometry, and ground-based observatories follow, presenting the topics in an entertaining, accessible, and comprehensive fashion.

Having covered the basics, the book then goes on to explore the more exciting aspects of modern telescope design, covering both ground- and space-based techniques in optical astronomy. The author's job as a physicist at the US Air Force Academy reveals itself in chapters on surveillance, remote sensing, and laser communications, but the book quickly returns to astronomical themes with some key discoveries and a look at the future of the telescope.

In the midst of this is a chapter on non-traditional observatories, which describes liquid-mirror telescopes, solar telescopes, Čerenkov-type, gamma-ray telescopes, and gravitational-wave observatories. These topics are all interestingly and accurately presented, but the rather arbitrary selection seems to me to highlight the one big deficiency of the book. How, exactly, does the author define a telescope? You will look in vain in the index for any mention of radio telescopes, X-ray telescopes, or many of the other essential tools of modern astronomy. It's in this chapter on non-traditional observatories that the author owns up. "The emphasis of this book has been on optical telescopes, as the extension to other parts of the spectrum would constitute an overwhelming amount of subject matter." Thus, we understand that the author has written about only those technologies of interest to an optical physicist. Maybe that limitation should have been reflected in the book's title.

Notwithstanding that gripe, I do recommend this book. The writing is lively and engaging, and the concepts are well-presented at a non-specialist level. It is copiously illustrated, and there are useful appendices and a bibliography of printed and internet sources. Although not apparently aimed at the academic market, it would be a worthwhile read for undergraduate students wanting to gain an overview of modern techniques in optical astronomy. — FRED WATSON.

State of the Universe 2008, by M. Ratcliffe (Springer, Heidelberg), 2007. Pp. 192, 28 × 21 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 978 0 387 71674 9).

It has often been said that today's headline news will end up as tomorrow's trash. Well, in the modern spirit of recycling, this softback volume brings together 100 of the most significant astronomy-related press releases that were issued between 2006 April and 2007 March, together with full-colour reproductions of the relevant illustrations.

Few areas of scientific research are so prolific in their efforts at public outreach as astronomy, so if you find it difficult to keep up with the daily output and failed to take note of those releases at the time, then this compilation by writer and educator Martin Ratcliffe is a welcome reference.

However, many readers will find the most valuable section of the book to be a series of ten lengthy feature articles on a wide variety of astronomical subjects. Written by recognized experts in their field, these articles cover such diverse topics as 'The history and future of telescopes', 'The state of the Universe', and 'Building planetary disks'. Of less interest — particularly if you were one of the attendees — is a chapter describing the 2007 January American Astronomical Society meeting in Seattle. It is worth noting that, although the selection includes items about exoplanetary systems, press releases and feature articles referring to the Sun and other members of the Solar System are not included. — PETER BOND.

Why is Uranus Upside Down? And Other Questions about the Universe, by Fred Watson (Allen & Unwin, Crows Nest, NSW), 2007. Pp. 254, 19·5 × 13 cm. Price AU$24·95 (about £11) (paperback; ISBN 978 1 74175 253 3).

Not only has Professor Fred Watson made his 'escape' from Bradford (school) and Scotland (university) to become the project manager of the stellar-radial-velocity survey at the Anglo-Australian Observatory, Coonabarabran, NSW, but he has also taken the time to look away from his telescope. He has become a great communicator and the ace astronomy pundit on ABC (Australian Broadcasting Corporation) radio. Listeners ask him questions, and in this, his latest book, he treats us all to the answers.

And 'treat' is the right word. There is no pretension; there is no scientific obscuration, academic self-back-patting, and pedantic point scoring. Fred is convinced that everyone out there should, and can, enjoy and understand the fascination of science. They can be helped to appreciate the mysteries and the possible solutions of some of the contemporary big astronomical questions.

The variety of listeners' questions is impressive. "Is there anything in astrology?"; "Do astronomers believe in God?"; "Why does the Moon appear the same size as the Sun in the sky?"; "What is space–time?"; "Where did the energy of the Big Bang come from?"; "Where is the nearest black hole?"; "Do you think there is intelligent life out in space?"; "What makes planets round?; "How do we know some meteorites come from Mars?"; "Is it true that planets don't twinkle?"; "Will the Earth's magnetic field reverse?". There are many, many more, and all are answered in an engaging, sensible, amusing, and non-mathematical fashion.

