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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday, 2013 March 8 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

D. Southwood, *President*
in the Chair

The President. I'd like to announce that the 2011 Keith Runcorn thesis prize is to be presented to Dr. David Kipping, for his thesis on the transits of extrasolar planets with moons, but I will give it to him after his talk this afternoon [laughter] so he has something to live up to. The first speaker today is Dr. Martin Elvis from the Harvard–Smithsonian Center for Astrophysics, and his title is 'Almost stars: quasars, asteroids and the future of space astronomy'. Quite a modest title!

Dr. M. Elvis. This meeting is being held one week from the 50th anniversary of the discovery of quasars. Maarten Schmidt's half-page letter announcing the enormous 15% redshift of 3C 273 was published on 1963 March 16 (*Nature*, **197**, 1040). The discovery of quasars caused a sensation in astronomy. How could so much power come from such a small region, as inferred from rapid variability in both the radio and optical?* Here I take a look at how much we understand about quasars at this milestone.

The answer is partly good, as our inadequate standard model — black hole, accretion disc, relativistic jet — is being augmented with accretion-disc winds that look as if they will explain a vast array of observations. But also partly disappointing as we still don't know why they are X-ray sources, 35 years after that discovery — which was my thesis — or why they evolve with cosmic time. The answer, as so often, is better, much better, observations. That leads me into discussing the apparently bleak future of space astronomy, and so into the future of spaceflight in general. The latter topics will be covered more fully in an article in *Astronomy & Geophysics*; here I concentrate on quasars.

Within ten years of the discovery of quasars we had a 'standard model' that explained the energy-production problem: a supermassive black hole produces

*Wrongly, as it turned out. The rapidly variable quasars were blazars, so the light-travel-time argument is invalidated by the relativistic beaming. Just as that was being realized, rapid X-ray variability was found, and the X-rays are essentially isotropic, so all is well.

the vast luminosity; an accretion disc explains the 'non-thermal' (actually a sum of black bodies) optical and ultraviolet spectrum and the peak temperature of a few 100 000 K; and the relativistic jet explains radio lobes, rapid variability, polarization of the radio, and apparent superluminal motion in VLBI images. Disappointingly, the 'quasar standard model' has nothing to say about why some quasars make jets, nor does it predict the strong, broad emission lines that are the striking characteristic of quasars, or indeed any of the rich array of atomic absorption and emission features of quasars.

Fifty years on we should have explanations for all these awkward details. But we don't. Certainly we don't have a consensus. I believe we are, nonetheless, close to a detailed quantitative theory of quasars.

The key to progress is an observation so obvious it goes unremarked, like the dog that did not bark in the night. The spectrum of the highest-redshift quasar ($z = 7{\cdot}1$), found in 2011 by Mortlock *et al.* in the UKIDSS survey, is indistinguishable from the mean quasar spectrum from the Sloan Digital Sky Survey at a redshift of 1. This tells us that *quasars are simple*. The atomic features and continuum are unchanging over 13 Gyr of cosmic time, six decades of luminosity, and four decades of black-hole mass. That means that the physics governing these features must be robust. Once gas is thrown down within the sphere of influence of the black hole it always does the same thing. But the physics must yet be complex enough to produce the complex emission and absorption features observed.

The solution is radiation pressure, I believe. It is simple, robust physics, and if radiation pressure is not important close to the most luminous objects in the Universe, where can it matter at all? Radiation pressure comes in just three forms: electron scattering ('Compton scattering'), atomic absorption ('line-driving'), and molecular absorption ('dust-driving'). It turns out that each is important, both where they work and where they fail. Several teams have worked on this. Most notably, a series of papers by Norm Murray, Jim Chiang and collaborators from 1995 to 1998 applied the O-star line-driven-wind theory to quasars. Detailed hydrodynamic simulations by Daniel Proga bear out Murray and Chiang's analytical work on accretion-disc winds. But they do not yet give a complete description of atomic features in quasars.

Guido Risaliti and I (*A&A*, **516**, A89, 2010) tried an intermediate path, eschewing hydrodynamics on the grounds that the flow is almost always supersonic, but still numerical. Working in two dimensions is important as it allows the basic geometry to be seen. We didn't try to launch the wind off the accretion disc but simply assumed that discs always throw off material at some moderate velocity at all radii, and examined the fate of that wind as it is exposed to the quasar continuum. We find that several, otherwise mysterious, values are produced naturally by this model. There is (*i*) an inner Compton thick ($N_{\rm H} \sim$ few $\times 10^{24}$ cm^{-2}), producing the highly-ionized, high-but-sub-escape-velocity X-ray absorbers. This gas shields (*ii*) a middle region of lower ionization where line-driving is effective in producing an escaping wind with $N_{\rm H} \sim 10^{22-23}$ cm^{-2}, as seen in X-ray 'warm absorbers' and ultraviolet 'associated absorbers'. (*iii*) These winds reach escape velocity > 10 000 km s^{-1}, as seen in broad-absorption-line quasars. But the wind comes off the disc at a large angle, ~ 30°, because the force multiplier does not reach the high values (100–300) seen in O stars. Instead the wind escapes as soon as the multiplier exceeds $1/\lambda$, the Eddington ratio, ~ 10–30. (*iv*) This wind region is physically thin ($\Delta R/R \ll 1$), as sometimes observed, because this $N_{\rm H}$ is enough to remove all the accelerating UV photons. It is possible, but not yet calculated, that the high-ionization broad emission lines come from the escaping wind zone. Immediately outside the wind zone is (*v*) a zone of lower ionization, from which the low-ionization broad emission lines, also seen as X-ray eclipsing clouds, may arise. Being closer to pure Keplerian rotation, these lines will be better for measuring black-hole masses, as observed.

To find so many numerically correct predictions is unusual in quasar research. We think that this model is worth investigating further. It is also satisfying that all these features fit with the geometric and kinematic model I proposed in 2000, as that model is consistent with a wide range of observations, and this new model gives a physical underpinning to that structure.

This is all good, but it leaves many second-order details that must be explained. Moreover it does not address the origin of quasar X-rays, or evolution. So, even if we have found a new component to add to the quasar standard model, there is still much to work on.

And so we consider future observations. Quasars are the quintessential point sources. Yet we know their physical sizes from reverberation mapping, and hence their angular sizes. Directly imaging the inner structures of quasars at the 0·1-milliarcsecond scale needs kilometre-scale interferometers in space. The prospects for new, greater observatories in space are bleak though. At US$6B for one greater observatory, the *James Webb Space Telescope*, astronomy has hit the funding wall. There will be no new generation of flagship space observatories unless we can build them far more cheaply than at present. It is my personal view that this will only become possible when profits are being made from space. My bet is that asteroid mining, initially for platinum-group metals, is the most likely path to this goal.

Mr. H. Regnart. It is impossible to run a charity, such as the RAS, or any commercial or industrial organization without the use of capital. For those of us who have observed the events of the last few years we will surely be aware of the difference between the ethical, sustainable use of capital, and ideological, reductionist capitalism. And the answer to the points you make, to the extent that they are valid, is the use of competitive tendering. But I must point out, that although there is a great deal to be said — and I'm a Green Party member — for mining asteroids, rather than trashing indigenous people's homelands, I don't think there is a necessary connection between successful commercial activities in space and the provision of large instruments, even though I wish there was. So we have to go for what is appropriate, and some activities, such as large instruments, need public funding even if there is competitive tendering; and those of us who are in favour of Keynesian economics can put forward an argument in favour of them, on that basis.

The President. I don't think you need to answer!

Dr. Elvis. Thank you. But I could!

Mr. A. P. Bird. Speaking as an economist who specializes in the mining industry, there is one thing about your argument that bothers me. If you happen to find an asteroid worth 30 billion dollars' worth of platinum, the first thing that's going to happen after that is the price of platinum is going to fall, real big. Have you factored that into your calculations?

Dr. Elvis. There have been minimal studies of that. The current market for platinum is about 200 tonnes a year, and if you bring back 200 tonnes a year, the market will crash — that's correct. If you bring back 20 tonnes a year, then it probably won't have a dramatic effect on prices, I am told. But I am trying to make contacts with the business school and other people who know about these things to learn more. That has to be worried about, yes, but it's not my area of expertise.

Dr. G. Q. G. Stanley. Two points I'd like to make. One, that you can probably re-market it as space platinum, and charge a premium. [Laughter.]

Dr. Elvis. That works for the first billion dollars of jewellery. Jewellery is not an important part of the platinum industry, it's mostly used as a catalyst.

Dr. Stanley. And the second point is that considering that it has been about 40 years since we put a man on the Moon, or left near-Earth environment — what are your hopes for all of this?

Dr. Elvis. Quite good. That is, the reason we haven't gone back to or beyond the Moon in the last 40 years is that nobody knew why we were going to do it, and nobody could muster the capital or the tax expenditure to do it. I think if we can actually make a profit, that is plenty of incentive — in fact we will tax it as soon as it is available! [Laughter.]

Dr. Stanley. So it's a good way to bribe politicians to put money into space, by getting a return on it?

Dr. Elvis. A 'bribe' if you like — I think if you explain to them that it will help the economy, that's what they're there for.

Dr. M. Dominik. Currently, the outer-space treaty tells us that no one can claim possession on celestial bodies. Moreover, the exploitation of space has to benefit mankind at large — in particular taking into account the rights of developing countries. So that would tell us that a commercial exploitation would currently violate our space rules!

Dr. Elvis. First of all, if that's the problem, we have to get rid of those treaties. But it turns out it isn't really settled law at all. We actually had a talk at CfA last fall by Joanne Gabrynowicz, who is one of the experts on this, and she said that basically if somebody does it, that will start the law being settled. And it's a real problem. But the really cute thing is, you can't own a celestial body, but you can own a Moon rock — those are property of the US government, and who is going to take argument with that? So the question is, how big does it have to get? Or, if I take an asteroid and move it, does it still make it a celestial body or is it legally not one anymore? I had a neighbour at Harvard law school who got very excited when I talked about this. [Laughter.]

The President. I think I'm going to bring down the guillotine at this point; clearly this issue can be debated further. Thank you for an immensely interesting talk. [Applause.]

The President. I am now happy to introduce the Keith Runcorn thesis prize winner, Dr. David Kipping from Harvard–Smithsonian CfA, speaking on 'The transits of exoplanets with moons'.

Dr. D. Kipping. I'd like first to thank the RAS committee for awarding me this prize — it is a great honour and privilege to receive it. I'd also like to thank UCL, where I took my PhD; it was a wonderful place to conduct my research.

It is remarkable to consider that just two decades ago the notion of detecting the planets around stars other than our Sun was a topic of pure speculation and conjecture. Since that time, observers utilizing an array of techniques have brought forth an embarrassment of riches, leading to great insights into our place in the Universe and the frequency and nature of extrasolar planetary systems. One of the outstanding challenges, in not just exoplanetary science but observational astronomy as a whole, is the detection of a moon around an extrasolar planet: a so-called exomoon.

There are three grand motivations to achieving this lofty goal. The first is that moons may be intrinsically habitable, by which I mean that they may represent frequent temperate abodes for life and perhaps even outnumber planets as inhabited spheres in the cosmos. The second is that judging by our own natural satellite, the habitability of planets may be connected to the existence and type of companions in orbit. For example, the Moon is thought to stabilize the axial tilt of the Earth, as Jacques Laskar skilfully explained in the discussion meeting earlier this afternoon. Thirdly, I believe we will learn a great deal about planet- and satellite-formation theory by the detection of a large population of exomoons. In the example of our own satellite again, it is thought that the Moon formed through a giant impact, but with only one known example of this event, we have no idea how common or rare such occurrences may be — no idea how unique the Earth is.

Can we really detect an exomoon with current instrumentation? The *Kepler* mission is arguably the most successful planet-hunting mission to date with a haul of nearly 3000 candidates ranging in size from that of Jupiter to that of Mars, all found using the transit method. *Kepler* is sensitive to Earth- and sub-Earth-sized planets and the transits of a moon should reflect approximately the same sensitivity. Unfortunately, the largest satellites in the Solar System, with radii of around 0·4 Earth radii, would be just below *Kepler*'s threshold. Therefore, the ultimate question as to whether *Kepler* will detect exomoons or not is really a question as to whether moons larger than those found in the Solar System are common or not.

If such Earth-like moons do exist, it has been shown by Barnes & O'Brien in 2002 that they would be dynamically stable around a wide array of gas-giant planets for billions of years. Except for Jupiters which migrate in too close to their star or experience planet–planet scattering, there is no reason why such moons should be stripped over time. The real question is how initially to place a large moon around a planet. For regular satellites, which are those that coalesce from the initial circumplanetary disc, Canup & Ward have argued that moons are limited to grow to a mass of 10^{-4} of the planet's mass. This limit is due to the competing forces of increasing disc density leading to increased drag and shorter moon lifetimes. For moons which are captured or form through an impact, so-called irregular moons, there are no known limits to the maximum moon mass. The Pluto–Charon, Neptune–Triton, and Earth–Moon systems offer several counter-examples to the 10^{-4} rule and perhaps these types of moons offer our best hope for a large exomoon detectable by *Kepler*.

It is therefore quite plausible that *Kepler* could be successful in a hunt for exomoons, but how exactly could a signal be found? One powerful trick is to look at the timing of the host planet's transits. The moon tugs gravitationally on the planet inducing sky-projected simple harmonic motion (SHM) for circular orbits. The tugging causes deviations in the position and velocity of the planet during its transit across the host star. These deviations reveal themselves through transit-timing variations (TTVs) and transit-duration variations (TDVs), respectively. Furthermore, since the motion is SHM, the TDV effect displays a 90-degree phase shift to the TTV effect, leading to a unique moon-signature that we can use to discriminate against other hypotheses, such as a resonantly perturbing planet.

TTV is conceptually analogous to the astrometric technique of finding planets, since it measures deviations in the host object's position. TDV is conceptually analogous to the radial-velocity technique of finding planets, except that really we are measuring tangential deviations in the velocity here and not radial. Just as with astrometry and radial velocities, detecting both signals yields a unique solution for the orbit and mass of a putative exomoon. Best of all, these two types of observations come from the same data set — a precise photometric time series delivered by a telescope such as *Kepler*. These two dynamical techniques, which yield a moon mass, can be combined with transits of the moon, which yield a moon radius, not only to check for a self-consistent dynamical solution but also to derive the density of the moon, yielding insights into its internal composition. The idea and derivation of the TDV effect and the suggestion and methodology to combine these different measurements to eke out an exomoon signal were the major original contributions of my thesis.

With the theoretical framework for detecting exomoons laid out in my thesis, I have spent my time as a post-doc thus far attempting to apply these ideas to real *Kepler* data. Recently, I initiated the Hunt for Exomoons with Kepler (HEK) project, which seeks to determine the occurrence rate of large moons around viable planet hosts. For every planet we inspect, whether a positive or null detection, we derive a probability distribution for the mass ratio of a putative exomoon, and by the end of the project an ensemble of these posteriors will reveal the true underlying occurrence rate. We dub this occurrence rate η_{moon}, analogous to the occurrence rate of Earth-like planets, $\eta_{\oplus}$.

To finish, I will reveal results from our first survey for an exomoon. We analyzed seven *Kepler* planetary candidates displaying the correct dynamical properties to host an Earth-mass moon and sufficient photometric precision to detect it. Three of these seven stars exhibit stellar activity which led to spurious detections and hence we are unable to derive reliable upper limits, although we can say for sure that we do not see a clean signal. The other four also yield null results, but here we derive upper limits on the mass ratio of $<4\%$ to 95% confidence. Given that these planets have radii of around 2–3 Earth radii, these limits correspond to Earth and sub-Earth masses. With this initial survey complete, we are now moving on to other targets, including a focussed study of M-dwarf host stars where our sensitivity reaches even Moon-sized satellites.

The President. I think I'll get you to answer questions before you get your award [laughter] — full value for money!

Professor D. Lynden-Bell. Our Moon has an elliptical orbit — what's the effect of eccentricity?

Dr. Kipping. The Moon's eccentricity is quite low compared to what we've found for many exoplanets. But we do actually model the eccentricity in the orbits, and we include the eccentricity in the equations I showed at the beginning. It actually acts as an enhancement factor: if there is eccentricity, it means that these effects get a little bit bigger.

The President. Having reduced them to silence, you get your just reward. I'm very proud to present you with the Keith Runcorn thesis prize. Thank you very much! [Applause.]

The final talk this evening is the Eddington Lecture, by Professor Philip Armitage from Colorado: 'The turbulent environment of planet formation'. [It is expected that a summary of this talk will appear in *Astronomy & Geophysics*.]

The President. Questions, comments?

Professor S. Miller. I think on the time-scales you are looking at, the far-ultraviolet radiation from the star is going to be decaying quite rapidly. Presumably ionization breaks in the disc will also follow that; are you able to take that into account in your dynamical models?

Professor Armitage. Yes, that's right. Particularly on these large scales in the disc, we can get far-UV ionization, and it's quite critical. On these time-scales there are two sources: one is actually accretion itself and the other is the stellar activity. The models I have been showing you include just one estimate for the far-UV flux, but I would say that the greater uncertainty is on the recombination side of the ionization balance: there is a large but partially known dispersion in the far-UV fluxes; what we know much less about is how many small grains there are that will cause those electrons to recombine. So we are most worried about the recombination — that's probably the biggest uncertainty in the chemical physics of the disc.

Dr. Stanley. I notice from your graphs showing the animation of the semi-major axis against eccentricity throughout, you didn't show very much disc clearing from large-planetary formation, which other studies have shown. Can you possibly comment on why you're not seeing that?

Professor Armitage. The basic answer is that it takes longer. In something like the Solar System, after 100 million years a lot of this small material has been swept up. Actually, the specific movie I showed didn't have Jupiter present, and that will also have some effect in causing that terrestrial-planet-forming material to accrete.

Dr. Stanley. You also showed that you had enough time for a large planet to be formed and sufficient instabilities to throw some large objects out.

Professor Armitage. Yes, in those calculations, where we were looking at giant planets, if there was instability among the giant planets, it would typically destroy extrasolar proto-belts very easily; if there isn't, then there is no collisional evolution, and so they are quite stable. I think this is a bit of an issue — why we don't see more very massive debris discs that would last for a long time around solar-mass stars. It suggests that a lot of that debris has to be dynamically cleared out, which would have to be done by a giant-planet system.

Mr. N. Calder. I notice in one of your papers you use the lovely phrase "irrational exuberance" to describe any expectation of a quick resolution to these problems in the medium term. Do you think that there will be any early observations from facilities such as *ALMA* which will change things quickly?

Professor Armitage. I think the prospect of making this turbulent-velocity measurement is reasonably good, although it's a challenging measurement. There are other aspects of the problem which are harder to address — anything where there is a coupling between the disc properties and the collisional growth of small particles involves compounded uncertainties, and those are difficult things to tackle. For example, experiments on ice collisions are really only just getting started — almost all of the information we have about collisional properties in protoplanetary discs comes from looking at silicates, which is a reasonable approximation for the inner Solar System; but if instead one is interested in forming the cores of the giant planets, then icy materials, organic materials perhaps, are going to be much more important. So there are some really quite basic uncertainties that either need a lot more laboratory work or some observation that gives an insight into what is really going on.

The President. I think you've reduced them to silence; thank you again for an excellent talk. [Applause.]

I will close by re-iterating the unusual fact that, owing to the National Astronomy Meeting taking place in July, there will be an Ordinary Meeting held here on April 12. With that, I remind you of the drinks reception in the Library next door, immediately following this meeting.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday, 2013 April 12 at $16^h 00^m$
in the Geological Society Lecture Theatre, Burlington House

LYNDSAY FLETCHER, *Geophysics Secretary*
in the Chair

The Chairman. I am not Professor Southwood [laughter], nor either of the Vice-Presidents; everybody is indisposed today apart from me. I am Lyndsay Fletcher and I am the Geophysics Secretary. I have been asked to take over the organization of this meeting, and I hope that I do it in a fitting manner. The 2011 Michael Penston Thesis Prize is presented to Dr. Ryan Cooke for his thesis on 'Finding the first metals', and I have a cheque here for Ryan but you can only get it once you have given your talk! [Laughter.] And so onto the programme of talks; we have four talks today, and the first is by Dr. Matthew Middleton from the University of Durham and Universiteit van Amsterdam: 'Discovery of the first extragalactic microquasar: the doorway to understanding extreme accretion onto black holes'.

Dr. M. J. Middleton. The study of gravitationally collapsed objects began in the early 1960s with the advent of X-ray astronomy. Half a century later, it is now widely accepted that black holes — the most extreme solution to Einstein's field equations of General Relativity — power the high-energy emission from both bright active galactic nuclei (AGN) and some 30 X-ray binaries (BHXRBs) in our Galaxy. In both cases, their luminous nature can only be reconciled with infalling matter forming a viscous accretion disc, thereby liberating the huge gravitational potential energy into thermal radiation. The energy liberated in the disc peaks at the inner edge where circular, classical orbits are still stable. The position of this last stable orbit is set by General Relativity and is heavily influenced by the angular momentum, or 'spin', of the black hole. Application of classical mechanics allows us to infer a simple scaling of the peak energy with mass which is corroborated by observation; accretion discs around the supermassive ($10^{6-9}\,M_\odot$) black holes in AGN peak in the UV with the tail of the *Planck* curve occasionally extending into the soft (low-energy) X-rays, whereas accretion discs around the smaller, stellar-remnant (3–70 $M_\odot$) black holes peak in the X-rays.

Emission from the disc dominates at high rates of accretion; however, at lower rates where the spectrum is complicated (the origins of which are still a source of debate), we find separate evidence for the nature of accretion scaling in a simple way with mass. Powerful radio jets have been seen at low rates from both AGN and BHXRBs, providing direct evidence for a population of relativistic electrons being injected into magnetic fields. Given that their luminosities correlate with the accretion rate, the jets must be associated with the inflow, and indeed the radio brightness is found to scale with X-ray luminosity and central-black-hole mass in a log–linear relationship that connects AGN to BHXRBs.

In the highest-mass-accretion-rate AGN — the quasars — even more luminous jets are launched, appearing to the observer to be travelling faster than the speed of light. This superluminal motion is a result of the jets being launched at small angles to the line-of-sight at highly relativistic speeds, considerably faster than the jets at low accretion rates. Should these events be generically associated with accretion at high rates, then we should expect to see them in their smaller cousins, the BHXRBs. Given the strong evidence for scaling, it should come as no surprise that, in 1994, the first micro-quasar BHXRB was discovered. It now turns out that *all* BHXRBs can appear as microquasars when they transition from low to high mass-accretion rates. How these radio-bright jets are launched is still uncertain; it has been suggested that the power should scale as a function of the angular momentum as the jets 'tap' the spin energy of the black hole, whereas others suggest that the rate of accretion is the primary driver. Certainly the brightest events occur when the accretion rate approaches the theoretical maximum for spherical infall, the Eddington limit. Seen in only a very small number of BHXRBs — most analyses place the number at four or five — these extremely bright jets can also be accompanied by day-time-scale, rapid re-flaring once the accretion rate has peaked.

Understanding how the brightest jet events occur, how they couple to the apparently Eddington inflow, and how much energy and matter they carry, is important not only for an appreciation of black-hole accretion but also for the high-redshift Universe. The first quasars grew at rates close to Eddington and were fully grown by a redshift of 6–7. Assuming their growth wasn't constant but the result of many merger and/or accretion epochs, then it seems inescapable that there would have been powerful jets carrying matter and energy into the surrounding medium, possibly heating the young intergalactic medium and affecting the growth of the host galaxy. However, studying these events is non-trivial, the time-scales for accretion-rate changes are of the orders of decades to millennia for quasars, making observations impractical. Studying Galactic Eddington microquasars may then seem the logical solution — the accretion flow changes on time-scales of hours to days; however, there are only a very small number of these and their duty cycles, *i.e.*, how often they appear bright, is generally unknown. Importantly, whilst the radio emission can be readily observed, photoelectric absorption by neutral hydrogen in the Galactic plane (where most binaries are located) heavily attenuates emission from the accretion disc. We are therefore prevented from having a robust understanding of how the inflow and outflow couple, vital if we are to understand how the jets are launched.

As the Milky Way is fairly typical, we would expect to see microquasars in other galaxies. If viewed out of the plane of our Galaxy, that would mitigate the effect of absorption whilst substantially enlarging the sample size. The nearest massive galaxies are generally well monitored by X-ray satellites which combine considerable fields-of-view with high sensitivities and thus provide the best opportunity to detect a source rising to an X-ray luminosity consistent with Eddington for a 'standard' black hole (usually taken to be 10 $M_{\odot}$).

A candidate source in our nearest-neighbour galaxy, M31 (Andromeda), was identified by ESA's *XMM-Newton* satellite in early 2012, rising rapidly over the course of approximately two weeks to an X-ray luminosity consistent with Eddington for a 10-$M_{\odot}$ black hole. Although the major jet event would have occurred during the transit to the high luminosities, this still provided a unique opportunity to detect the first extragalactic microquasar (Eddington or otherwise) by searching for the bright, variable radio emission corresponding to the jet re-flaring. To obtain an initial detection we were awarded a one-hour observation with NRAO's *Karl Jansky Very Large Array* (*JVLA*), the most sensitive radio telescope in the world. This sensitivity was vital: scaling the flaring jets from known microquasars in our own Galaxy to the distance of M31 would potentially give only a marginally significant (2–3 sigma) detection based on the detector noise in a one-hour exposure. The observation detected an extremely bright radio counterpart with a luminosity of 0·5 mJy at the distance of M 31 and a statistical significance above the detector noise in excess of 40-sigma (*i.e.*, the probability that this is noise is vanishingly small). For comparison, placing the source in our Galaxy, it would be one of the brightest radio sources ever observed. In fact the data were of such a high quality that they allowed the detection of significant variability on the time-scale of tens of minutes. As the smallest size-scale for the emitting region is the light-travel time, this variability implies a source size of 5 AU (or the distance between the Sun and Jupiter) and therefore requires a compact jet (rather than something more nebulous) and in turn reveals the presence of a microquasar. Given the source size and luminosity, we infer a brightness temperature (that at which a *Planck* curve would need to peak in order to give the observed radio luminosity) comparable only to those Galactic microquasars viewed face-on. This suggests that the extreme brightness is similarly amplified by relativistic beaming with the jet aligned close to the line-of-sight.

A series of follow-up observations took place using *JVLA*, the *Arcminute Microkelvin Imager* (*AMI*) large array, and the *Very Large Baseline Array*, the highest-resolution radio interferometer on Earth. The latter confirmed the source position was fully consistent with the most accurate position for the X-ray source (as determined by NASA's *Chandra*) and placed a direct upper limit on the radio-source size of 1500 AU. The approximately daily monitoring with *AMI* and repeat observations with *JVLA* identified highly significant, day-time-scale variability, the same behaviour uniquely associated with re-flaring in the Galactic Eddington microquasars. Accompanying this remarkable radio variability, X-ray monitoring with *XMM-Newton* and NASA's *Swift* showed the spectrum to be dominated by the accretion disc which decayed over the course of 150 days. By comparing the luminosity of the source when dimmest, yet still dominated by the disc, to the well-studied Galactic BHXRBs, we place a robust upper limit on the mass of 17 $M_{\odot}$. Given the extreme radio brightness, day-time-scale rapid flaring, disc-dominated spectrum, and upper limit on the mass, we confirm the detection of the first extragalactic Eddington microquasar.

Our pathfinder project has demonstrated the viability of using the latest instruments to detect Eddington microquasars in nearby galaxies. Finding more of these sources and performing simultaneous X-ray/multi-wavelength observations will allow us to study the coupled behaviour of inflow and outflow, thereby testing models for the jet launching and constraining the proportion of mass and energy distributed into the surroundings. To this end we have an on-going programme with *JVLA* to monitor the nearby massive galaxies M 81 and NGC 2403. This will detect face-on Eddington microquasars with guaranteed follow-up in X-rays (*Swift*), optical and near-IR (*Liverpool Telescope*), mm (*CARMA*), and radio (*JVLA* and *AMI*). The results of this programme so far are very promising and will naturally complement the next-generation surveys of *ThunderKAT* and *VAST*, the *SKA* pathfinders.

The Chairman. Thank you very much, Matt. I'd like to open this presentation for questions.

Mr. J. C. Taylor. May I ask an embarrassingly elementary question? The Eddington limit in this case — how exactly is it relevant when the material that is produced is coming in one direction on the plane of the disc, but most of the emission is going out in a different direction?

Dr. Middleton. This is a major problem in X-ray astronomy in terms of looking at high-rate behaviour. The Eddington limit is set for spherical infall; we don't fully understand how that relates to a disc. In most cases, we think that we should be able to go above that, and there are several different ways you can do it. We should also keep in mind that the numerical Eddington limit as we know it is set for ionized hydrogen only (although this will certainly dominate in most cases); it's not set for anything else that's in the disc as well, so there are several things that makes that numerical value slightly inaccurate depending on how it is being applied. In terms of what we speculate happens to the disc itself, there are mechanisms that you can use, so you don't substantially break the Eddington limit. The best known means are to give in and lose material or swallow the radiation before it can blow your disc apart. In the first case as the disc gets very hot, radiation pressure dominates and the disc 'puffs up'. As the material is then farther from the black hole, the gravitational binding is lower so material can be lost in an equatorial wind. As a result of the changed geometry it's possible to 'beam' the emission into the empty cone such that the luminosity we observe is not isotropic and can appear above Eddington, although the accretion rate we subsequently derive may not in fact be so high. However, another important consideration is that as the disc is very thick when hot, the time-scale for photons leaving the disc are longer than the time it takes to swallow material, and we have a process called advection. As a result we have another way to cool the disc and maintain accretion above Eddington. So in reality, that number and its application to a picture of 'normal' accretion can be a bit misleading but it's used so generally, so widely, and can still serve as a limit for strange things happening that I really had to use it.

Rev. G. Barber. Going back to the original supermassive black holes, the cosmological ones, is there an age problem in the early Universe, in getting it to form quickly enough?

Dr. Middleton. I think the whole point is that by assuming a starting point of primordial micro-black holes, to get to supermassive black holes in such a short space of time really requires Eddington-rate accretion.

Mr. M. Hepburn. Is there any evidence of red-shifting in the emitted light?

Dr. Middleton. Not from this source, sadly. If the jet was moving away from us, and there was optical emission, then absolutely we could look for it (and we could also of course search for blue-shifting if it moved towards us, in optical emission). In this case we've got the jet coming towards us and only observations in the radio band, so it's difficult to look for lines that would allow us to perform that particular diagnostic, although for reference it's worth mentioning that these jets are usually kicked out at 0·98*c*.

Mr. Hepburn. And normally they go in both directions?

Dr. Middleton. Yes, absolutely, normally they're bipolar jets. In this case, because it's so far away we can only really get the radio source rather than resolve structure, although of course it would be fantastic to get that!

Mr. Hepburn. Certainly with SS 433 there is a net redshift.

Dr. Middleton. Certainly, and because SS 433 is so close to us, we're able to use it for some excellent diagnostics of jet emission.

The Chairman. I'm afraid we're going to have to stop the questions now, so thank you again. [Applause.]

Our next presentation is by Dr. Ryan Cooke. And in fact I'm going to have the great pleasure of awarding you your thesis prize before you give your talk. This is the award of the 2011 Michael Penston Thesis Prize to Dr. Ryan Cooke. Please, join me in congratulating him! [Applause.] Now you've got to give your talk on: 'Finding the first metals'.

Dr. R. Cooke. I wish first to express my gratitude for the donation from the Michael Penston thesis prize fund, which is a lovely idea that has encouraged me as a scientist. For that, I am very grateful. I am certain that I wouldn't be the researcher I am today if it weren't for the guidance and support that was offered to me by my thesis advisors, Max Pettini and Donald Lynden-Bell, in addition to many others — thank you.

The periodic table, as seen by astronomers, can be divided into two categories: the first elements — including H, He, and a small fraction of Li, that were primarily produced in the first 20 minutes after the Big Bang (through a process known as Big Bang nucleosynthesis); and 'metals' — encompassing the elements that were produced through other forms of nucleosynthesis (*e.g.*, stellar nucleosynthesis, spallation reactions, *etc.*). The primary goal of my thesis work was to uncover environments that have only seen the metals that were produced by the first stars.

The first stars formed out of pristine, Big Bang gas. To date, no one has found one of these first stars, and as a result we still know very little about this elusive first stellar population. One way forward is to find the rare astrophysical systems that were solely enriched with the metals produced by the first stars, and compare the measured chemical abundances to model calculations of metal-free nucleosynthesis. Hitherto, many studies have focussed their attention on the most metal-poor stars in the halo of our Galaxy. These second-generation stars are believed to have condensed from a cloud of gas that was enriched solely by the first stars; the atmospheres of the second-generation stars are encoded with the chemical signature of the first stars. My collaborators have opted to take a different approach. We have instead directed our attention to find the clouds of gas from which these second-generation stars formed.

To achieve this, we used a technique known as quasar-absorption-line spectroscopy, whereby the atoms residing in clouds of gas absorb the bright continuum emission spectrum from a more distant, unassociated quasar. Our attention was drawn to quasars at redshift 3, where the Lyman-alpha transition of hydrogen is redshifted to optical wavelengths. Some of these QSO sightlines were found to exhibit strong absorption from neutral hydrogen, which resulted in the absorption profile having Lorenztian damping wings for the Ly-α feature. These systems are called damped-Ly-α systems, or DLAs for short, and have the physical conditions that are conducive to forming stars. Based on Sloan Digital Sky Survey spectra, we identified the small fraction of DLAs that exhibit very weak (or apparently absent!) associated metal-absorption lines — these systems are the most likely clouds of gas to hold the chemical signature from the earliest stellar generations.

We re-observed these candidate metal-poor systems with echelle spectrographs on the world's largest ground-based optical telescopes. Our final sample comprised 22 DLAs with an iron abundance $Fe/H < 1/100$ of solar, which included four of the five most Fe-poor stars systems that have $Fe/H < 1/1000$ of solar. By measuring the chemical composition of these near-pristine clouds of gas, and comparing the chemical-abundance distribution to models of nucleosynthesis by the first stars, we were able to obtain a handle on the mass range, stellar mixing properties, and explosion energy of the early stellar populations. Here, I will describe three of the research highlights that have developed from this work.

The primary highlight of this research was the discovery of a gas cloud that exhibits a marked enhancement in the number of carbon atoms relative to the number of iron atoms. In general, most of the DLAs in our sample exhibited a C/Fe ratio that is similar to what is measured in the Sun. The chemically peculiar cloud of gas that we discovered contained more than four times the typical ratio of C/Fe atoms. This enhancement of C/Fe is believed to be a key nucleosynthetic signature of the first stars, and is also seen in some of the most metal-poor stars in our Galaxy. Indeed, three of the four most-Fe-poor stars in the halo of our Galaxy exhibit strong enhancements of C/Fe. At present, we have obtained good estimates for the abundances of C, N, O, Al, Si, S, and Fe. When the next generation of 30-m-class telescopes and bigger becomes available, we will be presented with the opportunity to more than double this number of elements, and uncover the relative element abundances for the Fe-peak elements, which we have recently shown can provide interesting bounds on the explosion energy of the early stars.

Another parameter of interest is the stellar initial mass function (IMF) of the first stars. For a long time it has been appreciated that the ratio of O/Fe atoms provides the best diagnostic for the shape of the IMF at the lowest metallicities, for two reasons: first, in the low-metallicity régime, O and Fe are exclusively produced by the nucleosynthesis from massive stars ($\gtrsim 10\ M_{\odot}$); and second, the O/Fe ratio increases almost monotonically with increasing stellar-progenitor mass. Although the O/Fe ratio has been measured in the most metal-poor stars there has been some controversy regarding the O measurements at the lowest metallicity. In short, there are four diagnostics to measure the O abundance in stars, and at the lowest metallicities, none of these mutually agree with one another.

To help alleviate some of this tension, we estimated the O/Fe ratio for all DLAs in our sample and found that to within the error estimates, the O/Fe ratio is constant for all DLA systems. This implies that all systems were enriched by the same IMF worth of stars. In fact, this observation is in very good agreement with the stellar O/Fe ratio, when the O abundance is derived from the [O I] 6300 Å line, which has long been thought to be the most reliable indicator of the O abundance. Intriguingly, there is tentative evidence that would suggest a rise in the O/Fe ratio at the lowest metallicities. If confirmed, this might indicate that the IMF of early stellar populations was more bottom-light than the second generation of stars. This would be in good agreement with current theoretical predictions for the mass distribution of metal-free stars.

Since most of the gas in these near-pristine DLAs is yet to be processed through cycles of star formation, these DLAs can also be used to measure the primordial chemical composition — the abundance of the elements produced in the first minutes after the Big Bang. Such measures not only provide a crucial estimate for the baryon density of the Universe, but can also indicate departures from the standard model of cosmology and particle physics. In some of the most metal-poor DLAs in our survey, we were able to resolve the high-order neutral-deuterium absorption lines from the associated neutral-hydrogen absorption lines. For one of these systems, we were able to obtain the most precise measure of the deuterium abundance to date. To take advantage of this unique opportunity, we developed a new software package that allowed us to measure carefully the baryon density of the Universe. Our measure of the baryon density was recently found to be in excellent agreement with the baryon density measured from the temperature fluctuations of the cosmic microwave background recorded by the *Planck* satellite. We also placed strong bounds on physics beyond the Standard Model.

The outlook for this line of research is exceptionally promising. We have already identified dozens of excellent metal-poor DLAs that are along the line-of-sight to faint quasars. Unfortunately, a reliable chemical-abundance analysis for these DLAs will have to wait for the next generation of ground-based optical telescopes with 30-m aperture or larger, which will feasibly be able to observe these faint quasars. Such studies will bring us ever closer to understanding the properties of the elusive first stars, in addition to performing high-precision cosmology by measuring the primordial chemistry of our Universe.

The Chairman. Questions?

Mr. Hepburn. That value of 0·4 in O/Fe. Is that with respect to the solar ratio of oxygen to iron, or is this an absolute ratio?

Dr. Cooke. It is relative to solar.

Mr. Hepburn. And what is the actual ratio?

Dr. Cooke. There are about ten times less iron atoms, a bit over ten times less.

Mr. Hepburn. Yes, but in terms of proton masses, it's only about four times I take it.

Dr. Cooke. That would be correct, yes. In terms of mass, there's about a factor of four.

Mr. L. Hardy. Do you know why it is that the first stars have an overabundance of carbon?

Dr. Cooke. There are a number of models that predict this behaviour; the simplest view that I can provide you with is that we know that iron is produced in the very central regions of stars, and then silicon and then oxygen and then carbon, and so forth. It's like an onion shell. And so, in the most massive stars larger than 10 $M_{\odot}$ the carbon gets ejected, in a wind perhaps, and most of the outside of the star is ejected during the explosion, whereas the inside collapses down into a black hole. And so the iron core tends to get held up in the black hole, whereas all or most of the carbon gets ejected. As nucleosynthesis proceeds, towards the highest metallicities, that signature gets washed away by all the other forms of nucleosynthesis, of both carbon and iron. So at the lowest metallicities that's what we suspect is happening.

The Chairman. I think that time has caught up with us! So thanks again, Ryan, and many congratulations again on your prize [applause].

Changing the subject quite dramatically now, our next talk is by Dr. Ian Bastow, from Imperial College, and he is talking about: 'Precambrian plate tectonics: seismic evidence from northern Hudson Bay, Canada'.

Dr. I. Bastow. I would like to talk today about a project I have been involved with over the past several years in northern Canada: the Hudson Bay Lithospheric Experiment, HuBLE. We have been thinking a great deal about precisely when plate tectonics may have begun on Earth, as well as the reasons for the existence of Hudson Bay. This is work I started while at the University of Bristol, but am continuing now at Imperial College in London. My collaborators here, David Thompson, Mike Kendall, George Helffrich, James Wookey, David Snyder, David Eaton, and Fiona Darbyshire represent both UK and Canadian sides of the Atlantic.

So what are shields? They are regions of Precambrian geology, which means that the rocks are older than 550 million years old. These regions are identifiable not only from the geological record, but in the seismological record as well. At depth they are characterized by their fast wave-speed. Examples include Fennoscandia, Australia, Siberia, Congo, West Africa, and the heart of North America. It is the Canadian Shield I will spend most of my time talking about today.

In Hadean times, the earliest part of the geological record four billion or so years ago, the Earth would have been significantly hotter and more ductile than it is today. Only after a period of cooling would the plates have had sufficient strength for mountains to form. Precisely when that transition occurred is very poorly understood, however.

What certainly surprised me as I first began to work on Precambrian geology was the extreme debate as to precisely when plate tectonics began on planet Earth: estimates range from as early as 4·1 billion years ago to as recently as one billion years ago. Some very distinguished scholars argue intensely at major conferences about the question of Precambrian plate tectonics and the heart of the debate is this: from a geochemical point of view, how does one melt hydrated basalt in the garnet stability field? That is, how do we melt material akin to the present-day oceanic plates, in the presence of water, at considerable depth in the Earth? Many argue that this can only be achieved in subduction zones, with the implication that rocks of this particular nature formed by processes that we recognize as modern-day-style plate tectonics today. However, other workers argue that tonalite–trondhjemite–granodiorite-type geology can develop due to melting of thick mafic plateaus, rather like that which we can see today at the Iceland hotspot.

So where do we go to try to address some of these problems? The Hudson Bay region of northern Canada spans more than two billion years of the Precambrian geological record, including some of the oldest rocks on Earth. At 1·8 billion years ago, the final stages of assembly of the present-day landmass of North America occurred in what is known as the Trans-Hudson Orogen (THO). At that time, present-day Quebec in the form of the Superior Plate collided with the Churchill Plate, which is made up of several Archean fragments including the Rae, Hearne, and Slave cratons. A really interesting piece of work a few years ago by Mark St. Onge from the Geological Survey of Canada, along with colleagues from the University of Oxford, suggests that the Trans Hudson Orogen was of similar scale and nature to the on-going Himalayan–Karakoram–Tibetan Orogen of Asia.

Specifically it was the exposure of medium and high-grade metamorphic rocks on southern Baffin Island that was cited as evidence for considerable uplift and erosion during the Trans Hudson Orogen. The rocks at the surface here would have formed at lower crustal depths in Palaeoproterozoic times, and a very good way to erode down to these depths is to have the whole region markedly uplifted, as would be expected if the region was Himalayan in nature. The challenge, though, is to improve our understanding of the deep seismic structure of the plates in this remote and ancient region. And that is what project HuBLE aimed to do.

The map here shows the various subdivisions of the Hudson Bay region, including the Trans Hudson Orogen. The triangles are broadband seismograph stations, which we deployed using light aircraft: De Havilland Twin Otters. The fieldwork was immense fun, with the pilots coming in to test the integrity of the surface they were about to land on, by bumping the wheels into the ground, rather like a golfer taking a practice shot with a divot. If the ground felt firm, they would turn the plane around and land, bumpily but successfully, on glacial deposits such as those shown here. I have to say that the mosquitoes, of which there were literally millions, rather spoiled the enjoyment of the work. But there you have it: a broadband seismic station.

So what can we do with these data? Well, one of the first things we do is study seismic anisotropy, the directional dependence of seismic wave-speed. Anisotropic olivine crystals will, if coherently aligned by mantle flow, result in the splitting of a polarized shear wave into two pulses, one travelling faster than the other. The orientation of the fast shear wave tells us about the orientation of mantle alignment, while the delay between the fast and slow shear waves tells us something about the strength of the seismic anisotropy and/or the thickness of the anisotropic layer. Using seismic phases such as SKS, we can study mantle seismic anisotropy *via* analysis of shear-wave splitting, and we present the results as vectors on a map. The arrow length tells us the strength of the anisoptropy, its orientation tells us what the fast polarization direction is.

The results from Hudson Bay, to cut a long story short, indicate strongly that plate-scale deformation occurred during the THO. There is a very close correspondence between anisotropic fast directions and the geologically inferred boundaries between the Superior and Churchill plates. Complex variations in splitting parameters, theta and phi, also point towards the sort of anisotropic fabrics that we might expect to be associated with plate-tectonic processes.

Another technique we can use to study our seismological data is called receiver-function analysis. Using the energy from distant earthquakes, we can isolate P-to-S-wave conversions from velocity discontinuities such as the Moho. The Moho marks the transition from approximately 6·5-kilometres-per-second wave-speed crust, to approximately 8-kilometres-per-second mantle. The timing of these P-to-S-wave conversions, and their subsequent reverberent phases, can inform us about the bulk properties of the crust: specifically, its thickness, and V_p/V_s ratio.