I advise you to do two things. Buy this book for yourself. It will help you greatly when you are trying to tell your children and friends what astronomy is all about. Secondly, buy this book to give to your children and friends. It will make a great present, and one that will be hugely enjoyed and appreciated. — CAROLE STOTT.

Introduction to Astronomical Photometry, 2nd Edition, by E. Budding & O. Demircan (Cambridge University Press), 2007. Pp. 434, 23·5 × 15·5 cm. Price £45/$90 (hardbound; ISBN 0 521 84711 7).

One form of astronomical photometry has been performed almost unchanged for millennia and it is remarkable that in this day and age useful science can still be achieved with the naked eye. However, with the advent of modern detectors, precise and accurate measurements became possible, and in more recent years, with the falling cost and increasing availability of CCD detectors, high-quality photometry is possible with modest equipment by small university groups and determined amateurs. The relentless increase and falling cost of computing power means that it is also possible to do more with the measurements as well as make them, and these are the two main areas addressed by this book.

As with the original book, which was favourably reviewed in these pages (**114**, 130, 1994), the target readership of the second edition is university researchers and advanced amateurs, but having said that the mathematics used, the concepts discussed, and general rigour suggest that this is principally a university text. Despite this, there is much here for anyone keen to improve their understanding and practice of photometry, and to extract more from their measurements. The second edition follows the same structure as the original book but has been updated to take in the advances in technology since 1993, particularly the increased use of CCD detectors. There is also an additional chapter on period changes in variable stars.

The book has two central themes: the theory and practice of astronomical photometry, and the extraction of science from the measurements. The first theme leads off with a brief history of photometry and introduction to the basic terminology, and continues with field-radiation concepts, the notion of a black body, flux and effective temperature, luminosity and bolometric corrections — standard stuff but coherently presented. There is a useful discussion about the problems caused by broadband filters on photometric 'reduction'; that is, the process of going from raw counts to calibrated magnitudes and colours.

The next three chapters cover the main substance of the first theme. The first describes the theory of atmospheric and interstellar extinction and the standard relationships of the *UBV* system, and then discusses the design and application of other broadband systems like the Sloan Digital Sky Survey, intermediate- and narrow-band systems. There is a good introduction to the photometry of extended objects and photopolarimetry, with particular application to close-binary systems. The next chapter covers the more practical issues of the design and mechanics of photometers and the physics of different types of detector. Much of this is rather historical and is unlikely to provide real guidance to anyone actually trying to build a photometer, but it does give the essential background. However, there is a good discussion of CCDs, which is more relevant to today's practitioners, and a very useful description of the sources of noise in photometry. The next chapter concludes the 'practice' side of things with a discussion of what is usually called data reduction. This gives step-by-step instructions on how to determine the extinction coefficients and colour equations, and perform absolute calibration of the magnitudes, with worked examples. It also includes an important discussion of the errors. However, 'all-sky' photometry like this is much less practised these days; the vast majority of variable-star photometry is differential and the ways of getting the most out of this are also covered. Finally, there is a comparison of the different methods of obtaining times of minima/maxima from light curves.

The remaining chapters are given over to the business of analysing light curves for the physical parameters they can yield about different types of variable stars. As well as close binaries this includes the analysis of spotted and pulsating stars. Chapter 7 deals with the basics of eclipsing binaries, firstly with an instructive 'trivial' model and then a more realistic model with χ^2 minimization involving limb darkening but for spherical stars, and how this works in practice. The next chapter is the new one covering the different types of period changes seen in variable stars and describes the $O-C$ diagram and its complications, and how the changes can be attributed to angular momentum changes, apsidal motion, light-time effects, and other physical changes in the star. The next chapter builds on the simple photometric model to describe the development of a complex 16-parameter, general close-binary model. However, there is scant mention of the programs by Wilson & Devinney and Hill (LIGHT) that are generally available for this type of analysis. It would have been very helpful if the authors had given some practical advice on the application of these programs or even a brief review of their different approaches and how this works in practice. The next chapter describes the theory behind modelling spotted stars and how to obtain spot temperatures and distribution using the circular-spot model and the more general maximum-entropy methods, with particular emphasis on RS CVn systems. The final chapter describes how to obtain the physical parameters of pulsating stars from their light curves and contains a detailed description of the Baade–Wesselink method and its limitations.