The receiver-function observations from Hudson Bay reveal some of the lowest crustal V_p/V_s ratios of anywhere on Earth — as low as 1·7. Closer inspection of the data in fact reveals an age-dependence on bulk crustal V_p/V_s ratio. V_p/V_s rises slightly when ones moves from the Palaeoarchaean Rae domain to the Mesoarchaean Hearne domain. Remarkably, however, across these two Archaean terranes, there is almost no variation in crustal thickness. One explanation for that is that during Precambrian times the hot, ductile crust was able to flow and homogenize in terms of depth.

One area where we do see a marked change in crustal thickness is the Quebec–Baffin Island segment of the THO. Here, crustal thickness is around 7 km greater than elsewhere across the HuBLE network. Adding twenty-five to thirty kilometres to the exposed high-grade, lower crustal rocks observed at the surface here would produce a crust that was Himalayan in scale. And so here is what we propose for the region: when the earliest rocks of northern Hudson Bay were forming in Archaean times, we suggest that ductile, plume/delamination processes may well have been dominant. By the time of the THO, however, the seismological evidence supports that of the geological record, which indicates modern-day-style plate tectonics was likely in operation.

Now, what about the other question I posed at the start of this talk? Why does Hudson Bay exist? The Bay, intact, is something of a distraction here; it is merely a giant puddle of depth only about 100–200 m or so. But below it lies the Hudson Bay basin, an accumulation of remarkably un-disturbed sediments that encompasses most of Phanerozoic times.

The Hudson Bay region is characterized by a broad geoid low, and some workers have explained this as the result of incomplete glacial rebound. However, it has also been suggested that part of the geoid anomaly is a mantle downwelling beneath the Bay. Also in question is the depth of the Laurentian keel — specifically whether or not it extends as far as the mantle transition zone at 410–660-km depth. One of the ways we can investigate these questions is by using receiver-function analysis to study the depths to the olivine-to-wadsleyite (the '410') and the ringwoodite-to-perovskite+magnesiowustite (the '660') phase transition in the olivine system. The opposite Clapeyron slopes of the olivine-to-wadsleyite and the ringwoodite-to-perovskite+magnesiowustite transitions produces a thinner transition zone for anomalously hot regions, and a thicker transition zone for cold regions. In the same way that we were able earlier to study P-to-S converted phases from the Moho, we can use receiver-function analysis to study the transition zone.

And here are some of the results from David Thompson's excellent PhD work. To cut a long story short, the transition zone is about as flat and normal as we could have ever imagined. This has the implication that no thermal anomaly characterizes transition depths beneath the Bay. The cratonic root doesn't impinge on the transition zone. If the geoid low and Hudson Bay basin owe their existence to a mantle downwelling, it is confined to the upper mantle. Finally, thermal effects due to the root (cold downwelling/heating due to insulating effect/small-scale convection) are also not in evidence. If they exist, they are below the detection threshold of the method we are using.

So I've told you a little about the work we have done in Hudson Bay, which I hope you are convinced has interesting implications for the onset of plate tectonics on Earth, and for the development of the Hudson Bay basin itself. What next? Well, this summer I will be initiating a project with colleagues in Canada and the USA to study seismically the crust and mantle structure beneath the transition from the Archaean Superior craton, all the way to the Palaeozoic geology of New Brunswick and Nova Scotia. This work is timely because it is coincident temporally with *USArray*, a major National Science Foundation effort to study the Earth beneath North America. I hope to come back and speak to you all one day about the results of this exciting new experiment. Thank you very much.

The Chairman. Questions?

Rev. Barber. Could Hudson Bay be an impact crater?

Dr. Bastow. People have proposed that for the Nestapoka arc, I think it's called. I believe, however, that most of the evidence that you normally find in the geological record associated with impact craters, such as the iridium layer, shocked quartz, and such like is not evident there. I could be wrong but I don't think it's evident.

Professor Kathy Whaler. So if your cratonic keel, or whatever, doesn't extend down as far as 410 km, what's the explanation for the geoid anomaly do you think? Because it's very broad, and as you said it's extremely deep!

Dr. Bastow. I mean the discussion that was going on here, back ten or so years ago, I think, with Jerry Mitrovica, in a paper entitled 'Going halves over Hudson Bay'. I think this was a commentary on the *Nature* paper by others, the halves referring to a combination of incomplete glacial rebound, and the mantle downwelling. So I'm not exactly sure what their status is on that nowadays, but certainly incomplete glacial rebound would contribute something to the geoid anomaly. Specifically how much of the total is due to incomplete rebound, I'm not sure. I'm not even sure if Jerry still agrees that it would be half and half.

Professor D. W. Kurtz. You're first question was, "when did plate tectonics begin?", and it leads me towards maybe your last answer: "when will it end?". The reason why I ask is that I wonder if it will stop in time for erosion to turn this into a water planet or the heat death from solar evolution.

Dr. Bastow. I've constantly thought back, not forwards I'm afraid. I'm not sure! When would the mantle stop convecting perhaps? I'm not sure. What is the consensus if any, on how long the Earth would take to cool to the point where that would happen? I'm not sure!

Professor Kurtz. You answer first! [Laughter.]

Dr. Bastow. You need to find somebody who studies the core. Kathy might have an idea.

Professor Whaler. Well, I was just thinking about that. Presumably what would happen is that once the mantle stops convecting the core also stops convecting. So we'll lose our magnetic field and we'll lose our atmosphere and so we'll lose our water, like Mars has done. So we'll never become a water planet. It's one answer!

Dr. Bastow. What is the consensus, if any, on the time it would take the core to cool sufficiently?

Professor Whaler. I think that's one of the big debates at the moment; when did the inner core initiate? People have changed their minds remarkably over the last couple of years, with the new *ab-initio* thermal properties for the core, which implies a real problem in driving the dynamo. So, I don't know.

Professor D. Lynden-Bell. Why has the Hudson Bay existed so long, if it's only a weak depression?

Dr. Bastow. That's an extremely good question. I should emphasize to people really how pristine all of the sediment layers are. We go all the way to the Cambrian, so it's 500 or so million years ago down there, but actually beautiful seismic reflectors are the basis on which this has been drawn. I realize this isn't a seismic image, but if I showed you one, it would still be incredibly convincing. People often debate the Hudson Bay, Michigan, and Illinois basins, specifically how they formed in the first place in the middle of the shields. It's a difficult question to answer. I suppose one of the reasons that they've managed to survive is that the shields generally are resistant to erosion and to break-up. Which is why we see them as the cores of the continents; but then, how on earth do you form them in the first place? I don't know, and I'm not convinced our results necessarily tell you a huge amount about that, except to say, I don't think a mantle downwelling tells you a great deal about why the base may have formed in the first place. People have worked on the question of whether or not a certain amount of extension back 500 or so million years ago would help to develop a depression, and we found seismic evidence for a certain amount of crustal thinning beneath the bay. Quite why rifting produces a circular bay, I have no idea. [Laughter.] Although it's worth remembering that the bay itself is just a puddle on the basin, so I'm not sure.

Professor Lynden-Bell. But surely that map itself shows you that there must have been some downwelling, because every layer is thicker in the middle.

Dr. Bastow. Yes ... now this was pulled off the internet about 20 minutes ago [laughter]. In fact this is an image of the Michigan basin, not Hudson Bay, I'm afraid!

Professor Lynden-Bell. Oh, is it the Michigan basin? It's the wrong basin!

Dr. Bastow. Yes, but they are all very similar in appearance, in terms of their stratigraphic record, though Illinois is another good one. They are really quite remarkable.

The Chairman. I'm sorry we're going to have to stop the discussion there, thank you very much! [Applause.] Our final talk for this afternoon is Dr. Keith Smith, of the RAS, on: 'Small-scale structure in the interstellar medium'.

Dr. K. T. Smith. Thank you for inviting me to give this talk, in which I will review what we know about the small-scale structure of the diffuse interstellar medium (ISM). I'll start by discussing the physical processes which govern the diffuse ISM, then run through the observations which can tell us how the ISM is structured on small scales, and finish with a discussion of whether the observations are indicating differences in density or ionization.

The ISM holds the raw materials for star formation, and the efficiency of turning gas into stars is governed by the physical conditions of the ISM — temperature, density, *etc*. Those conditions are driven by the physics and chemistry of the gas and dust.

The density, temperature, ionization, and dynamics of the ISM depend on: (*a*) heating by UV and X-ray photons, and photoelectrons from dust and polycyclic aromatic hydrocarbons (PAHs); (*b*) cooling by thermal emission from dust, and line emission from Lyα, Hα, [C II], [O I], *etc*.; (*c*) the pressure, including thermal, gravitational, and magnetic components; and (*d*) chemistry, especially the abundances of H_2, CO, and PAHs.

If we calculate the equilibrium properties of the ISM on the basis of our current understanding of those four processes, we get an S-shaped graph of pressure against temperature. The weight of gas in the halo pushes down on the plane of the Milky Way with a particular pressure, and in equilibrium the gas must be at the same pressure. There are then three possible equilibrium states — one at low density and high temperature (the warm phase), one at higher density and low temperature (the cool phase), and one in between which is dynamically unstable and so will quickly fragment into separate warm and cool regions. The 'three-phase model' predicts that there is a minimum size for the cool regions of about 1 pc.

The first indication that this might not be true came from interferometric observations of foreground Milky Way H I absorption towards extragalactic radio sources. These show large differences in H I column density over scales of about 20 AU (10^{-4} pc), and implied a population of very small and very dense (10^4 cm^{-3}) clouds in the ISM.

To learn more about this, observers turned to optical observations of binary stars, in which the interstellar absorption lines are compared towards two stars separated by much less than 1 pc. When this was first done in the 1990s, it showed significant structure of the ISM on scales of 700 to 6000 AU, in almost every direction which was examined. The three-phase model had predicted no structure at all on those scales. The observations again indicated very small, high-density clouds of neutral atomic material, with no evidence for H_2.

The existence of high-density clouds could be important, because out-of-equilibrium structures are signatures of energy input into the ISM, through the processes of galactic feedback, *e.g.*, supernovae. It might also provide a solution to some problems in interstellar chemistry, helping to explain the abundances of some simple molecules. They could be the seeds for molecular clouds, once sufficient column density builds up for H_2 to become self-shielding.

It's possible to extend the optical observations into two dimensions, by observing stars in clusters. This gives us maps of the interstellar absorption towards 452 stars in the globular cluster ω Cen. They show significant variations on scales of 0·1 – 1 pc.

The first theory to try to explain these structures postulated that they were caused by sheets or filaments of cold material. When these happen to line up along the line of sight they can cause the observed variations in column density without needing to violate pressure equilibrium. However, the clouds would need to be at very low temperatures (15 K), and there was no hypothesis for how they could form.

Binary-star observations are limited to certain directions and separations by the systems available, the seeing, *etc*. This can be circumvented by moving into the time domain, and observing a single star multiple times over several years. The proper motion of the star means the line-of-sight samples slightly different paths through the foreground interstellar cloud. Very small scales of about 10 AU can be sampled towards stars which are bright enough for extremely detailed observations.

I've recently used observations of the star κ Vel, which is bright enough that we could use ultra-high resolution to resolve fully the profiles of the narrow interstellar lines. These show large changes in column density over the 12 years of observations, and allow the determination of some of the physical properties. We found that the density has to be at least 2×10^4 cm^{-3}, but the temperature is typical for the cold phase, and the dimensions are not consistent with sheets or filaments aligned along the line of sight. This results in a pressure which is two orders of magnitude higher than the equilibrium value, so clouds like this one will evaporate quickly.

There might be a problem with these numbers, because we had to assume ionization equilibrium. When observers have looked at UV absorption lines they have found variations in minor neutral species, but not in dominant ions. This suggests that the ionization conditions are changing between lines of sight, not necessarily their density.

One way to test this would be to use pulsars to measure directly the H I column density using their high proper motions. Unfortunately results are inconclusive for those observed so far. An alternative is to look at emission or reflection from dust grains, which trace the density but are not affected by ionization.

Observations of the reflection nebula behind the Pleiades indicates a single power-law distribution from 1 pc down to 100 AU. *Herschel* observations of dust emission from the Polaris Flare region show the same power law on scales down to 2000 AU, with the same spectral index. The power law could be indicating that the structure is driven by turbulence.

Further evidence that there are real density variations, not just ionization changes, has come from UV observations of the fine-structure lines of C I. The ratios between the lines can be used to determine directly the pressure and temperature, although there are some degeneracies in the modelling. The results show that most interstellar material is at the expected range of densities, but that a small fraction (less than 0·1% by mass) is at very high pressure and density, more than 10^5 cm^{-3}, which agrees with the small-scale-structure observations.

The diffuse interstellar medium is structured on scales $<$ 1 pc, contrary to theoretical expectations. Observations show that the structure is ubiquitous, requires high-density gas, and may have a constant power spectrum. Ionization changes alone cannot explain the observations, and the variations could be a signature of energy feedback into the ISM.

The Chairman. Questions?

Mr. H. Regnart. Could what would be known on Earth as greenhouse-gas effects ever be relevant?

Dr. Smith. In what sense?

Mr. Regnart. A tendency for the nature of the gas to retain heat and energy more effectively because of differences in composition of the gases and the dust.

Dr. Smith. That's all taken into account in those models, in the heating effects. So optical-depth effects are in those models.

The Chairman. I was surprised to see that the photoelectrons from PAHs are considered energetically a dominant pressure term in this. Why is that?

Dr. Smith. It's not a pressure term, it's a heating mechanism, because there are particular UV wavelengths where there is not strong absorption by atoms, but there is strong absorption by molecules. And if you have a significant population of PAHs, then you knock an electron off the molecule, which will then collide with the atoms and other electrons of the gas, and heat the gas.

The Chairman. And that is sufficient?

Dr. Smith. In fact one of the reasons why PAHs were first postulated as being a significant constituent of the ISM was to explain that extra source of heating that seemed to be missing.

Mr. Hepburn. Do we have any information at all as to the ratio of the matter in the dust to the matter in the gas?

Dr. Smith. Yes, there is a typical interstellar number. I don't remember what the exact ratio is off the top of my head — it's a paper by Bohlin in 1978 that gives that particular ratio, which is measured for a large number of nearby stars. But I'm not aware of it having been measured inside a small-scale structure. It might be different.

Professor Lynden-Bell. If you take a pulsar, and you ask how much flux of the galactic magnetic field comes into this pulsar and goes out again, you find that the pulsar will rotate columns which are only of the order of size of the Solar System. And these will be columns of quite high magnetic fields, which run through the interstellar gas. I don't know if this is at all relevant?

Dr. Smith. I did sit down once and try to work out the tidal enhancement behind a neutron star passing through the ISM. I didn't think about magnetic confinement though, which would push that up significantly. So I am not aware of anyone having actually done that calculation to see whether you could drive those sort of filamentary structures.

Professor Lynden-Bell. Well, you do drive structures of the order of the Solar System size.

Dr. Smith. Yes, but would they come up to two orders of magnitude denser than the surrounding material? That'd be interesting to find out!

Dr. G. Q. G. Stanley. When you looked at the ISM, using the proper motion of the star, have you also looked at it with another object, which is static so you can actually see how the column densities vary at the same time? So you can actually determine how the cloud is moving?

Dr. Smith. You can do this with multiple sources at once: it has been done for binary stars, where basically you watch both stars. You take the binary signal, and then you do a proper-motion survey on top of that. The problem is, if you are doing anything relatively nearby, it's going to have a significant proper motion. So you're never going to get a completely static source behind it. It's difficult to tell how you'd come up with something that you knew was static with respect to the cloud. You can obviously measure the velocity offsets along the line of sight, but you've got very little tangential information. I don't know how you'd do that.

Dr. Stanley. I was actually thinking of it as something like Donald's idea, but where you've got a quasar at a distance and you can determine the proper motion.

Dr. Smith. Yes, if you had something that was at cosmological distances, then you could assume that it was essentially static, and look for the cloud moving now. The interferometric observations in front of radio lobes do extend for baselines of 20 years, essentially. And there is no clear evidence for motion in those. Whether that's sensitive enough that you would actually measure it, I don't know. And we don't have stars in those directions to actually go and measure the physical properties, we'd just have H I observations.

Dr. Stanley. The reason I ask is because you say it's non-equilibrium. If you're seeing something like a turbulence going through, that may help you determine that.

Dr. Smith. Yes.

The Chairman. I think we will stop the discussion there. I would like to ask you to join me in thanking all of this afternoon's speakers [applause]. I think that for this audience I probably don't have to remind you about the drinks reception which is being held over in the RAS Library right now! But I would like to remind you of the next monthly meeting on Friday the 10th of May, following the AGM.

MAGNETISM ALONG SPIN

By D. Lynden-Bell
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There is some evidence that the magnetic moments of astronomical bodies are sensitive to the sign of their spin. The published intrinsic Faraday rotation for the centres of galaxies seen at inclination angles of less than 60° show that their central magnetic fields are preferentially oriented along $\boldsymbol{\Omega}$. There is also evidence from cosmic jets. Dynamo action is favoured in the giant planets, so it is probably a coincidence that all four of them too have $\mathbf{M.\Omega} > 0$.

Cosmic magnetic-field orientations

Dynamo theories of magnetic-field generation do not favour a correlation of magnetic-field sense with the sign of the spin of the object. Magnetic fields in galaxies are now quite well determined thanks to work by Beck[1] and collaborators[2–5]. Faraday rotations were mapped at low resolution in NGC 4298, 4303, 4321, and 4535 by Wezgowiec *et al.*[2]; at high resolution in NGC 4038, 4039, 4254, and 4736 by Chyzy[3] and Chyzy & Beck[4]; and in NGC 5194 by Fletcher *et al.*[5]. At the higher frequencies now commonly used, the Faraday rotation of the polarization vectors become small, so the lie of the magnetic-field lines can be seen by merely rotating the E-vector polarizations through a right angle. To get the sign of the fields or to use data from lower frequencies, the intrinsic Faraday rotation has been mapped over a number of galaxies. The Faraday rotation is positive if the electron-density-weighted field component along the line to the observer is positive. For fields in the direction of the spin as suggested, *e.g.*, by the Contopoulos–Kasanas battery[6,7], galaxies rotating anti-clockwise as seen on the sky would give positive intrinsic central Faraday-rotation measures (RM) while those seen in clockwise rotation should give negative central values. This test is easier for galaxies than for stars as we can see the sense of rotation directly from the trail of the spiral structure. Fig. 1 demonstrates that most of the galaxies for which data are available in the literature have their central magnetic-field components along their rotation axes. The small corrections for foreground Faraday rotation from Oppermann *et al.*[8] have been applied. At, or close to, the nucleus of NGC 6946 there is a point of strong negative RM (as predicted) but positive RM is so close we could not even determine the sign of the central RM. We plotted all others for which we could find data in Fig. 1. The probability of scoring eight out of nine correct due to chance alone is $9 \times 2^{-9} = 2\%$. Alternatively if we dismiss the three galaxies within two standard deviations of zero we get a probability of $2^{-6} = 1/64$. While this is small enough to be worth pointing out, it could be a chance event.

There is also evidence from cosmic jets which has been marshalled over several years by Gabuzda and collaborators[9–12]. Close to the jet source the Faraday rotation due to the toroidal field component agrees with the concept of a poloidal magnetic field along the rotation dragged around by the accretion disc at its base, but at larger distances the gradient of Faraday rotation across the jet reverses. This has been interpreted as the returning field dominating the more distant Faraday-rotation signal. It should be remarked that all force-free models of the returning field (such as mine[13]) wrongly predict that the toroidal component of the returning field has the same sense as the outgoing toroidal component since $2\pi B_\phi = \beta/R$ where R is the axial distance and β is constant along any force-free field line. Inclusion of inertia changes this; the expanding plasma of the jet slows its axial rotation as it expands sideways. Even the returning foot-points of the field-line are dragged around by the disc at a greater angular speed, so in more realistic jet models that include inertia the azimuthal components of the returning field have the opposite sense to those of the out-going field. This is what Gabuzda appeals to in her interpretation of the jet data.

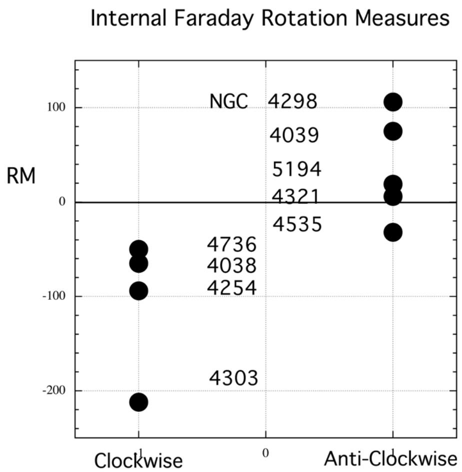

FIG. 1

Galaxy spin sense and central Faraday rotation. Errors are up to twice the symbol size. NGC 4038 has a point with opposite RM close-by.

We conclude that there is probably a preference for the central magnetic fields of galaxies to lie along their spins but a definitive test of this symmetry-breaking is still needed. The *Jansky VLA* in its compact or near-compact configurations has the sensitivity and the ability to map the Faraday rotation across the faces of nearby nearly-face-on galaxies. Since any symmetry-breaking, such as that suggested by the results cited above, implies a major departure from the dynamo theories currently favoured, there is a strong case for a programme to observe a significant number of nearly-face-on spirals to see whether the correlation found in Fig. 1 is merely a fluke of small-number statistics, or whether it represents a true symmetry breaking in Nature.

The possibility of symmetry breaking on a galactic scale suggests that we should survey the data for stars and the Solar System. The magnetic fields of the Earth and the Sun reverse their polarity; this indicates that they are continuously generated by dynamo action. Magnetic stripes have been found in the Martian rocks indicating that Mars too once had a field-reversing dynamo. Venus has no detectable internal field. Mercury's field like the Earth's present field has $\mathbf{M.\Omega} < 0$ where $\mathbf{M}$ is the magnetic dipole moment and $\mathbf{\Omega}$ the angular velocity of the planet's spin. Since dynamos cannot work in axial symmetry, a prediction of dynamo theory is that in general $\mathbf{M}$ should be inclined to $\mathbf{\Omega}$ and that $\mathbf{M.\Omega}$ is equally likely to have either sign. Extrapolating from the accepted dynamos of the terrestrial planets it has been assumed that the magnetic fields of the giant planets are also so generated, *via* pressurized metallic hydrogen for Jupiter and Saturn and perhaps *via* ice in Uranus and Neptune. Dynamo action is supported by the inclinations, i, and offsets of the dipoles of Jupiter, $i = 11°$, Uranus, $i = 59°$, and Neptune, $i = 47°$; however, Saturn's magnetic moment is aligned with its spin. We do not yet know whether the magnetic fields of the giant planets have reversed, but data from the rock-magnetism of satellites in their magnetospheres may eventually tell us. All four of these giant planets have $\mathbf{M.\Omega} > 0$. This is in the same sense as that found above in galaxies but it is probably a one-in-sixteen chance.

The work of Olin C. Wilson[14,15] on the cores of the calcium K lines and the extensive follow-up of Noyes *et al.*[16] and Noyes, Weiss & Vaughan[17] led to the detection of magnetic cycles in a number of the G, K, and M stars that are not too dissimilar from the 11-year solar cycle, thus dynamo action is the natural explanation for their magnetic fields. Babcock found magnetic fields in narrow-line A stars from the Zeeman effect. Many of these change but the oblique-rotator model accommodates nicely those with periodic changes and again the tilts of $\mathbf{M}$ *versus* $\mathbf{\Omega}$ can be taken as evidence for dynamo action. To see the sense of their rotation, stellar surfaces have to be resolved and few resolved stars have detectable magnetic fields. The dynamo prediction that on average $\mathbf{M.\Omega} = 0$ cannot yet be tested, though work by Auriere *et al.*[18] gives some hope that this might prove possible for resolved giant stars.

Gilman[19] discusses the behaviour of non-linear differential equations closely related to the dynamo process. He shows that non-reversing dynamos occur in some parameter régimes and that reversals are often sensitive to small changes in the parameters. Even if a highly conducting turbulent fluid is unstable to perturbations that amplify any small stray magnetic field, seed magnetic fields must be present to start that amplification. Biermann's[20] battery is widely quoted as a possible source of such a seed-field but Mestel[21] has outlined its limitations and it provides such a weak seed that it is often quoted *faute de mieux*. The Contopoulos–Kasanas battery can provide a bigger seed and non-reversing dynamos may maintain a memory of the sense of the seed field they amplified. The slow turnover times for fields of galactic scale limit the amplification available by dynamo action and make it less likely that the fields observed in galaxies and the intergalactic medium in clusters arise from the small Biermann seed fields. Its symmetry breaking was rejected by Krause & Beck[22] as the reason why four of their five galaxies had fields whose radial components pointed inwards to their centres.

I thank Ioannis Contopoulos for initiating my interest in whether this symmetry exists in Nature by hosting a small conference in Athens, and C. D. Mackay, R. Beck, N. O. Weiss, and C. A. Tout for their unstinting help.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 232: HR 3360, HR 4927, HR 6999, AND HR 8653

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The four bright stars have orbital periods of about 0·86, 5·5, 12·1, and 4·3 years. Their orbits have eccentricities of about 0·18, 0·17, 0·23, and 0·48. HR 6999, which has been known as a very close visual binary since 1900, is double-lined, with solar-type components that are very nearly equal; the other three stars are single-lined early-K giants.

Introduction

It may seem remarkable that in the 21st Century, after systematic radial-velocity observing programmes have been running for well over 100 years, there are plenty of stars, bright enough to feature in the *Bright Star Catalogue*, whose spectroscopic duplicity has not long been recognized and/or whose orbits remain to be determined. Well over 100 of the 9000 or so stars in the *Catalogue* have already featured in just this one series of papers, and four more are presented here. They have found their way onto the writer's observing programme in different ways. Velocity variations in HR 3360 and HR 4927 were first recognized in a survey programme by Duquennoy & Mayor[1] in 1999. HR 6999 has been known as a binary since Aitken's discovery[2] of it as a very close visual double in 1900, and as a short-period one since he[3] determined its orbit as early as 1912. HR 8653 is one of a small number of stars whose duplicity was discovered more than 40 years ago in a deliberate search for small-amplitude spectroscopic binaries among a set of about 50 F3–G1 main-sequence stars that were monitored with the Lick 120-inch reflector and coudé spectrograph over an interval of two years by Anderson & Kraft[4].

Those authors included three stars that were already noted in the *Bright Star Catalogue* as having variable velocities, and orbits have since been given for them by others. Two other stars were immediately found to be double-lined and were not followed; orbits have been given for both of them subsequently, one of them (HR 7955) by me[5]. Five more (HR 784, 2452, 7756, 8077, and 8653) were regarded as 'certain' new variables by Anderson & Kraft, on the basis that each exhibited a velocity dispersion greater than 0·5 km s^{-1} among several plates, and four others with $0{\cdot}5 > \sigma > 0{\cdot}4$ km s^{-1} were noted as 'possible' variables. Among those in the 'certain' category, HR 784 is too far south to be observed with the Cambridge telescope; orbits for HR 2452 and HR 8077 (4 Equ) have already featured[6,7] in the series of papers of which *this* is a member that includes HR 8653; the remaining one of the five, HR 7756, seems not to be so obviously binary, and some radial-velocity observations for it are given in the Appendix to this paper. The Appendix also presents measurements of HR 1687 and HR 7973, two of the four 'possible variables', which have also seemed rather constant in velocity. Famaey *et al.*[8] have equally failed to demonstrate variation in any of the 'possible variables' or in HR 7756.

HR 3360 (*HD 72184*)

HR 3360 is a sixth-magnitude star in an area of Lynx that to the naked eye is extraordinarily barren, about 10° north-following Castor and Pollux, two-thirds of the way to the star still known to some diehard visual-binary people as 10 UMa although when the constellation boundaries were objectively defined[9] it found itself well within the boundary of Lynx. It (HR 3360) featured in several of the early Mount Wilson papers giving 'spectroscopic parallaxes'. The earliest was in 1917, when Adams & Joy[10] attributed to it (under the alias Groombridge[11] 1450) an absolute magnitude of $+1^{m}{\cdot}3$ — very close to the (revised)[12] *Hipparcos* value of $+1^{m}{\cdot}57 \pm 0^{m}{\cdot}06$. They gave the 'estimated' spectral type as K2 and the 'measured' one as K1; the former was a classification made in what has since been regarded as the normal way, by visual comparison with spectra of standard stars, while the 'measured' type was determined by comparison, on tracings of the spectra, of the strengths of certain Balmer lines with specified nearby metallic ones. Adams *et al.*[13] (an enlarged consortium) gave revised information for HR 3360 in 1921, this time under the alias

Boss[14] 2268: they gave the 'estimated' and 'measured' types as K3 and K2, respectively, and the absolute magnitude as $+0^{m}\cdot 7$. Two years later, Adams & Joy, probably on the basis of the same plates as served for the luminosity estimates, published[15] the star's radial velocity as $+15\cdot 3 \pm 1\cdot 8$ ('probable error') km s^{-1} as a mean from six plates; the velocities were long afterwards published individually, with dates, by Abt[16] (the six values do not lead to quite the same mean and 'probable error' as the one given by Adams & Joy).

The reason for HR 3360 having an unusually rich literature (more than 100 references in *Simbad*) is that it was recognized more than 50 years ago by Miss Roman[17] as a 'high-velocity star' — she gave its space motion as 92 km s^{-1}. She also gave *UBV* photometry for it, which is in good agreement with subsequent measurements by Johnson *et al.*[18] and by Eggen[19] at $V = 5^{m}\cdot 90$, $(B-V) = 1^{m}\cdot 11$, $(U-B) = 1^{m}\cdot 17$. At the same time she gave its spectral type as K2 III. The star featured in the early Cambridge programmes of narrow-band spectrometry, which mostly[20–23] showed it to have spectral features of above-average strength notwithstanding its large space motion which is generally associated with low metal abundances. The first such programme, that of Griffin & Redman[24] on the λ4200-Å CN band, showed that feature to be distinctly stronger than in the average star of its type. Subsequent analyses, both from photometric criteria analogous to the Cambridge ones[25,26] and real spectroscopic analyses[27–29], confirmed the star to have above-solar abundances, and Spinrad & Taylor's term 'super-metal-rich'[30] was freely applied to it. Bond *et al.*[28], in a paper with that expression actually in the title, published spectral tracings in which the enhancements of certain lines in HR 3360 are obvious, and Faber *et al.*[27] noted the star as a "good SMR candidate". Much later, however, Luck & Challener[31] reckoned that it is not metal-rich at all, apart from a great over-abundance of yttrium which they found for many other stars on their programme but was apparent only when determined from Y II and not from Y I, decreasing the conviction that their results carry for the reader.

Keenan, Yorka & Wilson, agreeing with Griffin & Redman's finding[24] on the strength of CN in HR 3360, included that object in their paper on 'Recognition and classification of strong-CN giants'[32], in which they gave its type as K1·5 III CN 0·5; the star featured soon afterwards in Keenan's 'Perkins Catalogue of revised MK types for the cooler stars'[33], in which the classification is the same except that the luminosity class was slightly demoted to IIIb. The star also appears in Keenan's final catalogue[34], after he had had the benefit of parallaxes from *Hipparcos* as an objective guide to luminosities, just as K1·5 III–IIIb, with nothing noted about CN strength.

Eggen, who had a particular *penchant* for herding stars into dynamical groups, assigned HR 3360, in a paper[35] in the *Royal Observatory Bulletins*, to his 'η Cephei group'. He did that by requiring its distance to be such as (in conjunction with its proper motion and radial velocity) to fix its Galactic *V*-velocity (the component in the direction of Galactic rotation) at the value of 97 km s^{-1} that he adopted as that group's common characteristic. He repeated the assignment in papers[19,36] in the *PASP* (in the first of which he misprinted the reference to his own paper[35]). The necessary distance modulus was $5^{m}\cdot 50$, which is nearly 20 standard deviations adrift from the $4^{m}\cdot 37$ now known[12] from *Hipparcos*, so if any such group actually exists it is not likely to include HR 3360.

In 1999 the matter of HR 3360's radial velocity, which as far as observations were concerned had been in abeyance for three-quarters of a century, sprang to life again with the discovery by de Medeiros & Mayor[1], with the Haute-Provence (hereinafter OHP) *Coravel*, that the star is a spectroscopic binary.

TABLE I

Radial-velocity observations of HR 3360

Except as noted, the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	*(O – C)* km s^{-1}
1917 Feb. 8·31*	21267·31	+21·6	$\overline{100}$·767	+0·6
11·30*	270·30	22·4	·776	+1·3
1918 Mar. 29·21*	21681·21	11·4	$\overline{98}$·083	−4·0
Apr. 30·24*	713·24	12·8	·185	−1·0
1920 Jan. 12·40*	22335·40	9·2	$\overline{96}$·165	−4·8
Mar. 4·27*	387·27	17·5	·330	+3·0
1988 Mar. 7·93†	47227·93	14·6	$\overline{17}$·353	−0·1
1989 Apr. 21·97†	47637·97	19·7	$\overline{16}$·658	+0·1
2002 Dec. 9·18	52617·18	17·0	0·498	+0·1
2003 Jan. 5·16	52644·16	18·6	0·584	+0·2
Feb. 18·02	688·02	20·5	·723	−0·1
Mar. 17·83	715·83	21·3	·812	0·0
Apr. 15·91	744·91	21·0	·904	+0·2
May 13·89	772·89	18·4	·993	0·0
Oct. 12·23	924·23	16·3	1·475	−0·2
Nov. 28·19	971·19	19·1	·624	+0·1
Dec. 27·22	53000·22	20·4	·716	−0·1
31·26‡	004·26	21·6	·729	+0·9
2004 Feb. 9·06	53044·06	21·2	1·856	−0·1
Mar. 16·97	080·97	19·3	·973	+0·3
29·97	093·97	17·4	2·015	−0·2
Apr. 25·92	120·92	15·1	·100	+0·1
May 3·87	128·87	14·7	·126	+0·2
10·89	135·89	13·9	·148	−0·2
21·87	146·87	13·5	·183	−0·3
Sept. 4·19	252·19	17·3	·518	0·0
Oct. 26·24	304·24	20·0	·684	0·0
2005 Jan. 9·09	53379·09	20·5	2·922	+0·1
14·16	384·16	19·9	·938	−0·2
16·35‡	386·35	19·5	·945	−0·4
26·09	396·09	18·8	·976	−0·2
Mar. 11·25‡	440·25	14·3	3·116	−0·3
Nov. 30·16	704·16	19·7	·956	+0·1
2006 Feb. 14·29‡	53780·29	14·9	4·198	+1·1
16·94	782·94	13·8	·206	+0·1
Mar. 1·95	795·95	13·8	·248	0·0
19·14‡	813·14	14·0	·302	−0·2
22·89	816·89	14·5	·314	+0·2
Apr. 3·93	828·93	14·6	·353	−0·1
May 9·92	864·92	16·3	·467	−0·1
13·13‡	868·13	16·5	·477	−0·1
31·89	886·89	17·3	·537	−0·3
Oct. 27·21	54035·21	17·8	5·009	0·0
Nov. 17·21	056·21	+15·7	·076	+0·1

TABLE I (*concluded*)

Date (UT)		MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
2007 Jan.	23·06	54123·06	+14·2	5·288	+0·1
	29·31‡	129·31	13·7	·308	−0·5
Feb.	26·90	157·90	15·5	·399	+0·1
Mar.	4·25‡	163·25	15·7	·416	+0·1
	8·21‡	167·21	15·4	·429	−0·4
Apr.	4·21‡	194·21	16·9	·515	−0·3
	18·86	208·86	18·1	·561	+0·1
Nov.	3·25	407·25	13·9	6·192	+0·1
2008 Feb.	2·07	54498·07	16·8	6·481	+0·2
Apr.	23·89	579·89	20·9	·742	+0·1
May	2·87	588·87	20·8	·770	−0·3
2009 Dec.	1·17	55166·17	18·7	8·607	−0·1
2010 Apr.	12·91	55298·91	17·2	9·029	+0·1
	16·96	302·96	16·7	·042	0·0
Dec.	9·21	539·21	21·3	·794	+0·1
2012 Jan.	13·13	55939·13	15·9	11·066	0·0
Apr.	17·89	56034·89	15·0	·370	0·0
May	11·89	058·89	16·0	·447	−0·1
Nov.	18·22	249·22	+16·3	12·052	−0·1

*Mount Wilson photographic velocity[15]; wt. 0.
†Published OHP *Coravel* velocity[1]; weight 1.
‡Published by Massarotti *et al.*[38]; weight 0·07.

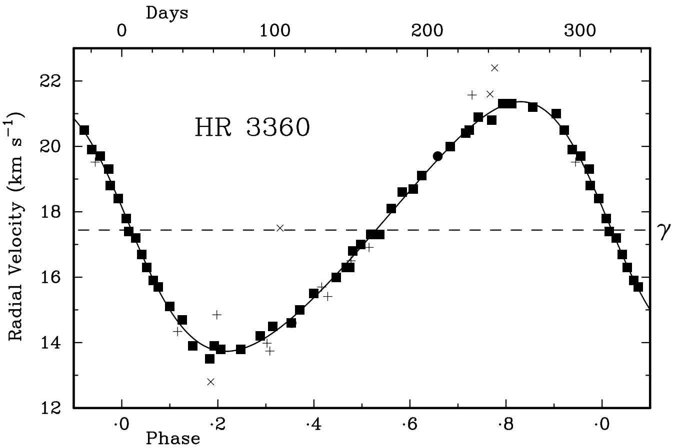

FIG. 1

The observed radial velocities of HR 3360 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Cambridge observations are plotted as filled squares. Those given by Massarotti *et al.*[38] (weighted 0·07 in the solution of the orbit) are shown as plusses. Four of the six early Mount Wilson observations[15], zero-weighted in the orbit, appear as crosses; the other two are outside the boundaries of the figure.

Those authors had just two measurements, which gave a mean of +15·88 ± 2·56 km s^{-1}, so the two velocities themselves could be expected to be given by the selfsame expression. When they were later made available individually, however, through the Centre de Données Stellaires (CDS), they were both about 0·5 km s^{-1} below the values implied by the original publication[1]. It is the revised values that have been included in Table I here, along with the six old Mount Wilson photographic velocities, after the usual adjustment of +0·8 km s^{-1} to try to bring them into line with the zero-point commonly adopted in this series of papers. De Medeiros & Mayor also listed a rotational velocity, of < 1·0 km s^{-1}. The same data were repeated by de Medeiros, da Silva & Maia[37], except that they omitted to give any mean velocity and they had modified the rotational-velocity number by the omission of the < symbol.

The accessibility of the individual OHP observations galvanized the present writer into adding HR 3360 (and many other stars, including HR 4927 which is treated below) to the Cambridge spectroscopic-binary observing programme, The object was systematically observed in 2002–12, and 45 radial-velocity measurements, listed in Table I, were accumulated. The period proved to be 314 days, about 6/7 of a year, so there are seven revolutions in six years and the phases migrate round the calendar on the six-year time-scale, facilitating uniform observational coverage. While the star was under investigation at Cambridge, it was also observed by the Harvard group of Massarotti *et al.*[38] with the 60-inch *Wyeth* and *Tillinghast* reflectors, ten measurements in all. On that basis they offered a somewhat preliminary orbit, all but two of the observations being bunched within a range of 30% of the orbital period.

The orbit has been derived from the data in Table I by giving unit weight to the Cambridge and OHP data and weighting the Harvard velocities 0·07 to put their weighted variance into approximate equality with that given by the *Coravel*s. An empirical adjustment of +1·3 km s^{-1} has been added to the Harvard velocities to bring their mean residual to zero. The Mount Wilson velocities, which have a variance about 400 times greater than that of the *Coravel*s, have not been included in the solution. The orbit is illustrated in Fig. 1. The corresponding orbital elements are given in Table II, with those of the Harvard orbit for comparison. The family resemblance is apparent, but the standard errors are seen to be improved by a factor of order ten or more in the new solution.

TABLE II

Orbital elements for HR 3360

Element	*Massarotti et al.*[38]	*This paper*
P (days)	324 ± 10	314·34 ± 0·08
T (MJD)	54051 ± 59	53718·1 ± 2·2
γ (km s^{-1})	+16·53 ± 0·41	+17·44 ± 0.03
K (km s^{-1})	3·83 ± 0·34	3·82 ± 0·04
e	0·149 ± 0·096	0·183 ± 0·009
ω (degrees)	90 ± 54	81·0 ± 2·8
$a_1 \sin i$ (Gm)	16·9 ± 1·4	16·22 ± 0·15
$f(m)$ ($M_\odot$)	0·00183 ± 0·00051	0·00172 ± 0·00005
R.m.s. residual (km s^{-1})	0·54	0·14

The very small mass function shows that, for a reasonable value of say 2 $M_\odot$ for the primary mass, the companion need not have a mass of more than 0·5 $M_\odot$, corresponding approximately to a type of M0 V. It would be possible for a main-sequence secondary to be considerably brighter than that without being apparent in the radial-velocity traces: the small amplitude means that the secondary dip would always be blended with, and thereby easily masked by, the principal one. The star has no measurable rotational velocity: all but one of the Cambridge traces yield rotation values of either zero or one-half km s^{-1} (the values are quantized in half-km s^{-1} steps). Of course negative values are not permitted, but the concentration of the observed rotations towards zero suggests that there would be a significant spread of the individual values into negative numbers if they were allowed; the impression is given that the spectral lines are if anything *narrower* than the standard zero-rotation profile which serves as the basis for determinations of rotational velocities with the *Coravel*.

HR 4927 (HD 113049)

Among the constellation figures, the tail of Draco occupies a jagged area, the declination of whose northern boundary increases towards earlier right ascensions in four steps, from about 65° near 15^h to above 81° near 10^h; in the second of the four north-eastern corners lies HR 4927, a 6^m K0 III star that (like HR 3360) was discovered to be a spectroscopic binary by de Medeiros & Mayor[1]. The position of HR 4927 at $\alpha \sim 13^h$, $\delta \sim 75°$, happens to place it in Selected Area[39] no. 5. Photometry of the star has seemed to have been less than straightforward; indeed the photovisual and photographic magnitudes of $6^{m}\cdot08$ and $7^{m}\cdot05$ given by Parkhurst[40] in 1912 seem almost as satisfactory as the *UBV* results that were obtained originally by Rybka[41] and Nekrasova & Nikonov[42] in 1957 and 1965, respectively, 'corrected' by reference to standards from the Arizona LPL[18] by Rybka[43], and finally incorporated into the latter's catalogue[44] of 'standards' in the Selected Areas, with $V = 5^{m}\cdot95$, $(B-V) = 1^{m}\cdot01$, $(U-B) = 0^{m}\cdot75$. Häggkvist & Oja[45], more directly, presented their own measurements made at Kvistaberg and then averaged them in some way with those that they found in the literature (presumably Rybka's), obtaining $V = 5^{m}\cdot98$, $(B-V) = 1^{m}\cdot042$, $(U-B) = 0^{m}\cdot82$, which may be the best values to adopt for the present.

On the basis of photometry in the Vilnius system[46], Bartkevičius & Lazauskaitė[47] considered HR 4927 to be a 'Population II' star; they assigned it a spectral type of 'MD-K0 III' (where the MD stands for Metal-Deficient), though the [Fe/H] was only –0·23, and derived an absolute magnitude of +0·92. The revised *Hipparcos* parallax yields a value slightly brighter than zero, $-0^{m}\cdot12 \pm 0^{m}\cdot14$. The star has been identified[48] with a *ROSAT* X-ray source. Its angular diameter has been directly measured[49] interferometrically at just under one millisecond of arc, and its linear radius thereby determined as $17\cdot35 \pm 1\cdot07\ R_\odot$.