The book is well indexed and each chapter concludes with bibliographical notes, which often contain useful comments, and a set of references. Although this is an updated edition and refers to new techniques and modern technology, there is much that harks back to single-channel photometry that begins to look increasingly historical. Having said that, this book has considerable breadth and depth, and will repay detailed study. Although the price may not be an issue for university departments — well, departments that don't do physics or astronomy — it will be a significant investment for even the keenest individuals. — CHRIS LLOYD.

Volcanism on Io: A Comparison with Earth, by A. G. Davies (Cambridge University Press), 2007. Pp. 355, 25·5 × 18 cm. Price £65/$125 (hardbound; ISBN 978 0 521 85003 2).

The study of the volcanic activity on Jupiter's satellite Io over the last four decades or so has been something of a saga. The earliest spacecraft to pass near Jupiter were *Pioneers 10* and *11*, in 1973 and 1974, respectively. Those probes focussed on magnetic fields and charged and neutral particles; *Pioneer 11* obtained an image of Io's north polar area but of too low a resolution to resolve significant details. The first detailed study of Io began with the passage of *Voyagers 1* and *2* through the Jupiter system in 1979. High-resolution images rapidly verified a prediction by Peale, Cassen & Reynolds, made only weeks prior to *Voyager 1*'s arrival, that the enormous tides raised by Jupiter in the solid body of Io would cause "widespread and recurrent volcanism". Giant umbrella-shaped plumes extending above the satellite limbs and brightly-coloured surface markings verified the presence of explosive activity and of surface flows of liquid, though there was initially uncertainty as to whether the surface flows were of liquid rock or liquid sulphur.

The *Voyager* images showed that some volcanic centres seemed to be transient while others were more long-lived. Just how long-lived was not fully appreciated until the arrival in Jupiter orbit of the *Galileo* spacecraft in 1995 December. Despite severe problems caused by the failed deployment of its main antenna, *Galileo* collected an enormous set of multispectral images and thermal measurements as it passed close to Io on six of its 35 orbits of Jupiter. Those data extended the list of identified volcanic sites from the roughly one hundred seen by the *Voyagers* to at least 160, and allowed a detailed study of the development of many sites in the 16-year interval between the *Voyager* and *Galileo* missions. In the process it confirmed that Io's activity is dominated by the eruption of liquid rock, just as on Earth and the other inner planets.

Ashley Davies' lavishly illustrated book not only describes in detail the ways in which various kinds of data about Io's volcanic activity have been obtained but also surveys in great technical detail the methods used to analyse them. At each stage there is a comparison with the analogous styles of activity and their investigation on the Earth. The author, who has been a major contributor to Io research during the last ten years, shows how it has been possible to deduce the volume rates and temperatures at which lava reaches Io's surface, and the ways in which lava interacts with sulphur-dominated surface volatiles to cause the explosive activity. In essence this is a technical book aimed at professional planetary volcanologists; but as an illustration of how one can go about extracting the maximum amount of information from a spatially- and temporally-limited set of observations it must be of interest to a much wider readership. Finally, as a repository of just about everything we currently know about Io (and are likely to know for some time, with no new missions in view) it is an enormously valuable reference work. — LIONEL WILSON.

The Physics of Chromospheric Plasmas (ASP Conference Series, Vol. 368), edited by P. Heinzel, I. Dorotovič & R. J. Rutten (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 653, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 1 583 81236 1).

It is some time since I attended a conference concerned explictly with the solar chromosphere so I welcomed the opportunity to review *The Physics of Chromospheric Plasmas*. The chromosphere is a notoriously complex region and discussions of its properties can easily be overwhelmed by phenomenology; it was therefore encouraging to see the words "Physics" and "Plasmas" in the conference title. The conference was arranged to coincide with the eightieth anniversary of the first spectroheliographic observations at the University of Coimbra in Portugal, and the proceedings begin with a historical section. Keynote papers introduce each topic, with the shorter contributions adding specific examples of particular observations or theories.