Until comparatively recently, the only radial-velocity measurements obtained for HR 4927 were those made at the David Dunlap Observatory (DDO) in a project involving the stars brighter than $7^{m}\cdot6$ in the Selected Areas at declinations of +15° and above. They were made with a one-prism spectrograph on the 74-inch reflector, mostly with a dispersion of 66 Å mm^{-1} at Hγ. A mean from five plates was given[50] as $-14\cdot0 \pm 0\cdot9$ ('probable error') km s^{-1}. Even amongst the *late*-type stars on that programme, that was quite a small value for the uncertainty and did not by any means give grounds for thinking that HR 4927 was a spectroscopic binary. In fact the author of the paper (Young, the Director at the time of the DDO) particularly states in his introductory text, "The detection of the binary character of those stars with small range, less than 20 km., is uncertain and doubtless some of these have been included as of constant velocity.". Evidently the perception of what is a 'small range' has altered since the introduction[51] of velocity measurement by cross-correlation of spectra!

TABLE III

Radial-velocity observations of HR 4927

Except as noted, the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s^{-1}*	*Phase*	*(O – C)* *km s^{-1}*
1988 Mar. 28·06*	47248·06	–17·1	$\bar{3}$·624	–0·2
1989 Apr. 22·10*	47638·10	–22·0	$\bar{3}$·819	+0·1
1992 Jan. 31·12*	48652·12	–16·1	$\bar{2}$·324	–0·1
1993 Apr. 9·00*	49086·00	–15·9	$\bar{2}$·540	–0·1
2002 May 30·90	52424·90	–18·6	0·204	0·0
July 14·92	469·92	–17·9	·227	+0·1
2003 Feb. 15·20	52685·20	–16·4	0·334	–0·5
Apr. 16·05	745·05	–15·7	·364	–0·1
June 14·99	804·99	–15·4	·394	0·0
2004 Mar. 1·25	53065·25	–15·2	0·524	+0·4
May 6·96	131·96	–15·8	·557	+0·1
July 30·87	216·87	–16·4	·599	+0·1
2005 Jan. 13·18	53383·18	–18·5	0·682	–0·3
Mar. 23·13	452·13	–19·1	·716	–0·1
Apr. 22·08	482·08	–19·6	·731	–0·2
May 21·93	511·93	–19·8	·746	0·0
June 26·95	547·95	–20·2	·764	+0·2
July 20·88	571·88	–20·7	·776	0·0
Nov. 5·25	679·25	–22·6	·830	–0·2
Dec. 18·28	722·28	–23·3	·851	–0·3
2006 Jan. 27·27	53762·27	–23·6	0·871	0·0
Mar. 1·20	795·20	–23·9	·887	+0·2
Apr. 4·02	829·02	–24·4	·904	0·0
May 9·94	864·94	–24·6	·922	+0·2
June 24·92	910·92	–25·2	·945	–0·1
2007 Feb. 3·27	54134·27	–24·3	1·056	–0·5
Mar. 22·13	181·13	–23·3	·080	–0·3
Apr. 30·04	220·04	–22·2	·099	+0·1
May 29·99	249·99	–21·5	·114	+0·3
June 26·96	277·96	–21·0	·128	+0·3
July 19·89	300·89	–20·8	·139	+0·1
Aug. 10·86	322·86	–20·6	·150	–0·1
Nov. 9·24	413·24	–19·0	·195	–0·1
Dec. 8·17	442·17	–18·5	·210	0·0
2008 Feb. 25·18	54521·18	–17·4	1·249	0·0
Apr. 8·05	564·05	–16·8	·271	+0·1
2009 Jan. 6·31	54837·31	–15·9	1·407	–0·6
Feb. 12·09	874·09	–15·2	·425	+0·1
Mar. 30·13	920·13	–14·9	·448	+0·4
Apr. 29·03	950·03	–15·1	·463	+0·2
May 24·01	975·01	–15·3	·475	0·0
June 30·92	55012·92	–15·1	·494	+0·3
Aug. 17·84	060·84	–15·5	·518	+0·1

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2010 Apr. 8·08	55294·08	−17·3	1·634	−0·2
May 9·94	325·94	−17·4	·650	+0·1
June 3·95	350·95	−17·7	·663	0·0
July 5·95	382·95	−18·2	·679	−0·1
Aug. 15·85	423·85	−18·7	·699	−0·1
Dec. 12·28	542·28	−19·9	·758	+0·3
2011 Apr. 7·00	55658·00	−21·8	1·816	+0·2
2012 Feb. 25·11	55982·11	−25·2	1·977	0·0
Apr. 6·05	56023·05	−24·9	·998	+0·1
May 11·98	058·98	−24·6	2·016	+0·2
July 22·91	130·91	−24·0	·051	−0·1
2013 Mar. 27·13	56378·13	−19·4	2·175	+0·2

*Published OHP *Coravel* velocity[1]; weight 1.

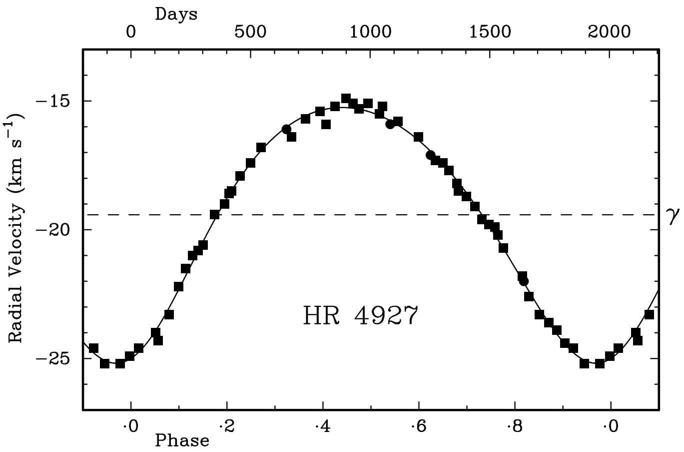

FIG. 2

Orbit of HR 4927, analogous to Fig. 1, but here the only non-Cambridge observations are four from OHP[1], plotted as filled circles and included with full weight in the solution of the orbit.

De Medeiros & Mayor[1] made four observations of HR 4927 at OHP and showed it to be a spectroscopic binary — the r.m.s. spread of the four measurements implied a standard error of 2·88 km s^{-1} per measurement, no doubt about ten times the real uncertainty. When those authors deposited their individual measurements with the CDS in 2002, HR 4927 was one of the stars selected by the present writer for observation from Cambridge. It is noted that the mean of the four OHP observations as listed at the CDS is −18·56 km s^{-1} whereas the mean given in the original paper is −19·09, although the spread of the four velocities tallies with the number given in the paper. There are 51 radial-velocity observations that have been made with the Cambridge *Coravel*; they are listed in Table III after those of de Medeiros & Mayor, which have as usual been increased by 0·8 km s^{-1} (the DDO ones cannot be included because they were presented only as a mean). All the tabulated measurements have been given the same weight in the calculation of the orbit, whose elements are presented below; the orbit is illustrated in Fig. 2.

P = 2006·4 ± 3·1 days	$(T)_1$ = MJD 54021 ± 16
γ = −19·42 ± 0·03 km s^{-1}	$a_1 \sin i$ = 135·0 ± 1·3 Gm
K = 4·96 ± 0·05 km s^{-1}	$f(m)$ = 0·0244 ± 0·0007 $M_\odot$
e = 0·167 ± 0·008	
ω = 195·8 ± 3·1 degrees	R.m.s. residual (wt. 1) = 0·21 km s^{-1}

The mass function sets a lower limit of 0·54 $M_\odot$ to the mass of the secondary star (corresponding to that of a very-late-K dwarf) if the primary is supposed to have a mass of 2 $M_\odot$. The rotational velocity of HR 4927, like that of HR 3360, is very small, but the individual values are not so firmly wedged up against the lower limit of zero rotation: only 13 of the 51 values are exactly zero, but the overall mean still is less than 1 km s^{-1}. The bias caused by negative rotational velocities being forbidden is probably small in this case, and the conclusion of < 1 km s^{-1} can be accepted as a fair summary of the observational results.

HR 6999 (*HD 172088*)

HR 6999 appears as 6½^m star 3° south of the celestial equator, in the middle one of the three south-eastern corners of the *Cauda* moiety of the constellation Serpens. (The stars treated in this paper were not *deliberately* selected as being in the tail ends of constellations!) Its photometry has been given by Cousins[52] and by Eggen[53,54] (it is not clear whether his two listings are independent); the results are in good accord at $V = 6^m{\cdot}49$, $(B-V) = 0^m{\cdot}55$, $(U-B) = 0^m{\cdot}04$. Interest in the star was aroused when Aitken discovered it to be a very close visual binary, with almost equal components separated by only 0″·13. He made that extraordinary discovery, at a separation right at the received 'Dawes limit'[55] for the resolving power of the available aperture, with the Lick 36-inch refractor, in 1900; the system, identified by its entry in the Lalande catalogue[56] as LAL 34524*, was listed by Aitken as A 88 in the second[2] of his papers presenting lists of visual-binary discoveries. He commented, "Found with the 36-inch on June 2, 1900. It is just separated with the highest powers."

Aitken had a meteoric rise to become one of the greatest-ever discoverers and measurers of visual double stars. He went to the Lick Observatory as a summer assistant in 1895, and was directed towards double-star work — with the wonderful 12-inch Clark refractor, on a programme provided to him by Barnard[57] — because such work had lagged there since the departure of Burnham for Yerkes in 1892. But it must have been the job he was born to do, and he was so proficient at it that just weeks after his arrival he was offered, and accepted, a post as Assistant Astronomer, and remained at Lick for the rest of his career, finishing as Director. It was not long after his arrival that he was allowed to use the 36-inch refractor, with which (like Burnham) he managed not only to measure, but actually to discover, binary stars right down to a

*The star is noted in Lalande as being in Aquila, which was no doubt true at the time that his catalogue[56] was published. It is only 1° from the point now defined as the confluence of Aquila, Scutum, and Serpens Cauda.

separation of a tenth of a second of arc, considerably *below* the Dawes 'limit'*. Somehow the master observers seem not to have been stymied as the rest of us are by the difficulties presented by *seeing*, which surely is never as good as *that*, even at Lick! The quotation at the end of the last paragraph seems not to carry any suggestion of particular difficulty and indeed can be read as indicating that the system was not at the extreme limit of resolution. A humble spectroscopist can only marvel at the brilliance both of Aitken as an observer and of the Clarks as makers of the object-glass of the 36-inch!†

The new double star proved to exhibit motion that, for a visual system, was unusually rapid, and by 1912 Aitken had seen it round a complete cycle and derived an orbit that is very similar to the currently determined one; it had a period of 12·12 years, and now it is found to be 12·13. Although he determined the orbit in 1912 (it did not get published[3] until 1914), he saw no need to revise it when he used it as a 'worked example' in his 1918 book[61], or indeed even in the second edition[62] that was published in 1935. That book fulfilled so well the needs of students of double stars that after almost another 30 years it was reprinted in an inexpensive edition by Dover[63], whose editor selected Aitken's diagram of the A 88 orbit to grace the front cover of the reprint. Long after Aitken, van den Bos[64], and then Hartkopf *et al.*[65], published updated solutions of the orbit. Other sets of elements (only, without data or discussion) have been published by Starikova[66], and (post-*Hipparcos*) by Söderhjelm[67]. Of course the accumulation of observations and the passage of time offer opportunities to improve the solution of any binary; in the case of this very close system, the virtual supplanting of visual observations by interferometric ones in recent years has led to the former having to be seen as systematically over-estimated and accordingly to a reduction in the angular scale of the orbit. Dr. Hartkopf has now very kindly furnished a new set of elements that utilize all data published up to date. They are presented here in Table IV beside the elements found by Aitken 101 years ago when only 12 years had elapsed since he discovered the object.

The star of interest here was (like HR 3360, above) on some of the early Mount Wilson observing programmes. In the earliest one[13] in which it features, in 1921, it was identified as 'Bu 8679' (from its number in Burnham's *General Catalogue of Double Stars*[68]); its magnitude, corrected to that of the brighter component alone, was given as $7^{\mathrm{m}}{\cdot}1$, the spectral type was 'estimated' as G0 and 'measured' as F8 (those terms are explained above, for HR 3360). Probably by means of a calibration of absolute magnitude as a function of spectral type, the absolute magnitude (presumably 'per star') was given as $4^{\mathrm{m}}{\cdot}6$ and the corresponding 'spectroscopic parallax' was deduced to be $0''{\cdot}032$. (The *Hipparcos* value[12] is $0''{\cdot}021$.) In 1935, the system (then identified as ADS[69] 11520 AB) was classified as F8, 'estimation' having been adopted then as the only method; the absolute magnitude per component was revised to $3^{\mathrm{m}}{\cdot}9$ and

*There is of course no hard-and-fast limit fixed by physical law; Dawes simply codified the empirical evidence of what a skilled observer could normally hope to achieve with a given aperture in favourable cases (equal pairs of about the sixth magnitude). His 'limit' corresponds to about 0·84 times the separation at which the central disc of the diffraction image of each component of a double star falls on the first dark ring of that of the other component. At smaller separations duplicity may still in principle be recognized by asymmetry in the mutually blended central discs of the two images.

†And that of the 12-inch! I had occasion to do that before[58], more than thirty years ago, in connection with Barnard's observations of Jupiter's satellite Io, which subtends an angular diameter only slightly more than 1″ at opposition. The 12-inch revealed detail on it only imperfectly[59], but later, in the 36-inch, "the satellite presented a beautiful appearance. It stood out in bold relief like a little globe."[60]

TABLE IV

Astrometric orbital elements for HR 6999 (A 88)

Element		*Aitken**	*Hartkopf*†
P	(years)	12·12	12·135 ± 0·006
	(days)	4427	4432·2 ± 2·0
T	(year)	1910·10	2007·18 ± 0·04
	(MJD)	18709	54166 ± 14
e		0·276	0·2567 ± 0·0029
ω	(degrees)	269·9	79·7 ± 1·5
i	(degrees)	±62·4	123·9 ± 5·0
a	(arcsec)	0·176	0·1461 ± 0·0008
Ω	(degrees)	2·4	173·4 ± 0·9

*Computed 1912, published 1914[3]
†Up-to-date solution, private communication 2013

the parallax to 0″·022, almost exactly the same as the currently adopted value that is noted just above. Much later (1969), Christy & Walker[70] gave the MK classification of HR 6999 (as ADS 11520) as F9V. They were trying to assign types to both of the components of visual binaries on the basis of classification spectra taken of the systems unresolved. That was easy to do in the relevant case: the components of HR 6999 are so similar in magnitude (the difference has generally been put between $0^{m}\cdot 0$ and $0^{m}\cdot 2$) that they can be expected to be very like one another in all respects, and so to have similar spectra. Thus, on the basis that the Δm of ADS 11520 was only $0^{m}\cdot 1$, Christy & Walker simply found the individual types both to be F9V! Later, (Anne) Cowley[71] gave the combined type as G0V; Abt[72] gave it as F9V. Beavers & Cook[73] invoked a procedure for disentangling the spectral types of unresolved visual binaries from spectrophotometry; they found the primary to be of type F7–F9 and the secondary F9–F7 — evidently implying once more that the components were actually indistinguishable. Two comparatively recent papers from largely overlapping syndicates[74,75], devoted to lithium and stellar rotation in sub-giants, appear to regard HR 6999 as a member of that category, presumably mistaking its position three-quarters of a magnitude above the main sequence in the H–R diagram as arising from evolution rather than duplicity, although in the earlier paper[74] it actually appears in a table listing binary systems.

The radial velocity of HR 6999 was first measured at Mount Wilson by Adams *et al.*[15], who gave a mean from eight plates as $-21\cdot 5 \pm 1\cdot 5$ km s^{-1}. The individual dates and velocities were published 50 years later, in 1973, by Abt[76]. They were all obtained within an interval of about one-fifth of an orbital period and have a total spread of about 18 km s^{-1}. They are of course what we would call single-lined, and the near-equality of the components of HR 6999 implies that single-lined velocities could be expected to show almost zero amplitude of variation. Abt[77] later gave two measurements of his own, noting that "The present velocities do not agree well with the previous ones (Abt 1973), but a correlation with phase in the visual orbit is neither expected nor observed.". Nordström *et al.*[78] gave a mean ($-17\cdot 5 \pm 0\cdot 5$ km s^{-1}) from no fewer than 20 OHP/ESO *Coravel* velocities; it is a bit surprising that the number was not larger, because even if nobody else observed that system with those instruments the present writer did so on 29 occasions, so the data base that was tapped by those authors should have contained at least 29 entries for the object. Evidently the observations were treated as single-lined; the broadening occasioned by the incipient splitting of the 'dip' signature at certain orbital phases by the radial-velocity difference between the components was ascribed simply to rotation, for which a mean value of 6 km s^{-1} was actually tabulated[78].

The writer first observed the radial velocity of HR 6999 with the ESO *Coravel* in 1987, and thereafter kept a careful watch on it with the OHP instrument[79]. To begin with the radial-velocity traces appeared single-lined, but in 1992 it became apparent that there were two 'dips' which were, however, badly blended together — and in 1994 they closed right up and appeared single-lined again. In the early days of the observations, a few were made with the original Cambridge instrument, as well as two at ESO and a few with the spectrometer at the Dominion Astrophysical Observatory's 48-inch reflector; all those observations were, however, effectively single-lined and so cannot contribute to the solution of the orbit. For a short time in 1997, and then more or less continuously (when the star was accessible to observation) in 2000–2013, HR 6999 has been followed with the Cambridge *Coravel*; altogether, 91 observations have been made with that instrument. They are set out in Table V. The low declination (the object culminates at a zenith distance of 56° and cannot be reached by the Cambridge telescope much more than two hours of hour angle from the meridian) means that the observing season is restricted; on the other hand the fact that the star is nevertheless well to the north of the ecliptic is a palliative. Observations have been possible from Cambridge from March till mid-November; at the other observatories mentioned they could be made in every calendar month.

Fig. 3 shows a Cambridge radial-velocity trace of HR 6999 taken near a nodal passage and showing the two dips as well separated as they can ever be. The degree of separation in the illustration is just about enough to leave one wing of each dip largely un-blended and so enables the profiles of the individual dips to be tolerably well determined. Once the dip profiles are known, other traces, even when they appear absolutely single-lined (as most of those of HR 6999 do), can be reduced to give twin velocities. It is only when the velocities

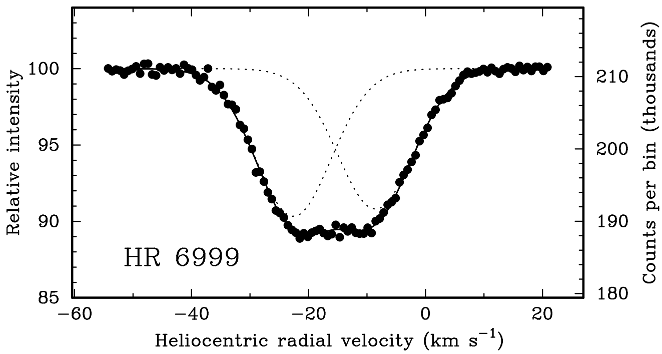

FIG. 3

Radial-velocity trace of HR 6999, obtained with the Cambridge *Coravel* on 2004 September 5 close to a nodal passage and showing the two dips almost as well resolved as they can ever be. The majority of the traces appear to the eye to be just single dips.

Table V

Radial-velocity observations of HR 6999

All the observations were made with the Cambridge Coravel
The early observations, noted with colons, were half-weighted in the orbit

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
1997	Mar.	31·19	50538·19	−9·6:	−23·5:	0·181	−0·4	−0·5
	Apr.	16·16	554·16	−8·9:	−23·5:	·184	+0·3	−0·5
	May	11·11	579·11	−9·4:	−23·1:	·190	−0·3	0·0
	June	20·09	619·09	−9·6:	−23·5:	·199	−0·5	−0·4
2000	Apr.	7·16	51641·16	−13·0:	−19·4:	0·430	−0·1	−0·3
	May	31·03	695·03	−13·7:	−19·3:	·442	−0·4	−0·6
	Aug.	14·88	770·88	−13·7:	−18·1:	·459	0·0	+0·2
	Sept.	20·83	807·83	−13·0:	−18·6:	·467	+1·0	−0·6
	Oct.	13·78	830·78	−13·3:	−18·7:	·473	+0·8	−0·8
	Nov.	3·75	851·75	−13·5:	−18·7:	·477	+0·7	−1·0
2001	Mar.	3·23	51971·23	−16·6		0·504	—	—
	May	12·12	52041·12	−15·8		·520	—	—
	June	18·10	078·10	−15·7		·528	—	—
	July	11·03	101·03	−16·3		·533	—	—
	Aug.	14·95	135·95	−16·0		·541	—	—
	Sept.	22·83	174·83	−16·0		·550	—	—
	Oct.	18·77	200·77	−16·0		·556	—	—
	Nov.	1·74	214·74	−15·8		·559	—	—
2002	Mar.	27·19	52360·19	−15·6		0·592	—	—
	Apr.	18·14	382·14	−18·7:	−13·1:	·597	−1·0	+1·0
	May	16·08	410·08	−18·2:	−13·8:	·603	−0·3	+0·1
	June	1·06	426·06	−18·6:	−13·3:	·607	−0·6	+0·5
	July	4·00	459·00	−18·2:	−12·8:	·614	0·0	+0·8
	Aug.	6·95	492·95	−18·2	−13·5	·622	+0·3	−0·2
	Sept.	1·87	518·87	−18·7	−12·3	·628	−0·1	+0·8
	Oct.	3·80	550·80	−19·1	−12·9	·635	−0·2	0·0
	Nov.	4·73	582·73	−19·3	−12·7	·642	−0·2	0·0
2003	Mar.	1·24	52699·24	−20·2	−11·7	0·668	−0·4	+0·2
	Apr.	7·17	736·17	−20·1	−11·4	·677	0·0	+0·2
	May	1·13	760·13	−20·5	−11·7	·682	−0·3	−0·2
	June	11·06	801·06	−20·7	−11·1	·691	−0·2	+0·1
	July	13·01	833·01	−20·8	−10·7	·699	−0·1	+0·3
	Aug.	2·94	853·94	−20·4	−10·2	·703	+0·4	+0·6
	Sept.	13·86	895·86	−21·4	−10·8	·713	−0·3	−0·2
	Oct.	14·81	926·81	−21·9	−10·2	·720	−0·6	+0·2
2004	Apr.	20·15	53115·15	−23·0	−9·9	0·762	−0·7	−0·6
	May	19·13	144·13	−23·0	−9·7	·769	−0·5	−0·6
	June	17·06	173·06	−22·3	−9·0	·775	+0·3	0·0
	Aug.	7·93	224·93	−22·2	−8·5	·787	+0·6	+0·3
	Sept.	5·84	253·84	−22·4	−8·5	·794	+0·5	+0·2
	Oct.	5·81	283·81	−22·8	−8·6	·800	+0·2	−0·1
	Nov.	4·72	313·72	−22·8	−8·2	·807	+0·3	+0·2
2005	Mar.	12·23	53441·23	−23·0	−8·4	0·836	+0·4	−0·3
	May	9·13	499·13	−23·3	−8·5	·849	+0·2	−0·4
	June	14·04	535·04	−23·2	−8·3	·857	+0·2	−0·2
		28·03	549·03	−23·6	−8·4	·860	−0·2	−0·3
	July	18·02	569·02	−23·1	−7·8	·865	+0·3	+0·4
		28·99	579·99	−23·1	−7·6	·867	+0·3	+0·6

TABLE V (*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*
2005	Aug.	8·96	53590·96	−23·6	−8·3	0·870	−0·2	−0·1
		16·92	598·92	−23·6	−8·5	·871	−0·2	−0·3
	Sept.	7·86	620·86	−23·7	−8·8	·876	−0·4	−0·5
		14·81	627·81	−23·5	−8·3	·878	−0·2	0·0
	Oct.	29·75	672·75	−23·5	−7·9	·888	−0·4	+0·6
2006	Apr.	9·17	53834·17	−22·9	−9·3	0·925	−0·8	+0·2
	May	30·06	885·06	−21·4	−10·3	·936	+0·2	−0·3
	June	23·07	909·07	−21·2	−10·4	·941	+0·1	−0·1
	July	17·99	933·99	−20·7	−10·4	·947	+0·4	+0·2
	Aug.	28·89	975·89	−20·3	−11·0	·957	+0·2	+0·2
	Sept.	22·82	54000·82	−20·2	−12·2	·962	0·0	−0·7
	Nov.	1·75	040·75	−19·6	−12·2	·971	0·0	−0·1
2007	Apr.	5·18	54195·18	−18·1	−13·4	1·006	−0·9	+1·2
2008	July	1·08	54648·08	−10·7	−21·7	1·108	+0·2	−0·4
		28·95	675·95	−10·3	−21·5	·114	+0·3	0·0
	Aug.	30·86	708·86	−10·0	−21·9	·122	+0·4	−0·1
	Sept.	26·81	735·81	−9·8	−22·1	·128	+0·4	−0·1
	Oct.	22·77	761·77	−9·9	−22·3	·134	+0·1	−0·1
	Nov.	18·70	788·70	−9·8	−22·1	·140	0·0	+0·2
2009	Apr.	29·15	54950·15	−9·6	−23·5	1·176	−0·4	−0·5
	May	29·11	980·11	−9·5	−23·0	·183	−0·3	0·0
	July	3·01	55015·01	−9·6	−23·2	·191	−0·5	−0·1
	Aug.	7·97	050·97	−9·0	−22·9	·199	+0·1	+0·2
	Sept.	25·82	099·82	−9·0	−22·8	·210	+0·1	+0·3
	Oct.	22·75	126·75	−9·3	−23·0	·216	−0·2	+0·1
2010	May	12·14	55328·14	−9·3	−22·3	1·262	+0·1	+0·5
	June	3·11	350·11	−9·8	−22·4	·267	−0·3	+0·3
	July	10·03	387·03	−10·0	−22·5	·275	−0·4	+0·1
	Aug.	12·93	420·93	−9·5	−22·1	·283	+0·2	+0·4
	Sept.	20·81	459·81	−9·7	−22·1	·291	+0·2	+0·2
	Oct.	16·78	485·78	−9·9	−21·8	·297	+0·1	+0·4
	Nov.	15·71	515·71	−10·2	−21·9	·304	−0·1	+0·2
2011	May	10·14	55691·14	−11·6	−21·5	1·344	−0·7	−0·2
	June	10·10	722·10	−11·5	−21·0	·351	−0·5	+0·1
	Sept.	10·87	814·87	−11·1	−20·3	·371	+0·4	+0·3
	Oct.	14·76	848·76	−11·9	−20·2	·379	−0·2	+0·2
	Nov.	16·71	881·71	−12·2	−20·5	·387	−0·4	−0·3
2012	May	16·12	56063·12	−12·6	−18·9	1·427	+0·3	+0·2
	July	25·91	133·91	−13·1	−18·7	·443	+0·2	0·0
	Aug.	20·91	159·91	−12·9	−18·6	·449	+0·6	−0·1
	Sept.	13·83	183·83	−13·3	−18·7	·455	+0·3	−0·3
	Nov.	5·73	236·73	−13·4	−18·3	·467	+0·5	−0·3
2013	May	9·14	56421·14	−14·7	−16·9	1·508	+0·4	−0·1

are almost identical that there may be difficulty in separating the components; that accounts for nine observations being reduced as single-lined, as shown in Table V. They are all in one batch, when the velocities of the two components crossed over one another in 2001/2.

The system has now been seen round a complete cycle with the Cambridge instrument, and it is possible to present a reasonable double-lined orbit. The observations are set out in Table V and lead to the orbit that is shown in Fig. 4 and whose elements are given below.

P = 4432 days (fixed)	$(T)_1$ = MJD 54169 ± 18
γ = −15·95 ± 0·03 km s^{-1}	$a_1 \sin i$ = 426 ± 3 Gm
K_1 = 7·19 ± 0·06 km s^{-1}	$a_2 \sin i$ = 445 ± 3 Gm
K_2 = 7·51 ± 0·06 km s^{-1}	$f(m_1)$ = 0·157 ± 0·004 $M_\odot$
q = 1·045 ± 0·011 (= m_1/m_2)	$f(m_2)$ = 0·180 ± 0·004 $M_\odot$
e = 0·230 ± 0·006	$m_1 \sin^3 i$ = 0·688 ± 0·013 $M_\odot$
ω = 258·9 ± 1·5 degrees	$m_2 \sin^3 i$ = 0·658 ± 0·013 $M_\odot$

R.m.s. residual (unit weight) = 0·36 km s^{-1}

The best-separated observations were reduced with all the parameters 'free', but rather more than half of them have had the profile parameters of the individual dips fixed at the mean values obtained from the best traces, so the principal remaining unknowns to be solved for are just the positions of the dips. The fixed values imposed upon most of the reductions are the ratio of the dip areas of the two components, and the rotational velocities. The ratio, 1·07 to 1, is determined with an uncertainty of little more than 0·01 as a mean value from 31 traces where the dips were well enough separated to warrant being reduced with all parameters free. Expressed as a difference in stellar magnitudes it is close to $0^{m}\!\cdot07$. According to a 'rule of thumb' adopted previously[80], the actual ΔV is estimated to be 1·15 times as great, *viz.*, $0^{m}\!\cdot08$. It corresponds to a difference in spectral type of about half a sub-type. Rotational velocities of 3 km s^{-1} were

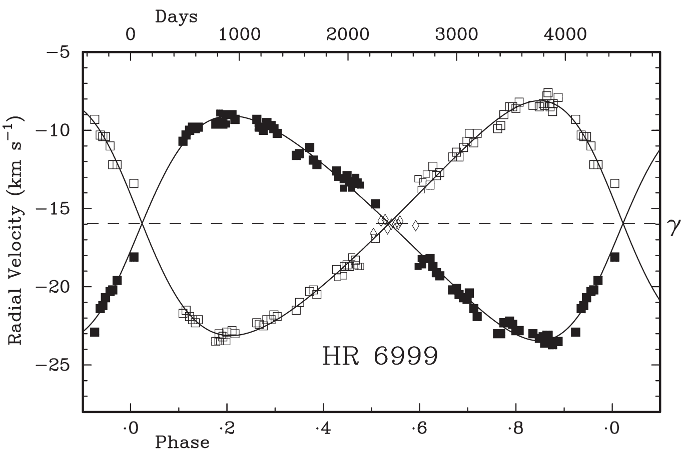

FIG. 4

The observed radial velocities of HR 6999 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*. The primary is shown by filled squares, the secondary by open ones. Open diamonds (in the immediate vicinity of the cross-over of the velocity curves near phase ·5) plot observations that were reduced as single-lined and were of course not included in the solution of the orbit. The other observations made before that time and immediately after it (before 2002 August) were attributed half-weight. They are plotted with smaller symbols and their velocities are noted with colons in Table V.

adopted for both stars, again on the basis of the 31 best-separated traces. Clearly the calculated separation of the blended dips is affected by the widths attributed to them individually. The velocity amplitudes found for the orbit, and the quantities derived from them (notably the masses of the component stars) are thereby subject to possible systematic error, not accounted for by their formal standard deviations, if the parameters adopted for the radial-velocity dips are inaccurate. The ratio of masses corresponds to rather more than one spectral sub-type; the discrepancy from the difference suggested by the dip ratio is not enough to cause concern.

The use of the astrometric value for the orbital inclination allows the substitution of $\sin i$ in the expressions for $a_{1,2} \sin i$, to obtain the actual orbital radii, and in the expressions for $m_{1,2} \sin^3 i$, to obtain the actual masses. Without wishing to 'look a gift horse in the mouth', the writer regrets to say that he believes that Hartkopf and his collaborators have had some difficulty with the part of their orbit-solving program that computes the standard deviations of the elements, at least of the inclination. In the 1989 paper that element was credited with the unbelievably small standard error of 0°·01, whereas now the value reproduced in Table IV is considered accurate only to 5°, which surely errs in the opposite direction. Looking at the diagram (not reproduced here) of the 'visual' orbit with the actual observations plotted, the writer — though admittedly inexperienced in this matter — believes that the inclination is good at least to 1°; although that guess is not as satisfactory as a correct calculation, it is adopted here as a conservative upper limit to the real uncertainty.

In that case, the value of $\sin i$ is 0·83 ± 0·01 (to a much greater precision than the two decimal places might suggest). Combining that value with the quantities $a_1 \sin i$ and $a_2 \sin i$ from the informal table above, we obtain the major axis of the relative orbit, $a_1 + a_2$, as 1049 ± 10 Gm or 7·01 ± 0·07 AU, where the principal contribution to the uncertainty, which may well be over-estimated, is the one assigned *ex cathedra* to the inclination. Equating that linear value to the angular separation 0″·1461 ± 0″·0008, from Hartkopf's new elements in Table IV above, yields the distance to the system as 48·0 ± 0·5 pc, which inverts to a parallax of 20·83 ± 0·22 milliseconds of arc, quite embarrassingly close to the (revised[12]) *Hipparcos* value of 20·85 ± 0·91 milliseconds. The masses found from the spectroscopic orbit, freed from the $\sin i$ factor, are 1·20 and 1·15, with uncertainties of a little over 0·02 $M_{\odot}$ — very plausible for ~F9 V stars that are a little above the Sun on the main sequence.

The epochs of periastron given by the visual and spectroscopic orbits differ by only 3 days, much less than the quoted standard error of either determination, so the agreement must be regarded as very satisfactory. Likewise, the values of ω, at 79°·7 (visual) and 258°·9 (spectroscopic) agree well within the errors given for either; they are expected to differ by exactly 180° owing to the ways in which they are respectively defined. It is noticeable in Table IV that Hartkopf's new ω is reversed from the one given by Aitken[61–63]. The reversal was first made by van den Bos[64], who gave ω as 78°·8 and whose implied decision to disagree with Aitken as to which component was the primary was followed by Starikova[66] (82°·87) and Hartkopf *et al.*[65] (81°·22). Söderhjelm[67], on the other hand, following the implication of the *Hipparcos* position angles, reverted to Aitken's decision as to the primary, giving ω as 258°. *Hipparcos*, however, was not very definite on the matter, giving[81] the Δm of the pair as 0m·29 ± 0m·28 (although from the separate magnitudes of the stars given in another place[82] the standard error of the difference would appear to be 0m·20). Its difficulty is readily understood, since the pair was far below the supposed resolving power[55] of its aperture of only 11·4 inches.

Although Table V and the orbit include only the velocities obtained with the Cambridge *Coravel* in the most recent orbital cycle, HR 6999 was seen round the complete previous cycle from OHP. Unfortunately the observations cannot be utilized in the orbit, because they have been reduced with the imposition of seriously erroneous dip profiles, and the velocities derived from them are meaningless. The writer has no access to the data base in Geneva in which they are stored, and it seems impossible now to obtain fresh reductions made with the proper dip parameters. Nevertheless there remains there what is believed to a perfectly good set of observations covering an additional cycle of the orbit and capable of improving the solution if only they were correctly reduced.

Naturally the visual orbit, which has been seen round nine times since Aitken's discovery of the system, has a period much better defined than the one given by the unaided radial velocities — it has a standard error of only two days. Therefore the visual period has been imposed on the spectroscopic orbit. It may be mentioned that exactly the same scheme was adopted by Hartkopf *et al.*[65] in their visual/speckle orbit: they adopted a period from the whole *ensemble* of observations, and then derived their final orbit from the speckle ones alone. That is what Hartkopf has also done in the case of the orbit he has contributed to Table IV above. A distinct systematic difference is apparent between the two methods of observation, in the sense that the speckle data (which are taken to be the more-correct ones) show angular separations somewhat smaller than the visually measured ones. The tendency for visual observers to over-estimate small separations was actually demonstrated in 1969 — from orbital residuals of visual measurements alone, before speckle interferometry was even invented[83], in a masterly paper by Couteau[84] — although, as an accomplished visual observer of very close binaries himself, he did not stand to benefit from his conclusion!

Couteau was in fact by no means the first to be aware of the problem. The likelihood of a systematic error of that sign in the visual measurements of close double stars had already been pointed out by Dawes[85] more than a century before. Aitken himself was well aware of it, and referred to it in the 1914 volume[3] of the *Lick Publications* that was entirely filled by his observations, on pp. 145 and 161 respectively, in the cases of OΣ[86] 400 (BDS[68] 9979; HD 191854) and τ Cyg. Moreover, he actually illustrated it in his own book (ref. 62, p. 63) from his own measurements of certain stars which he had observed with both the 12-inch and 36-inch telescopes. Stars that were 'close' for the former (separations $\lesssim 0''\cdot75$) were measured as closer still with the latter, where they were *relatively* easy objects. The effect was demonstrated in a table of comparisons for double stars of different separations measured with both telescopes; the table was reproduced from one by Schlesinger[87], who had compiled it from some of Aitken's published observations. Of course, the demonstration was not calculated to do Aitken any good as far as his reputation as an accurate observer was concerned, any more than the similar one benefited Couteau! — but they both could be seen as standing to gain more for honesty than they might lose for observational error!

HR 8653 (HD 215243)

HR 8653 is a $6\frac{1}{2}^{m}$ star about 35′ following ζ Peg. If one accepts the (normally reliable) photometry by Oja[88], $V = 6^{m}\cdot52$, $(B-V) = 0^{m}\cdot47$, $(U-B) = 0^{m}\cdot01$, the star is lucky to have a *Bright Star* designation, being slightly below the $6^{m}\cdot50$ ostensible limiting magnitude of the *Catalogue*[89]; there is, however, a listing by Eggen[90] (not retrieved by *Simbad*) of $V = 6^{m}\cdot46$, $(B-V) = 0^{m}\cdot455$,

$(U - B) = -0^{m}{\cdot}005$. The discrepancy might be considered evidence of variability, but the V magnitude transformed from the *Hipparcos* 'epoch photometry' is[91] $6^{m}{\cdot}51$, and the r.m.s. spread shown by the 90 individual transits that went into that mean value is only $0^{m}{\cdot}007$; there are only three smaller values among the 100 stars entered on that page of the *Hipparcos* catalogue, and they pertain to considerably brighter stars of about the fifth magnitude, so there is no support at all there for any change in the star.

Despite the colour index being much bluer than that of the Sun, and the *Henry Draper Catalogue* type[92] being F5, the first MK type that was listed for HR 8653 was[93] G8 IV. The discrepancy from the *HD* was much larger than for any other star in that listing. The tabulation[93] was so littered with misprints (including spectral types such as *V*0 and *U*1 III!) that a corrected version of the whole paper, of which it was the major part, was subsequently printed[94] in the same journal; but the type given for HR 8653 held fast. Heck *et al.*[95], however, demonstrated, by several different schemes for interpreting the photometric indices in the *uvbyβ* catalogue of Lindemann & Hauck[96], that HR 8653 is actually an F star. Heck & Mersch[97] went on to draw attention to the discrepancy between the photometry and the MK type when they made an effort to develop a scheme for predicting the latter from the former. In their initial paper they imposed the luminosity class and obtained a prediction of F6; shortly afterwards, having evidently gained confidence, in a second effort they[98] obtained a result of F6 V without imposing any restriction. Thus they actually obtained exactly the type that was found[71] by (Anne) Cowley, whose F6 Vn is much more acceptable than G8 IV, apart from the 'n' which implies that the spectrum is smeared by rapid rotation, as is sometimes the case with F-type stars but is not actually so in this instance.

It is unfortunate that, despite the original G8 IV being so obviously at odds with the colour index, and its having already been demonstrated to be anomalous, *and* superseded, it was nevertheless entered into the *Bright Star Catalogue*[89] and has since been uncritically transcribed into many of the papers relating to HR 8653. For example, Imanishi and his colleagues[99–102] have used it repeatedly as a standard star, to which they attribute a T_{eff} of only 5400 K, about 1000 K below the true value, in their galaxy investigations, which one might expect to have been seriously vitiated by such a gross error in a fundamental property adopted for a standard if the latter had any utility in the investigations at all.

Eggen attributed to HR 8653 on one occasion[103] (though not on others) membership in his 'Wolf 630 group'. (The designation comes from a somewhat obscure 1919 publication[104]; the star is actually HD 152751, a 9^{m} M dwarf near ζ Oph, notable for being the primary component of the nearest quintuple multiple star.) To obtain the space motion that was deemed proper to the group, the distance modulus of the star had to be $4^{m}{\cdot}13$. Of course Eggen was writing long before *Hipparcos* parallaxes were available, and the required modulus may well have appeared plausible at the time; the *Hipparcos* value, however, is[12] $3^{m}{\cdot}14 \pm 0^{m}{\cdot}15$. McDonald & Hearnshaw[105] made an independent assessment of the likelihood that the stars that Eggen had ascribed to the group were actually related to one another. They concluded that either the observational errors of the available proper motions and radial velocities must all be about 2·4 times greater than they were claimed to be, or that the 'cosmic scatter' in the H–R diagram of the cluster must be much larger than in the case of M 67, or that many of the stars assigned to the group by Eggen must not be members.

On the other hand, Bubar & King[106], who made a spectroscopic analysis of 34 of the 172 stars that Eggen had at one time or another assigned to the group, asserted HR 8653 to be one of 11 'probable' members. Those authors included in their paper a tabulation of modern kinematic data for their 34 stars, deriving the *U*, *V*, and *W* components of Galactic motion, with their uncertainties, but their interest seems to have been wholly in spectroscopy and they appear not to have taken any notice of the kinematic data that they themselves compiled. Eggen's criterion of 'moving-group' membership was the similarity in the Galactic *V*-velocities, which in the case of the Wolf 630 group were all constrained to be –33 km s^{-1} by the adoption[103] of individual distance moduli that caused that to be so. On any normal statistical basis, Bubar & King's whole set of 34 stars would fall as a group at the first hurdle, through not matching the basic kinematic criterion. Their r.m.s. dispersion, in terms of their individual discrepancies from the required *V*-velocity (here taken still to be Eggen's –33 km s^{-1}, but this result is not sensitive to the exact value) is 6·4 standard deviations! There are only eight of the 34 stars that are within 1σ of the proper value, and in most cases that is so not because they are particularly close to it but only because their uncertanties are particularly large and so large deviations are nominally acceptable: the eight cases of agreement within 1σ include five of the eight largest values of uncertainty in the *V*-velocity among Bubar & King's 34 stars.

It must be admitted that the present writer's attitude towards Eggen's (and others') assignment of objects to moving groups is fatally coloured by the recollection of a specific investigation[107] that he made some years ago and reported in this *Magazine*, on the 'Pleiades supercluster'. In her informal *PASP* 'annual review' for that year, Trimble[108] aptly concluded from that investigation that "there is apparently no Pleiades supercluster", and Taylor[109] commented that that was "a reaction … that may be widely shared".

The radial velocity of HR 8653 was first measured by Shajn & Albitzky[110] with the 40-inch Grubb reflector and a one-prism spectrograph giving 36 Å mm^{-1} at Hγ at the out-station of the Pulkova Observatory at Simeis in the Crimea. They obtained four plates and gave the result simply as a mean of –2·0 km s^{-1} with a 'probable error' of 3·4 km s^{-1}; the latter was an unusually large value but evidently not large enough to warrant a conclusion that the velocity was really variable. The four velocities were subsequently published individually, with dates, in the *Pulkovo Publications*[111]. They are, unfortunately, not of a character to assist with the orbit. The first three were obtained at intervals of only two days, and the first two differ by nearly 14 km s^{-1}; only the fourth one is effectively at a different epoch, and it is noted as 'very poor' and 'too under-exposed' and was even excluded by the Pulkova authors themselves in their calculation of the mean velocity. By fortunate chance the mean of –2 km s^{-1} that they adopted is very close to the γ-velocity found here, so the space-motion calculations of Eggen[103] and Bubar & King[106] can be taken at face value and do not require any modification to account for any error in the input value of the radial velocity.