Two broad advances in recent years are clear from the start: there have been significant improvements in the spatial resolution of the observations and in *ab-initio* modelling of the structure and heating of the chromosphere. Indeed, Rutten and Carlsson stress the importance of forward modelling through, for example, numerical radiative-hydrodynamical simulations, rather than semi-empirical modelling from the observed fluxes and profiles of the various emission lines. Personally, I regard these approaches as complementary, since the latter do provide a time-and-spatially-averaged constraint that must be satisfied. Also, unresolved stellar chromospheres show simple trends that require explanation. However, all would agree that future simulations will need to include the magnetic field and be extended to three dimensions. A talk by Fontenla *et al.* really belongs in this section, since it combines some forward modelling and semi-empirical modelling. They also stress the importance of including a wide range of lines, not just the few in the optical region, and show how the lines of CO could be formed in a mean (spatially averaged) model, rather than requiring a physically separate 'COmosphere'.

The papers on active regions and sunspots have less of a common physical theme, but the keynote talk by Mauas continues the discussion of chromospheric models to active-region and flare conditions and also some simple scaled models of stellar chromospheres. The improvements in spatial resolution and magnetic-field measurements are brought out in the section on prominences and filaments, with the review by Lopez Ariste & Aulanier containing a useful summary of how the Zeeman and Hanle effects contribute to observations of polarization. Hudson gives an excellent review of chromospheric flares, concentrating on the physical processes at work and the plasma parameters, while Berlicki stresses how chromospheric-line asymmetries during flares must be interpreted with caution. The discussion in the papers on active regions, sunspots, and flares is mostly restricted to chromospheric lines in the optical region (Hudson's paper is an exception), which means that much potentially useful information is not included.

The penultimate session was concerned with long-term solar variations in the chromosphere, and the final session returned to instrumentation for solar observations. It is good to see that advances continue to be made and that world-wide the work is still finding support.

Overall, the keynote talks do succeed in bringing out the recent improvements in observations, theory, and numerical simulations and provide a valuable account of modern work on the solar chromosphere and what is required for further progress. The proceedings will be useful to a range of researchers, from graduate students to specialists in the field. The production has two shortcomings: some of the reproductions of high-resolution observations could be better and the on-line version is required to understand the figures that include colour. — CAROLE JORDAN.

15th European Workshop on White Dwarfs (ASP Conference Series, Vol. 372), edited by R. Napiwotzki & M. R. Burleigh (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 668, 23·5 × 15·5 cm. Price \$77 (about £38) (hardbound; ISBN 978 1 58381 239 6).

Thirty-two years separate this 15th workshop from the first in Kiel in 1974, not quite so long as the 40 that separate my most recent white-dwarf (WD) paper from the 1967 first. Neither of 'my' topics (gravitational redshifts, coronal X-rays) features prominently in the proceedings. You will, however, find several fine summaries of masses determined in other ways (the range is 0·25 to very nearly 1·4 $M_{\odot}$ and the mean DB is a smidge heftier than the mean DA, though the actual numbers differ between two chapters by more than the nominal errors) and discussions of WDs that are X-ray sources by virtue of hot photospheres or drizzling companions. Curiously, in a large sample toward the Galactic Centre, the CV sources are harder (also of course fainter) than the neutron-star and black-hole ones. Also well represented are the quests for binary-white-dwarf progenitors of Type Ia supernovae (no very persuasive candidates), the strongest magnetic fields (apparently fossils of the strongest fields in Ap stars and likely to be found in the more massive WDs), and the edges of the instability strips (the ZZ Ceti one is probably purer than the PG 1159 but still probably not entirely so).

This is the sort of volume that an expert might well keep on a handy shelf till the next one arrives, in order to look up names (stars and astronomers) and numbers. On the other hand, if you have no other pictures of members of an SOC Morris dancing in flowered hats, you might want to keep it forever. — VIRGINIA TRIMBLE.

Binary Stars as Critical Tools and Tests in Contemporary Astrophysics (IAU Symposium 240), edited by W. I. Hartkopf, E. F. Guinan & P. Harmanec (Cambridge University Press), 2007. Pp. 519, 25·5 × 18 cm. Price £62/$110 (hardbound; ISBN 978 0 521 86348 3).

Prior to 2006, the last IAU-sponsored meeting on binary and multiple stars had been in 1992, since when there have been substantial developments in techniques of radial-velocity measurement, interferometric astrometry, wide-field photometry, and data analysis. The Czech Republic has been one of the centres of binary-star research in recent decades, so the IAU General Assembly in Prague presented a natural opportunity for a symposium to discuss these developments, as applied to both close (interacting) and wide systems.