The star was next observed for radial velocity by Anderson & Kraft[4], in the paper described in the *Introduction* above, and discovered to be a spectroscopic binary. That paper makes the offer, "Individual radial-velocity measurements can be supplied by the authors on request." — but after 40 years or so the implicit time limit on that offer proves to have expired! There are also two velocities from OHP given by de Medeiros & Mayor[1]. Those, together with the Simeis velocities, are listed at the head of Table VI, with the usual adjustment of +0·8 km s^{-1} in an effort to put them onto the Cambridge scale normally used in this series of papers. Table VI also sets out the 65 velocities obtained with the

TABLE VI

Radial-velocity observations of HR 8653

Except as noted, the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1929 Oct. 4·85*	25888·85	−10·0	$\overline{17}$·757	−9·8
6·85*	890·85	+3·8	·759	+3·9
8·83*	892·83	+1·5	·760	+1·6
1930 July 26·03*	26183·03	+10·2	$\overline{17}$·944	+6·2
1987 Aug. 31·06†	47038·06	−5·3	$\overline{3}$·181	−0·3
1988 Aug. 8·06†	47381·06	−3·9	$\overline{3}$·399	+0·3
2000 Aug. 30·11	51786·11	−4·8	0·195	+0·2
Sept. 17·03	804·03	−5·1	·206	−0·1
Oct. 8·92	825·92	−5·0	·220	0·0
Nov. 1·89	849·89	−4·8	·235	+0·2
Dec. 4·82	882·82	−4·7	·256	+0·3
2001 Jan. 6·75	51915·75	−5·0	0·277	−0·1
9·72	918·72	−4·7	·279	+0·2
July 1·11	52091·11	−4·3	·388	−0·1
Dec. 14·86	257·86	−3·9	·494	−0·5
2002 July 22·12	52477·12	−2·4	0·633	−0·4
Sept. 3·07	520·07	−1·6	·661	0·0
28·04	545·04	−1·4	·677	0·0
Oct. 12·97	559·97	−1·5	·686	−0·2
Nov. 4·93	582·93	−1·2	·701	−0·1
Dec. 9·84	617·84	−0·9	·723	−0·1
2003 Jan. 11·77	52650·77	−0·7	0·744	−0·3
June 28·10	818·10	+1·7	·850	−0·1
July 28·11	848·11	+2·5	·869	+0·2
Aug. 21·10	872·10	+3·0	·884	+0·3
Sept. 15·98	897·98	+3·1	·901	0·0
Oct. 16·96	928·96	+3·5	·920	−0·1
Nov. 12·88	955·88	+3·8	·937	−0·1
Dec. 15·79	988·79	+4·2	·958	+0·2
2004 Jan. 9·77	53013·77	+3·6	0·974	−0·1
June 17·10	173·10	−3·0	1·075	0·0
Aug. 7·09	224·09	−4·2	·108	−0·1
Sept. 1·05	249·05	−4·4	·123	0·0
Oct. 5·98	283·98	−4·6	·146	+0·1
Nov. 4·94	313·94	−5·3	·165	−0·4
Dec. 16·77	355·77	−4·8	·191	+0·2
2005 Jan. 12·73	53382·73	−5·4	1·208	−0·4
Aug. 7·10	589·10	−4·1	·339	+0·5
Sept. 3·03	616·03	−4·1	·356	+0·4
Oct. 5·00	648·00	−4·1	·377	+0·2
Nov. 4·95	678·95	−4·2	·396	0·0
Dec. 10·81	714·81	−4·0	·419	0·0
2006 July 13·11	53929·11	−2·3	1·555	+0·5
Aug. 11·08	958·08	−2·5	·573	+0·2
Sept. 8·07	986·07	−1·9	·591	+0·6
Oct. 3·06	54011·06	−2·6	·607	−0·3
Nov. 2·90	041·90	−2·0	·627	+0·1

TABLE VI (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O–C) km s⁻¹*
2007 June 28·09	54279·09	−0·3	1·777	−0·5
July 27·11	308·11	+0·8	·796	+0·2
Aug. 30·09	342·09	+0·6	·817	−0·4
Sept. 15·02	358·02	+1·9	·827	+0·6
2008 June 26·10	54643·10	+1·6	2·008	−0·1
July 13·12	660·12	+0·8	·019	−0·1
22·11	669·11	+0·3	·025	−0·1
30·12	677·12	+0·1	·030	+0·1
Aug. 11·07	689·07	−0·2	·037	+0·4
30·07	708·07	−1·4	·049	+0·1
Sept. 10·04	719·04	−2·2	·056	−0·2
18·98	727·98	−2·2	·062	+0·1
Oct. 1·95	740·95	−2·9	·070	−0·1
31·94	770·94	−3·7	·089	−0·1
2009 Oct. 8·96	55112·96	−4·7	2·306	0·0
Nov. 17·84	152·84	−5·2	·332	−0·6
2010 June 28·07	55375·07	−4·0	2·473	−0·4
July 30·14	407·14	−3·3	·493	+0·1
Aug. 30·10	438·10	−3·4	·513	−0·2
Oct. 6·96	475·96	−3·0	·537	0·0
2011 Sept. 28·05	55832·05	+0·2	2·763	+0·3
2012 July 25·13	56133·13	+4·0	2·954	0·0
Sept. 15·09	185·09	+3·1	·987	−0·1
Dec. 5·78	266·78	−0·8	3·039	0·0

*Published Simeis velocity[111]; weight 0.
†Published OHP *Coravel* velocity[1]; weight 1.

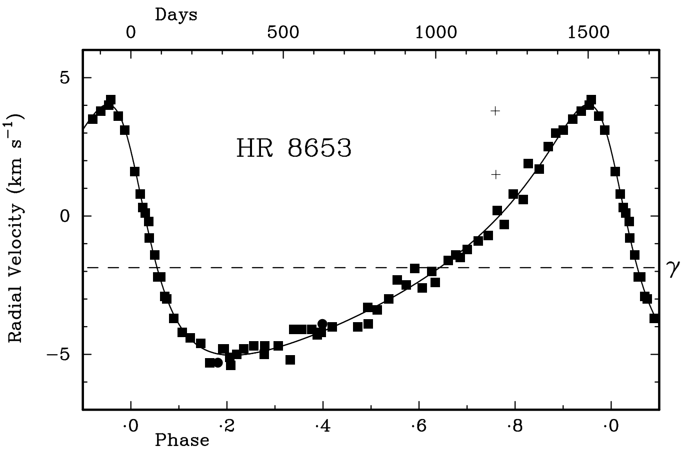

FIG. 5

Orbit of HR 8653, analogous to Fig. 2. There are two OHP observations[1], included in the solution of the orbit with full weight; only two of the Simeis ones[111], which were not used in the solution, are within the area of the diagram, and they are plotted as plusses.

Cambridge *Coravel*, starting in 2000. *They* have been subject to a zero-point correction of –0·5 km s^{-1}, which has been found appropriate to bluish stars such as the one treated here, to put *them* onto the intended scale. In the solution of the orbit, the OHP velocities have been given unit weight (the same as the Cambridge ones), and the Simeis ones have not been utilized although two are within the diagram of the orbit, Fig. 5. The elements are as follows:

P = 1575·5 ± 3·4 days	$(T)_1$ = MJD 53055 ± 6
γ = −1·87 ± 0·04 km s^{-1}	$a_1 \sin i$ = 86·2 ± 1·3 Gm
K = 4·53 ± 0·06 km s^{-1}	$f(m)$ = 0·0103 ± 0·0005 $M_\odot$
e = 0·477 ± 0·009	
ω = 50·9 ± 1·6 degrees	R.m.s. residual (wt. 1) = 0·26 km s^{-1}

The mass of the primary star can be estimated at 1·3 $M_\odot$, and that of the secondary is constrained by the mass function to be at least 0·3 $M_\odot$, corresponding to a star far down the M-dwarf sequence; it is not surprising that it has not been identified in the radial-velocity traces. Those traces show 'dips' that are appreciably broadened, no doubt by axial rotation of the star. The rotation is routinely quantified in the reduction of the observations, and the result repeats very well from one trace to another. Formally, the mean $v \sin i$ is found as 7·89 ± 0·13 km s^{-1}, but in view of the cavalier assumptions of standard turbulence and limb-darkening, *etc.*, rotational velocities determined in this way are not considered to be reliable to better than 1 km s^{-1} and the result is to be regarded as 8 ± 1 km s^{-1}. That is in exact agreement with de Medeiros & Mayor[1]. Previous results, given in papers that actually had the word 'rotation' in their titles, were < 10, ≤ 6, and 7·5 km s^{-1} (refs. 112, 113, and 114, respectively); the last was determined by measuring the half-widths of a number of absorption lines and 'quadratically subtracting' the width assigned to the instrumental profile, a method that the writer thought he had shown[115] to be dangerous more than 40 years ago.

Appendix

Cambridge Radial Velocities of HR 1687, HR 7756, and HR 7973

HR 1687 *HD 33608*		*HR 7756* *HD 192985*		*HR 7973 (15 Del)* *HD 198390*	
2000 Dec. 9·06	+35·7	2000 Aug. 12·01	−43·2	2000 Aug. 12·02	+6·2
2001 Feb. 12·92	+36·3	Sept. 2·98	−43·2	Sept. 2·98	+6·0
2003 Jan. 25·97	+34·6	2001 Dec. 7·75	−43·4	Dec. 9·77	+6·1
2006 Mar. 4·80	+35·3	2002 Jan. 1·78	−42·9	2002 Aug. 28·96	+6·2
2007 Mar. 3·87	+35·3	Aug. 28·95	−43·0	2005 Aug. 13·06	+6·3
2008 Feb. 27·81	+35·5	2003 Aug. 16·03	−42·4	2010 Jan. 1·72	+6·2
2009 Mar. 20·79	+35·4	2004 Sept. 4·98	−42·9		
2011 Sept. 28·20	+35·6	2005 Aug. 17·04	−42·4		
Dec. 6·03	+34·9	2006 July 13·04	−42·6		
2012 Dec. 2·09	+35·4	2007 Oct. 23·88	−43·5		
2013 Feb. 27·84	+35·8	2008 Aug. 15·02	−42·7		
		Sept. 27·88	−43·0		
		2009 Aug. 17·99	−42·7		
		2010 Aug. 31·05	−42·5		
		2011 Sept. 28·94	−42·4		
Means:	+35·44 ± 0·13		−42·85 ± 0·10		+6·17 ± 0·04

N.B.: Although the mean velocities can sensibly be computed to two decimal places, their absolute values are by no means so accurate, any more than the γ-velocities of the binaries are. That has been made quite clear from time to time previously in this series of papers, *e.g.*, refs. 116, 117.

Acknowledgements

In the course of writing this paper, I have been in correspondence with Dr. B. Mason of the U. S. Naval Observatory, who with his habitual kindness and promptitude has been very ready to supply information from the *WDS* data base in his charge. He forwarded an account of my interest in HR 6999 to Dr. W. I. Hartkopf, who has very kindly produced a new 'visual' orbit and allowed me to publish it in Table IV above. The number of observations available has nearly doubled since the syndicate in which Dr. Hartkopf was the senior author published the last well-documented orbital solution[65] in 1989.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Cosmic Confusion

Professor Liddle was perhaps a bit too polite in his response to Mr. Osmaston's statement[1] that the accelerated expansion of the Universe "is inferred from applying the relativistic Doppler formula to a linear distance–redshift relationship". A linear distance–redshift relationship exists, in general, only for small redshifts, where it can be used to measure the Hubble constant, while deviations from linearity provide information about the other cosmological parameters. In Friedmann–Lemaître–Robertson–Walker cosmological models, two parameters, apart from the Hubble constant, are necessary and sufficient for determining the entire expansion history (past and future) of the Universe. Thus, a non-linear distance–redshift relationship is essential for deriving the standard cosmological model Prof. Liddle discussed. (There is a linear distance–velocity relation, which holds for all Robertson–Walker models at all redshifts, but it does not involve observable quantities and does not play a role here, though this is a frequent source of confusion[2].) Furthermore, neither must cosmological recession velocities not exceed the speed of light nor is the relativistic Doppler formula used at all in modern cosmology[3]. (It is possible to use the relativistic Doppler formula in a cosmological context[4], though I have never seen this actually done except as an exercise. Cosmological recession velocities, as usually understood, can nevertheless still exceed the speed of light.) When discussing cosmological models based on General Relativity, whether one thinks of cosmological redshifts as being caused by cosmological recession velocities or not doesn't change the theoretical relationship between observed quantities and the quantities derived from them; the result is still valid. There is no debate at all about this within the established cosmological community. (Somewhat similarly, while there are many different interpretations of quantum mechanics, there is no disagreement when it comes to calculations connecting theory and observation.) Certainly no serious cosmologist sees modern cosmology threatened by an experiment done by the US Navy in the 1960s.

I agree with Professor Liddle when he says "that there are significant problems of trying to find fundamental understandings of the data", but this is not one of those problems. I also agree that "there should be more theoretical work", but none is needed in this area. I would also "like to see the pendulum drift back a bit from experiment to increasing the funding for theoretical exploitation and understanding", but readers shouldn't get the impression that this is needed in order to resolve any uncertainty about the issues raised by Mr. Osmaston, since there is none.

Yours faithfully,
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REVIEWS

Scientific Writing for Young Astronomers, by Christiaan Sterken (EDP Sciences, Les Ulis, France), 2011. Pp. Part 1: 162; Part 2: 298, 25 × 16 cm. Price Part 1: €36 (about £30); Part 2: €61 (about £52) (hardbound; ISBN Part 1: 978 2 7598 0506 8; Part 2: 978 2 7598 0639 3).

These two books form the proceedings of the Astronomy & Astrophysics School: Scientific Writing for Young Astronomers, which was held in 2008 and 2009. During three-day seminars, astronomy PhD students in the early stages of their studies were introduced to the process of scientific writing and publishing.

The first part of the proceedings (EAS volume 49) focusses more specifically on the journal *Astronomy & Astrophysics*, its history and the refereeing, editorial, and publishing processes. These detailed chapters give readers a look behind the scenes of a professional journal and explain the reasoning behind editorial policies on, for example, language use or the single–blind-refereeing process. The various issues that confront editors and publishers of *Astronomy & Astrophysics* on a daily basis will not be dissimilar from what their counterparts at other journals in different disciplines encounter, though obviously editorial policies may differ for a variety of reasons. As such, these chapters provide a good comprehensive general introduction into the size and scope of the processes that occur after a paper is submitted, something beginning PhD students do not always realize.

In part two of the proceedings (EAS volume 50) the focus shifts from the editorial process to the author. In three large chapters the writing process itself, communicating *via* graphics, and the ethics of publishing are unpacked in great detail. The first chapter deals with the writing process and offers numerous very practical examples on structuring a research paper in astronomy. The structure of such a paper translates well to other areas of the physical sciences, though may not transfer directly to the social sciences and humanities. The 'What to avoid at all price' section offers useful suggestions on improving the readability of a manuscript. The section on 'Frequently asked questions' offers a good insight into the writing and publication *mores* as they pertain to astronomy: the largely unwritten rules about co-authorship, authorship order, the etiquette around the submission process, whether to publish raw data, and working with the referee and editors.

The second chapter deals comprehensively with the graphical display of data. The central question this chapter aims to unpack is, "the data being equal, which graphical design conveys my message best?" (p. 89). Different types of graphs are discussed, as are the technicalities behind the use of colour, the resolution, and various other aspects of graphs that make them successful communicators of information. A long section is devoted to 'Lying with graphs', where the reader is cautioned against inconsistencies between the data and the representation of those data in the graph, accidental or not. Common pitfalls of communicating with graphics are again discussed in the wrap-up section, 'What to avoid at all price'. The third chapter deals with the ethics of scientific writing and publication. Various levels of misconduct are discussed, at the level of researcher, author, referee, editor, and publisher, as well as in interactions between supervisors and juniors. Here, as in the other chapters in both volumes, multiple illustrative examples taken from the authors' and editors' experiences as practising scientists, referees, editors, and supervisors offer insight into what can happen in the writing and publishing process, how people tried to gain unfair advantages over others, and how those situations were resolved. Throughout the proceedings, but in particular in this chapter, it is rightly pointed out that without strong professional ethics, not only will the scientific peer-review and publishing system be damaged, but science itself will suffer. Various reasons for engaging in misconduct, in particular the increased competition in the publish-or-perish culture that permeates academia, and possible solutions to combat unethical behaviour, are discussed.

In summary, the two volumes of proceedings provide a rich resource and reference guide not only for those who are embarking on writing a scientific paper for the first time, but also for those who are established practitioners in the field. While obviously the context of the proceedings are astronomy and its academic culture, many of the concepts covered in these two books readily transfer to writing for publication in other (scientific) disciplines. Teachers of academic writing will find ample material from which to construct workshops and seminars to support early-career academics in their writing and publishing efforts. What sets these proceedings apart from other scientific-writing resources is the dual emphasis of the technical 'how-to' on one hand, and the strong focus on professional ethics on the other. To become a well-respected scientist, one needs both. — ERIK BROGT.

The Adventurous Life of Friedrich Georg Houtermans, Physicist (1903–1966), by Eduardo Amaldi; edited by Saverio Braccini, Antonio Ereditato & Paola Scampoli (Springer, Heidelberg), 2012. Pp. 152, 23 × 15 cm. Price £44·99/$49·95/€53·45 (paperback; ISBN 978 3 642 32855 8).

Friedrich Georg Houtermans (variously Fritz or Fisse, sometimes all three in one paragraph) belongs in this *Magazine* because of a quartet of 1929 papers with Robert d'Escourt Atkinson, in which they moved briskly from "wave mechanics of radioactive nuclei" and "quantum mechanics of alpha-radiation" to "transmutation of lighter elements in stars" (only the last of the four in English, in *Nature*), *via* "*Frage der Aufbaumöglichkeit der Elemente in Sternen*". In the last two (pre-neutron!) papers, they proposed that lithium, beryllium, and boron might act as catalysts, capturing four protons interspersed with two electrons (which became "nuclear electrons") until a complete helium nucleus fell off, leaving the capturer to capture another day. If they had hit on CNO as the catalysts, we would perhaps remember them instead of, or at least in addition to, Hans Bethe and C. F. von Weizsäcker, who worked a decade later; but we do not (though I will take very modest credit for having cited the third German-language Atkinson & Houtermans paper in 1975). He also worked on cosmic rays, use of radioactive atoms for dating geological samples, and a number of topics in nuclear and particle physics, and was (perhaps) the inventor of thermo-luminescence as a dating tool. But if you read this book, it should be to find out about his extraordinary adventurous life. Fritz was imprisoned sequentially by the Russians and the Germans during World War II, and remarried his first wife after the war, not because of the prolonged separation (as it says in the *Biographical Encyclopaedia of Astronomers*) but because he had in the interim divorced her, married someone else, and added a couple more children to the world. Then she left him again and he married a third woman.

The book itself has led a fairly adventurous life. It was written by Eduardo Amaldi (one of Fermi's students and the only well-known one to remain in Italy throughout the war) more than 20 years ago; rediscovered by his son, Ugo Amaldi, who donated the manuscript to the Laboratory for High Energy Physics of the University of Berne, where Houtermans ended his career; and edited by the current director of LHEP and two colleagues.

Even reading the volume is a bit of an adventure. Not only does the protagonist go by several names, but a mysterious Prof. Wolfgang Paul (described as a mass spectroscopist and with slightly different dates from the familiar Pauli) haunts the pages. The text is far from idiomatic, and the narrative not as chronological as the chapter titles would suggest, but I do not regret a penny of the real [but discounted] $40 I paid to the Springer booth at the 2013 April meeting of the American Physical Society. Many old friends and friends of friends make brief appearances — Rudolph Peierls, Connie Dilworth Occhialini, Viktor Weisskopf, Peter Biermann, Paul Rosbaud (by last name only, but you should find out about him!), George Gamow, and Christian Möller, and one wishes they had lingered longer.

In any case, the book is definitely worth reading and perhaps even buying! Conflicts of interest? Well, I knew Atkinson (who once offered me a job at Indiana University) and Eduardo Amaldi (who established a gravitational-radiation detection group in Rome), but not Houtermans nor any of his other collaborators. — VIRGINIA TRIMBLE.

Newton's Gravity: An Introductory Guide to the Mechanics of the Universe, by D. W. MacDougal (Springer, Heidelberg). 2012. Pp. 433, 23·5 × 15·5 cm. Price £62·99/$89·95/€74·85 (paperback; ISBN 978 1 4614 5443 4).

This is a remarkable book — 433 pages of celestial mechanics, with no calculus, no vectors, indeed no mathematics or physics beyond what may reasonably be expected to be covered at high school. One would not expect to be able to cover much of celestial mechanics with such limitations, and I started out in a sceptical frame of mind. But the more I read, the more I was impressed.

The treatment is elementary and informal, and one wonders for what readership it might be suitable. Probably a university class in a formal programme in the physical sciences would expect more advanced mathematics than is used here. Perhaps it would suit a class in astronomy for non-scientists who know the basic rules of algebra and arithmetic and who are prepared for some quantitative work without the use of advanced mathematics; or an adult evening-education class; or amateur astronomers who profess to know little mathematics but who have always wanted to know how to do astronomical calculations. If students in such categories were to work seriously through the text (and, make no mistake, there is a lot of work) and carry out the numerous exercises, they will not be far behind (if at all) those who have taken a more formal university course.

The treatment is a mixture of historical and very modern. The author quotes directly and at length from Motte's translation of the *Principia*, as well as from other great classics such as Galileo's *Dialogo* and Huygens' *Horologium*. One appreciates from these excerpts what giants those early scientists truly were. The apple and the Moon were not enough to establish the universality of Newton's law of gravitation. He must also show that Kepler's third law applies to the planets around the Sun as well as to the satellites of Jupiter and of Saturn. As for the modern, the reader is led to several current web sites, such as those of JPL, for finding orbital elements and other data on Solar System bodies. Exercises in computing orbits are given using such recent discoveries as Comet Lovejoy, and such bodies as Sedna, Eris, Makemake, Quaoar and its satellite Weywot. If you haven't heard of all of these, neither had I!

Through carefully explained worked examples and numerous end-of-chapter exercises (solutions given), the reader learns how to calculate as a function of time the mean, eccentric, and true anomalies of a planet (though not the one step further to translate these into right ascension and declination). Kepler's third law is derived (without calculus) for *elliptical* orbits, not just circles. We are shown how to calculate the masses of binary stars (though I still don't know how people determine the masses of the so-called "exoplanets"). The elements of spaceflight, including Hohmann transfer orbits, are discussed. There are truly excellent chapters on the tides and on the Roche limit and mass transfer. And of course no book on celestial mechanics would be complete without a good discussion of the Lagrangian points.