Nearly 400 participants registered for the symposium, evincing the extensive interest in the field (as well as the convenience of drop-in attendance for those involved in other parts of the Assembly, perhaps). Nevertheless, my impression of the content is that recent progress has been evolutionary rather than revolutionary, as indeed one might expect for such a venerable research area. Interferometry, though not a particularly novel approach, is undergoing a resurgence, but is still so challenging technically that results are rather few, and restricted to specialist groups (often the instrument developers). It's perhaps the dedicated, wide-field photometric programmes, originally developed for gravitational-microlensing surveys, that are generating public-domain data of the widest interest and applicability, from studies of binary incidence and formation to determinations of fundamental stellar parameters. However, many of the presentations and reports detail studies of individual objects, of greater or lesser significance, using more or less traditional methods of observation and analysis.

The proceedings volume is one of the first from Cambridge University Press since it won the IAU publications contract, and has a new format to which we will all, no doubt, soon become accustomed. On the plus side, the overall design and production standards are a significant improvement over the old ASP volumes in my view (although there is a concomitant price hike, albeit thankfully not back to pre-ASP, Reidel levels). Furthermore, there is a sensible new hybrid publishing model, whereby reviews and write-ups of longer papers are printed in full, while only short abstracts of posters are listed, with full versions generally made available on-line. However, the implementation of this approach still needs significant work, by both publishers and editors. Anyone using the author or object indices in the present volume may very well be puzzled initially by 'missing' pages in the proceedings, since the indices don't give URLs, or even mention the printed/on-line pagination convention. Elsewhere, the same generic link is provided for all on-line material, leading to a CUP site that treats the entire 'IAU Proceedings Series' in the format of a single *journal* (which evidently includes at least both colloquia and symposia; I'd guess that the *Transactions* and *Highlight of Astronomy* will also be lumped in).

After a bit of tiresome clicking I figured out that Symposium 240 is assigned to 'Volume 2' of this strange 'journal'; but even having worked out the site navigation, it still takes as many as six clicks to get to any particular on-line paper from the (single, generic) URL in the printed version. Not good enough! What's more, anyone without access to a 'journal subscription' is going to get stung for £15 for each and every associated on-line paper they might want to access — which could come as an unpleasant surprise to anyone rash enough to buy a personal copy of the book. — IAN D. HOWARTH.

The Central Engine of Active Galactic Nuclei (ASP Conference Series, Vol. 373), edited by L. C. Ho & J.-M. Wang (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 772, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 307 2).

Meetings in China are increasingly popular as that country starts to flex its considerable astronomical muscles. Many nations, including the UK, are seeking to collaborate with China as it increases its investment in astrophysics and space science. These proceedings summarize a workshop about the central engine of AGN, held at the Xi'an Jiaotong University in Xi'an, a city best known today as home of the famous Terracotta Army.

The study of AGN sometimes feels as if it has been going on since the Qin Dynasty, with progress being made fairly slowly. The workshop brought together a significant fraction of the world's AGN experts for a packed agenda covering everything from dust to relativistic jets. As ever with this type of publication, I would have appreciated more review-type talks rather than many, many short papers. But nevertheless the main areas of interest are all covered in some detail. I found the papers on outflows and the torus of most interest. The nature of the feedback between the central engine and the AGN host galaxy is probably the liveliest research area today. That said, all AGN pundits should find something relevant in this volume.

As usual with the ASP, the price is reasonable for a 772-page book. These proceedings will be of use to graduate students and should be in every good university library. — PAUL O'BRIEN.

Astronomical Data Analysis Software and Systems XVI (ASP Conference Series, Vol. 376), edited by R. A. Shaw, F. Hill & D. J. Bell (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 731, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 313 3).

Every year so many new astronomical software packages and on-line services become available that it can be hard to keep up. This volume from the 2006 ADASS Conference, like the earlier ones in the series, helps solve this by providing a platform for new projects to shout their wares. But not just new projects: one can learn here about new features in old friends like NED and *Simbad*. The latter is covered by two papers, one covering its evolution since 1972, the other describing its recent re-incarnation using Java and PostgreSQL. One notable feature of this year's proceedings is a number of papers on how to handle the huge data flows from the telescopes of the next generation. The *LSST* appears to be in the lead with its expected flow of some 15 petabytes/year, but the mere terabytes from other telescopes also present interesting problems and ingenious solutions. Another interesting theme this year is 'Software as a Service', recently seen in the commercial world with, for example, Google Docs. The provision over the web of scientific-data-analysis services is still in its infancy, but perhaps if enough people start using facilities like HERA (at GSFC) and the *XMM–Newton* SAS (at ESAC) they will improve to the point at which we no longer need to wrestle with the installation of complicated packages on our own PCs.

One surprising contrast with all this cutting-edge stuff is that ASP conference papers still have to submitted in camera-ready form, having been written in Latex (invented in 1983), and that it still takes over a year for the printed volume to appear, by which time the software universe has expanded still further. One wonders for how long this can continue. — CLIVE PAGE.

OTHER BOOKS RECEIVED

New Solar Physics with Solar-B Mission: The Sixth Solar-B Science Meeting (ASP Conference Series, Vol. 369), edited by K. Shibata, S. Nagata & T. Sakurai (Astronomical Society of the Pacific, San Francisco), 2007. Pp. 596, 23·5 × 15·5 cm. Price $77 (about £38) (hardbound; ISBN 978 1 58381 237 2).

Held in Kyoto in 2005 November, these are the proceedings — somewhat remarkably — of the *sixth* science meeting on Solar-B, which wasn't even launched until 2006 September. Now operating successfully as the *Hinode* mission, designed to study the interaction between the Sun's magnetic field and its corona, the spacecraft's instrumentation and science aims are discussed in this volume. One wonders how many conferences will be spawned by the actual science data of this successor to *Yohkoh*!

THESIS ABSTRACTS

A NEAR-INFRARED VIEW OF QUASARS AND THEIR HOST GALAXIES

By Natasha Maddox

The debate regarding the fraction of the total quasar population that has eluded detection at optical wavelengths is on-going, with several recent studies arriving at seemingly discrepant results. An understanding of the shape of the quasar spectral-energy distribution (SED) over a large range of wavelengths, and the effect the host galaxy has on the shape of the combined quasar-plus-host SED, is essential for correctly interpreting the results of surveys performed at both optical and near-infrared (NIR) wavelengths. Employing a relation between the magnitudes of quasars and the magnitudes of the host galaxies, and a quasar luminosity function derived from a large, optically selected quasar sample, simulations are performed to predict the unreddened number–magnitude and number–redshift distributions of quasars that would be observed in hypothetical surveys at optical and NIR wavelengths. As anticipated, the host-galaxy flux contribution increases at longer wavelengths, with the effect detectable to $z \leqslant 2$ for a deep, K-band-selected survey.

As technological advances are made, new observing techniques become available to assist in the search for dust-reddened quasars. The newly available *UKIRT* Infrared Deep Sky Survey (UKIDSS) provides large-area coverage with deep photometric imaging to enable quasar selection in the K band, where reddening by dust is less severe. The high-quality data is used to create a clean, well-defined sample suitable for multi-object spectroscopic follow-up. Nearly every morphologically stellar candidate was observed spectroscopically, along with a large number of extended candidates. For the remaining candidates without spectroscopic data, photometric redshifts and classifications were computed with the aim of eliminating the possibility of the existence of a large number of dust-reddened, morphologically extended quasars within the unobserved candidates. A non-negligible number of morphologically extended broad-line quasars are confirmed, along with stellar quasars with colours and spectra consistent with being dust-reddened, illustrating the ability of the NIR selection successfully to target quasars with non-standard SEDs in addition to 'standard' quasars.

K-band selection has been shown to identify routinely as quasar targets objects that are excluded from optical selection. The NIR quasar sample contains a higher fraction of broad-absorption-line quasars than that found in optical surveys, and selects quasars with red optical–NIR colours, ranging from colours consistent with $E(B-V) = 0{\cdot}1$ to greater than $E(B-V) = 0{\cdot}5$. There is also a small population of unclassified objects with such red colours that they do not appear in optical catalogues at all. Based on the observed $i-K$ *vs* redshift colour distribution, it is estimated that less than 10 per cent of the quasar population is excluded from the *K*-band-selected sample due to dust reddening, compared to $\sim$ 30 per cent missing from a sample selected in the *i* band. This rules out the extreme results extrapolated from recent studies which claim as much as 60 per cent of the quasar population is currently excluded from optically selected samples. The *K*-band selection is also effective at uncovering narrow-lined type-2 objects, a number of which may be intrinsically bright enough to belong to the elusive high-luminosity type-II class of obscured quasars. The observed number–redshift distribution of the *K*-band-selected quasars is consistent with the distribution predicted from the simulations, reinforcing the importance of understanding the quasar SED for interpreting observational results. — *University of Cambridge; accepted 2007 November.*

A full copy of this thesis can be requested from: N.Maddox.03@cantab.net

The Luminosity and Baryonic Mass Functions, and Their Evolution

By Leda Sampson

A subject of concentrated studies in extragalactic astronomy nowadays is the determination of the galaxy luminosity function (LF) and baryonic mass function (BMF). Both these functions are intrinsically related to the physical processes responsible for galaxy formation and evolution and are thus fundamental to putting tighter constraints on such models.

The LF evolves with time, and so does the BMF. In the local Universe, three major processes are responsible for their evolution: (*i*) simple stellar evolution, (*ii*) quiescent star formation, (*iii*) mergers of galaxies. To constrain the first two factors, we need models of stellar-population synthesis and these can be obtained from the literature. However, to constrain the third factor we need measurements of merger fractions as a function of Hubble type (HT), and this information cannot be found in the literature.

Here we determine the LF and BMF in the local Universe, and calculate their evolution within 1 Gyr. The total LF is determined by combining different datasets in different magnitude intervals. Type-specific LFs are then computed in two different ways: by taking galaxy fractions from catalogues based on visual inspection, and classifying 200 000 SDSS galaxies using neural networks. Total and type-specific BMFs are obtained by adding stellar and H I and H_2 gas masses as a function of HT and absolute magnitude. Stellar masses were obtained through magnitude-dependent, initial-mass-function (IMF)-independent mass-to-light ratios. Such detailed determination of mass-to-light ratios has not been done before and is one of the most important aspects of this work.

Integrating the mass functions over their full mass ranges, we calculated matter densities (Ω, in units of the critical density) in stars, H I, H_2, and baryons. In general, our results are in good agreement with other studies from the literature. The results for $\Omega_\star$ are independent of an assumed stellar IMF and are about 20% lower and 67% higher than what is obtained assuming a Salpeter and a Chabrier IMF, respectively. We find 60% of the stars in the local Universe to be in discs and 40% in spheroids.

To constrain the evolution of the LF/BMF due to galaxy mergers (the active factor), we created a catalogue of 294 merging systems selected from the Sloan Digital Sky Survey. We find that 65% of our sample consists of star-forming mergers and 35% of dry (kinematic) mergers. Overall, we estimate that (0·4–2·0)% of all SDSS galaxies are going through a merger event at the present time. The active evolutionary factor was then calculated by constraining the numbers of galaxies participating in a merger (in the local Universe, on the timescale of 1 Gyr) as a function of HT and absolute magnitude, and determining the extra luminosity factor added to merger remnants if star formation happens during the merger. The evolution due to stellar fading and quiescent star formation (the passive factor) was calculated by creating models of stellar-population synthesis to represent the evolution of galaxies as a function of HT and absolute magnitude.

Applying both evolutionary factors to the LF/BMF we conclude that neither the passive nor the active factors cause any distinguishable evolution on the LF/BMF within 1 Gyr. Merger fractions are very small in the local Universe, but this was probably not true at higher redshifts. In the distant Universe, merger fractions must have been much higher than those we observe locally if mergers indeed were major drivers of galaxy evolution. — *University of Cambridge; accepted 2007 November.*

A full copy of this thesis can be requested from: leda@cantab.net

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A TRIBUTE

Sir Arthur Charles Clarke (1918–2008)

Sir Arthur Clarke, who died in Sri Lanka on 2008 March 19, was a man of many talents. He was a brilliant writer, a good scientist, and above all a visionary. I feel well qualified to write about him, because we first met when I was twelve years old, and we were fellow members of the British Interplanetary Society, which in those pre-war days was widely regarded in the same sense as the Flat Earth Society. Despite the five-year difference in our ages, we struck up an immediate friendship, which proved to be life-long.

There are two types of science fiction: one type is based on the best available facts, while the other is pure fantasy. Arthur wrote both types, but he managed to create a bridge between them in a way not managed by any other writer, apart probably from Isaac Asimov. Of course, he is best remembered for his 1945 article in *Wireless World* in which he made an uncannily accurate forecast of communications satellites, but we must not forget his other predictions which have since been borne out, as well as others which have not — yet. He was right in saying that men would reach the Moon before 1970, though he thought then that the first travellers would have reached Mars well before the end of the century, and that in the foreseeable future Mars would be 'terraformed'. Whether this will ever happen remains to be seen.

One of his short stories, *The Sentinel*, was the basis for the film *2001: A Space Odyssey*, which is still being shown regularly today, and is always regarded as one of the great classics of the cinema. Its sequels did not have the same sort of impact, but sequels seldom do!

Arthur Clarke is no longer here, but he will long be remembered, and he inspired millions of people. He was, without doubt, one of the greatest visionaries of modern times, and he will be greatly missed. — PATRICK MOORE.

Here and There

BLOW ME!

Many people believe the moon landings were a hoax set up in a US film studio. Most contentious factors are the lack of stars and the apparently fluttering US flag. Nasa's retort is that the stars in the background were too dim to see, and that the moon's atmosphere made the flag ripple. — *Sunday Times Magazine*, 2007 October 7.

A TRIFLE PREMATURE

The front of Westminster Abbey, where Newton was interred with great fanfare on March 28, 1726 — *The Calculus Wars* (High Stakes Publishing, London), 2006, figure caption, p. 150.

BLINDINGLY OBVIOUS

"I have a powerful laser so even if the moon is out, we can still see the Southern Cross" — *Sawubona* (in-flight magazine for South African Airways), 2007 July, p. 78.

HARDLY SURPRISING

[M3] has many background galaxies. — *The Daily Telegraph*, July Night Sky.

RGO MEETS DAVY JONES

Royal Greenwich Maritime Museum. — *The Daily Telegraph*, July Night Sky.

Advice to Contributors

The Observatory magazine is an independent journal, owned and managed by its Editors (although the views expressed in published contributions are not necessarily shared by them). The Editors are therefore free to accept, at their discretion, original material of general interest to astronomers which might be difficult to accommodate within the more restricted remit of most other journals. Published contributions usually take one of the following forms: summaries of meetings; papers and short contributions (often printed as *Notes from Observatories*); correspondence; reviews; or thesis abstracts.

All papers and *Notes* are subject to peer review by the normal refereeing process. Other material may be reviewed solely by the Editors, in order to expedite processing. The nominal publication date is the first day of the month shown on the cover of a given issue, which will normally contain material accepted no later than four months before that date. There are no page charges. Authors of papers, *Notes*, correspondence, and meeting summaries are provided with 25 free reprints if required; additional reprints may be purchased.

LAYOUT: The general format evident in this issue should be followed. ALL MATERIAL MUST BE DOUBLE SPACED. Unnecessary vertical spreading of mathematical material should be avoided (*e.g.*, by use of the solidus or negative exponents). Tables should be numbered with roman numerals, and be provided with a brief title. Diagrams should be numbered with arabic numerals, and have a caption which should, if possible, be intelligible without reference to the main body of the text. Lettering should be large enough to remain clear after reduction to the page width of the *Magazine*; figures in 'landscape' format are preferable to 'portrait' where possible.

REFERENCES: Authors are requested to pay particular attention to the reference style of the *Magazine*. References are quoted in the text by superscript numbers, starting at 1 and running sequentially in order of first appearance; at the end of the text, those references are identified by the number, in parentheses. The format for journals is:
(No.) Authors, journal, volume, page, year.
and for books:
(No.) Authors, [in Editors (eds.),] Title (Publisher, Place), year[, page].
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(1) G. H. Darwin, *The Observatory*, **1**, 13, 1877.
(2) D. Mihalas, *Stellar Atmospheres (2nd Edn.)* (Freeman, San Francisco), 1978.
(3) R. Kudritzki *et al.*, in C. Leitherer *et al.* (eds.), *Massive Stars in Starbursts* (Cambridge University Press), 1991, p. 59.

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