The book is not without flaws. In view of the author's familiarity with the history of the subject, it was surprising to see a wrong date for the recovery of Ceres — one of the most significant events in the advancement of celestial mechanics. And it was amusing to read that the existence of Neptune was predicted by Leverrier and Couch. (Well done, Couch!) The author uses what he calls a modification of the Gaussian constant (done away with by the IAU last year!) that he calls κ and has the value 29·785. He does not explicitly say so, and one wonders if he missed it, but this number is just the mean speed in km s^{-1} of Earth in its orbit. He uses the word "velocity" throughout as a synonym for "speed"; thus the velocity of a planet in a circular orbit is constant, which it plainly is not. The SI units for G are wrongly given throughout. The impression is given that one can suppose all the mass of a planet to reside at its centre only if the planet is of uniform density. This is not necessary. All that is required is that the planet be spherically symmetric. The mathematical typesetting is not quite perfect, and some of the drawings could be better. For example, some orbits that are supposed to be circles come out as ellipses, which could be confusing to the unwary reader. In one passage the reader is invited to do a thought experiment and mentally shift quantities of mass back and forth between two masses; as long as the total mass remains constant, we are told, so would the period. Well, I tried this thought experiment, but every time I shifted mass between the two bodies, without changing the angular momentum, the separation between the bodies and the period kept changing, reaching a minimum when the two masses were equal. Then again, we read that the Earth's pull on the Moon is a little more than 81 times the Moon's pull on the Earth; and here I thought all the time that *actioni contrariam semper & aequalem esse reactionem: sive corporum duorum actiones in se mutuo semper esse aequales & in partes contrarias dirigi*.

Well, I could go on and on nit-picking and finding small imperfections such as these. But don't discard this book. I give it high marks, and anyone working through it thoroughly will acquire a pretty good knowledge of the subject, so go for it. — JEREMY B. TATUM.

In Search of William Gascoigne, Seventeenth Century Astronomer, by David Sellers (Springer, Heidelberg), 2012. Pp. 222, 24 × 16 cm. Price £90/$129/€106·95 (hardbound; ISBN 978 1 4614 4096 3).

William Gascoigne is one of those figures whose contributions to the history of astronomy and astronomical technology have largely been overshadowed by those of his continental contemporaries, such as Galileo, Scheiner, Kepler, and Huygens. He also (like his friends William Crabtree and Jeremiah Horrocks) exemplified a different style of doing astronomy from that of the great continental figures, for Gascoigne was not the recipient of royal, academic, or ecclesiastical patronage, but a Yorkshire private gentleman doing his own research in his own time out of his own pocket.

David Sellers has done a splendid job of bringing Gascoigne into broader scholarly understanding, a process first begun by Robert Hooke and Sir Christopher Wren in the 1660s. Several scholars, indeed, especially in the 20th Century, have worked on Gascoigne and his north-country friends, usually in the context of a Lancashire–Yorkshire astronomical group. But few have studied Gascoigne specifically. Sidney Gaythorpe did fundamental work on Gascoigne's optics in the late 1920s, but Sellers' book is far wider in its scope and thoroughness than anything that has gone before.

While living near Leeds, South Yorkshire, Gascoigne appears to have been a well-travelled man who was certainly well-informed about the work of Galileo and in particular of Kepler — both Kepler's work on planetary orbits and his published work on optics, which by 1620 was the most advanced in Europe. And it was Gascoigne's invention of the screw-controlled eyepiece micrometer that would secure his enduring fame in the 1660s, in the Royal Society. The micrometer, when placed at the eyepiece of a Keplerian telescope, made it theoretically possible to measure angles to an arc-second or so, depending on the quality of manufacture. And this device, it was hoped, would enable an astronomer to measure small changes in the size of the solar and lunar diameters so as to confirm Keplerian elliptical-orbital criteria.

Yet while Gascoigne's basic achievement is well known, much of his short life — 1612 to 1644 — is obscure. Exactly who were the Gascoignes? Were they Roman Catholics who 'trimmed' to Protestant legal requirements? Where was William Gascoigne educated, and how did he develop his astronomical interests? Sellers addresses these and other matters in this fascinating biography of a 17th-Century independent gentleman of science and his context.

What I believe makes this book especially valuable is its author's meticulousness as an archival historian, and the large body of primary research data upon which it stands. Its 60 pages of Appendices, reproducing letters and other documents, make it a natural starting-point for any future studies, not only of English astronomical history, but also of the wider social and regional context in which early astronomical discoveries were made.

My only regret is that this excellent piece of scholarship has been so *astronomically* priced, at £90, for it deserves a much wider readership than I fear the cost will allow. — ALLAN CHAPMAN.

From Ultra Rays to Astroparticles: A Historical Introduction to Astroparticle Physics, edited by B. Falkenburg & W. Rhode (Springer, Heidelberg), 2012. Pp. 345, 24 × 16 cm. Price £117/$179/€139·05 (hardbound; ISBN 978 94 007 5421 8).

We are abjured from judging books by their covers, but if voting based on indices and back matter is permitted, then this is an excellent volume. There is one index each of people's names, subjects, and back-references to the text from papers, *etc.*, cited (hardest to do, but very useful). Other tables include events from 1054 (supernova CM Tau) to the completion of the *ICECube* detector for very-high-energy neutrinos and cosmic rays (2010); a list of relevant Nobel Prizes; and compendia of textbooks (1921 to 2012 publication) and history-of-physics books. Am I to be found in some of these? Just barely, for a 1987 review of Dark Matter, apparently the last one to include all experimental numbers and all candidates up to the time of publication. My later discussions of the topic have been mere updates, as candidates went in and out of fashion and data improved. The 1987 review cited none of my own papers, but 777 by others, from Aaronson to Zwicky.

Falkenburg's & Rhode's ten chapters are also nearly all very good. They have one or two authors each, all European, except for Chapter 4, which has five authors, all named P. L. Biermann, at three German and two American institutions. The chapter topics range in mass from neutrinos to the Universe. All (the chapters, not the Biermanns) are more or less historically focussed.

Not every page is perfect (only Mary Poppins was perfect, said the late Joseph Weber). "Decades" has become "Decays" in the caption to Fig. 7.3; E. Margaret Burbidge is indexed under Margaret, E; and in the last couple of years, enough variability has turned up in the X- and γ-ray emission from the Crab Nebula and its pulsar to suggest that some of the results reported by K. I. Greisen and his balloon-flying colleagues might have been correct.

But, all in all, worth reading, though given the price per page, I'm sorry you can't have the review copy! — VIRGINIA TRIMBLE.

Gamma-ray Bursts, edited by C. Kouveliotou, R. A. M. J. Wijers and S. Woosley (Cambridge University Press), 2012. Pp. 344, 25 × 18 cm. Price £75/$120 (hardbound; ISBN 978 0 521 66209 3).

In only a few years from now it will be 50 years since the first gamma-ray-burst detection by the *Vela IVa* satellite on 1967 July 2 (although this detection was not formally published until 1973). Despite many subsequent GRB detections by mission such as *CGRO*, it was not until 1997 that the first redshift was obtained, which, along with all others since, confirmed that GRBs are extremely luminous extragalactic phenomena. Indeed, it could be argued that GRBs put the 'extreme' into 'extreme astrophysics'. More recently, the *Swift* satellite, launched in 2004, has provided multi-wavelength light-curves and rapid, arc-second-accuracy localizations for many hundreds of bursts. Studies of these objects and their host galaxies have led to the general consensus that you can make a GRB in one of two main ways: the death (collapse) of a single massive star, or the merger of two compact objects, most likely two neutron stars. In both cases a black hole (or possibly a magnetar) is produced and a relativistic jet is launched. If pointing at us, we see intense emission from the jet and from shocks as the jet interacts with its surroundings. For a brief period the luminosity of a GRB is truly extraordinary, equivalent to the instantaneous energy output of all the stars in a million galaxies. No wonder then that we can detect GRBs at all distances and explain their random distribution on the sky. Astronomers normally hope to discover a nearby example of a phenomenon which we can then study in detail. In the case of GRBs we had better hope none happens too close — within a thousand light years or so — otherwise we may all be in trouble!

There are so many satellites, ground-based telescopes, and astronomers involved in observing GRBs it's perhaps surprising that until now there has been no dedicated textbook describing both the observations and the theory. The editors of this new volume explain that they first had the idea around 1995. In that case it was probably fortuitous to be slow, as the observational detail available now far exceeds that available even ten years ago. The chapters have been written by experts in their respective fields and provide both an historical perspective and a detailed summary of where we are and what we don't know. There is still a lot we don't know, nicely summarized by Roger Blandford in the final chapter, but reading the 300 or so pages of this volume will certainly give one ideas of how to rectify that problem. The book roughly splits in two with the latter chapters explaining the theory of the emission and the progenitors while the earlier chapters describe the main GRB-discovery missions and the interplay between space and ground observations. A lot of GRB science demands rapid follow-up observations as the emission fades quite quickly. Fortunately, technology has advanced to the point where you too can be woken up immediately by a GRB alert wherever you are in the world or have your precious observation interrupted by a target-of-opportunity request. Maybe a future edition of this book will have a chapter on the psychology of observing GRBs.

My only minor quibbles are that some of the chapters are up to date to different dates and there are not enough colour figures. Nevertheless, the editors and authors are to be congratulated at managing to squeeze so much into a modest-sized volume. The hardback price is not so modest, but I strongly recommend it to anyone interested in how the most luminous objects in the Universe work. — PAUL O'BRIEN.

Fifty Years of Quasars: From Early Observations and Ideas to Future Research, edited by M. D'Onofrio, P. Marziani and J.W. Sulentic (Springer, Heidelberg), 2012. Pp. 583, 24·5 × 16 cm. Price £153/$229/€181·85 (hardbound; ISBN 978 3 642 27563 0).

I have never read a book like this. It consists mainly of questions posed by the editors to 53 researchers (including the editors). There is little overlap of material (except where different opinions are desired) and the book is well structured, but at the same time it reflects the thinking of many people. I'm not sure who the intended readership is, but it should prove useful to those who want a good introduction to specific areas of quasar research. (This applies both to those without any in-depth knowledge of quasars and to those who are familiar with some areas and want to learn about others.) The questions and answers provide a good summary, and the copious references (collected at the end of each of the nine chapters) can be followed up for details. Theory and observation are both covered. Five chapters discuss the current state of the field with a bit of historical background, but there is one chapter which involves personal recollections and another which looks to the future. These seven chapters are contained between the short introductory chapter at the beginning and the final chapter which assesses current thinking and future prospects. (The first and last chapters are written entirely by the editors and, like the penultimate chapter on the future, are much shorter than the rest.)

In the second chapter on personal recollections, Suzy Collin reminds us that in the 1960s "there was only one electronic device in France", while at the end of the same decade Joe Wampler was told by a colleague at Lick that he should "stop developing new gadgets and do some real astronomy". Of course, modern quasar astronomy is dominated by electronic gadgets, as impressively detailed by Paolo Padovani in Chapter 8.4 on the future of quasar astronomy. Martin Gaskell, though, cautions that "[h]aving software packages readily available that will fit gaussians means that a lot of gaussians get fit", and manages to back this up with a quote from Aristotle's *Posterior Analytics*. (Luigi Foschini later quotes Cicero, probably marking the only time both of those classical authors have appeared in a book about quasars.)

There are a few contributions from those favouring unconventional views on quasars, such as Arp and Narlikar. I'm familiar with Arp's ideas, but can't figure out what he means when he asks "Is there a connection between that series [Karlsson peaks] with implications of population dynamics or frequency as the smallest matter mode under musical control?" Probably Geoff Burbidge would have been asked to contribute had he still been alive. Mike Hawkins, on the other hand, seems to have softened his stance somewhat on the question as to whether a large part of quasar variability can be attributed to microlensing, mentioning this idea, with a caveat, only in the last paragraph of his contribution on variability (one of two, the other being on naked quasars). Towards the end of Chapter 4.6, three other authors mention that Lípari *et al.* have proposed that neutrinos produced in hypernovae "are the probable origin of [cosmological] dark matter", which I strongly suspect many cosmologists will doubt (despite Landau's claim that cosmologists are often wrong but never in doubt).

These are exceptions, however; the bulk of the book deals with mainstream quasar astronomy. The length of the book, the fact that the contributions are just summaries of the corresponding topics, and the many references demonstrate how much progress has been made in the last fifty years. For someone like me who is not a quasar expert, the book provides a good overview of the entire field. Since the contributions are more or less independent of one another, it is possible to read portions of the book without suffering from lack of context and thus get introduced to a specific field; most answers are rather brief (a few paragraphs) but are nevertheless mostly standalone. (Of course, references are provided for those wanting more details.) One contribution which is longer than average is that by Sergui Komissarov (pp. 384–396) which is probably as good an overview as any of comparable length on the physics of quasars. However, there are still many mysteries; in one of his many contributions, Martin Elvis spends several pages in Chapter 3.10 summarizing what we do not know about the X-ray properties of quasars.

Apart from the numerous references, the book has many features to improve its usefulness: a list of contributors with contact information, a list of acronyms (almost three pages), a list of web pages, and an index. Despite the contributions being from many authors, they all have a similar look and feel, and there are surprisingly few typographical errors (mostly harmless), especially for a book of this size; it looks as if the editors have done some real editing. Although probably for most readers more a book for dipping into than reading cover to cover, almost everyone interested in astronomy would probably enjoy reading Chapters 1, 2, 8, and 9 for a good overview of the past and future of quasar research in less than 150 pages. — PHILLIP HELBIG.

Circumstellar Dynamics at High Resolution (ASP Conference Series, Vol. 464), edited by A. C. Carciofi & T. Rivinius (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 439, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 810 7).

This volume is the proceedings of a conference sponsored by ESO and by several Brazilian agencies, held in Foz do Iguaçu in 2012 February/March. 'High resolution' applies in a number of contexts; here, the asserted remit is the temporal, spectral, and spatial domains, but in practice the last two (and particularly the third) dominate. 'Circumstellar dynamics' allows even more latitude, but while evolutionary phases from star formation to planetary nebulae are touched on, the focus is very much on early-type stars in general, and Be stars and their decretion discs in particular.

The strongest themes that emerge are innovative observational results from recent progress in imaging interferometry, and the flourishing new industry in observations and modelling of massive-star magnetospheres that has arisen following the development of 'second-generation' spectropolarimeters such as *ESPaDOnS*. These new results are matched by vigorous progress in theory, and the two are marching ahead in tandem. This volume reflects this dual attack, and gives the strong impression that with 'only' 60 or so participants, the meeting was more a workshop than a run-of-the-mill conference. The most interesting reviews have a definite 'work-in-progress' flavour, and the post-talk discussions, reported in full, convey a sense of animated (but friendly!) dialogues. The proceedings includes texts (not just abstracts) of all the contributed talks, with an author index (though no star/subject indices). It is enlivened by many photos of the participants, who evidently had an enjoyable if perhaps tiring day out at Iguazu Falls, and is enhanced by being published in full colour throughout — in striking contrast to the last IAU/CUP conference proceedings to pass through my hands (see **133**, 195, 2013), costing more than half as much again for a comparable page count in dreary monochrome. — IAN D. HOWARTH.

Astrostatistical Challenges for the New Astronomy, edited by J. M. Hilbe (Springer, Heidelberg), 2013. Pp. 235, 24 × 16 cm. Price £76·50/$109/€90·90 (hardbound; ISBN 978 1 4614 3507 5).

This is an excellent book, covering advances in statistical analysis of astronomical data. It includes contributions from a number of authors, covering topics which extend the standard methods of analysis of data. The introductory chapters by Joseph Hilbe and Tom Loredo set the scene perfectly, with many insightful and thought-provoking observations. The remaining chapters cover mainly individual techniques for treating data that have some characteristic which makes inference more complicated than usual. For the most part, these are descriptions of techniques that are in the published literature, but here they are collected together, and given enough space to allow a clear pedagogical treatment. Scenarios include Bayesian hierarchical models (dealing, for example, with supernova observations where estimates of redshift, flux, *etc.*, are available, and one marginalizes over unknown latent variables — the true values); the object-classification problem; a 'Fusion Monte Carlo Markov Chain' (MCMC) method to deal with multi-modal target distributions; the multi-nest algorithms, of interest especially for Bayesian evidence calculations; 'Bayesian Estimation Applied to Multiple Species' (BEAMS), for contaminated data; independent component analysis; Gaussian random fields, and techniques for looking for anomalies in cross-matched survey data when some data are missing. This book will be of great interest to researchers whose inference problems may go beyond what a standard MCMC algorithm can handle, or those who want a detailed but comprehensible summary of some recent advances in astronomical data analysis. It is highly recommended. — Alan Heavens.

Astrometry for Astrophysics: Methods, Models, and Applications, edited by W. F. van Altena (Cambridge University Press), 2012, Pp. 471, 25 × 19·5 cm. Price £45/$85 (hardbound: ISBN 978 0 521 51920 5).

A great many experts worked on this book, and many very useful chapters can be found in it. But it doesn't really fulfil its aims: a focus on micro-arcsecond astrometry, and providing a textbook that can fill a gap in this research area. There are large differences between the depths at which subjects are described in the different chapters, and there are overlaps in the subjects discussed, in a way that could easily lead to confusion. The most striking in this respect I found in Part II, 'Foundations of astrometry and celestial mechanics'. After the very detailed sections 4, 5, and 6, section 7 is out of place, in particular considering the target of this book, micro-arcsecond astrometry. Then section 8 is effectively an advertisement for a software package to handle the various transformations.

The main drive behind the techniques that make micro-arcsecond astrometry possible comes from astrometric satellites, and in particular the *Gaia* mission and its predecessor *Hipparcos*. In this context, an extensive discussion on the effects of the Earth atmosphere seems slightly out of place, while a more detailed description of how data from astrometric satellites is processed to create the catalogue, and in particular the parallax information, would have been much more relevant. Instead, the discussion on stellar parallaxes, for example, has its focus mainly on *HST* and ground-based observations, differential parallaxes for which micro-arcsecond accuracy is not really within reach.

For a textbook, the collection of chapters is too inhomogeneous, and too often misses the target aim of the book, the micro-arcsecond astrometry. It has the strong appearance of 'favoured friends' having each made a contribution, each in their own way, which also follows from the way it was conceived.

This provided some very strong and detailed chapters, and others that are very brief, that are out of date, or are sometimes somewhat irrelevant. It also led to partial overlaps between chapters in the topics covered, and differences in the way they are described. At the same time, aspects that are critical for understanding how micro-arcsecond astrometry is obtained through specialized satellites are missing. Nowhere is described why these satellites have two fields of view, why they operate in scanning mode, and why it is that in this way absolute rather than relative parallaxes can be measured. Still, that is the main source for reliable micro-arcsecond astrometry.

All together it feels a little bit like a missed chance, as well as a mission-impossible. The missed chance, because it presented the subject from the point of view of ground-based and differential observations, and too little on how micro-arcsecond data are obtained through space missions, in particular for absolute-parallax measurements. A mission-impossible, as it was conceived as a collaborative project, and I don't think a textbook should be put together this way. Even when dealing with two to three authors, issues of homogeneity may show, let alone when dealing with 28 authors of very different backgrounds.

Finally, however, there are some very good chapters included, giving detailed overviews of relativistic effects, interferometry, diffraction-limited imaging, CCD image detectors, *etc.*, which all have their relevance in the subject, and make the book a useful reference or starting point for more detailed reading. — FLOOR VAN LEEUWEN.

Cold War Space Sleuths: The Untold Secrets of the Soviet Space Program, edited by D. Phelan (Springer, Heidelberg), 2013. Pp. 300, 24 × 17 cm. Price £22·95/$39·95/€42·75 (paperback; ISBN 978 14614 3051 3).

This is the story of a dedicated — some might say obsessive — group of space enthusiasts who set themselves the challenging task of unravelling the secrets of the Soviet space programme during the height of the Cold War. Although the chapters are written by ten different 'space sleuths', each giving his own account of the difficult, painstaking detective work which was essential if progress was to be made, frequent mention is also made of other pioneers, such as schoolteacher Geoffrey Perry and cosmonaut expert Rex Hall, who passed away before this volume was produced.

In a world where Western and Japanese crew members train in Russian space facilities and travel in Russian spacecraft launched from a once-secret cosmodrome in Kazakhstan, it is hard to remember a time when Soviet censorship and deliberate disinformation were the order of the day. Without the benefit of classified information obtained by US spy satellites and intelligence services, these amateur researchers struggled to pull back the blanket of secrecy that covered countries behind the Iron Curtain. This volume tells the fascinating story of how they were able to piece together a credible, largely accurate, picture of the hidden Soviet space programme.

It is a story of anonymous cosmonauts and leading designers who were airbrushed out of history until the sleuths were able to establish their existence and eventually discover their names. It is also a story of listening to little-used radio frequencies, analysing orbital elements, tracking down obscure sources of information, and interviewing occasional visiting cosmonauts. As the authors admit, many of the Soviet secrets have been brought out into the open since the introduction of *glasnost* in 1985, but the work of the space sleuth is never done. Today, their efforts are focussed on similarly secretive régimes with active programmes, including China, North Korea, and Iran. — PETER BOND.

The 9th LISA Symposium (ASP Conference Series, Vol. 467), edited by G. Auger, P. Binétruy & E. Plagnol (Astronomical Society of the Pacific, San Francisco), 2013. Pp. 351, 23·5 × 15·5 cm. Price \$77 (about £50) (hardbound; ISBN 978 1 58381 816 9).

In his introduction to these proceedings Professor Danzman notes that it has been 20 years since the first *LISA* proposal was written. The proposal to detect gravitational radiation from cosmic sources using a laser-interferometer system carried on an ultra-stable system of satellites in space must have seemed unbelievably challenging at that time. This probably explains why after so many years, and numerous reviews, revisions, and refinements this pioneering space system has not yet been launched, although the technology demonstrator, *LISA Pathfinder*, is currently nearing completion, with a launch expected in the middle of next year. In a presentation which unfortunately has not made it into these proceedings Professor Cruise illustrates some of the challenges facing the development of this radically new technology for astronomy. (It may be seen at the *LISA 9th Symposium* web site.) Among his comments he amusingly notes the difficulty of presenting as exciting something which above all else is astonishingly stable.

As we have seen in the case of the *Gravity Probe B* satellite, a long gestation period is no guarantee that all possible effects have been accounted for. However, as *GP-B* has also demonstrated, the knowledge gained through the extensive preparation period not only sharpens the technology of the flight instrument, but also provides the tools for understanding and dealing with unexpected results and extracting meaningful signal from the flight data. The *LISA* project seems to be going through a similar process. Many of the papers in this volume are full of the details of potential sources of noise, their spectra, and methods used to overcome their effect such that the unique signature of gravitational radiation is not masked. As well as those technical discussions there are also papers dealing with the science, both of the gravitational radiation sources themselves and the fundamentals of their detection using large-scale interferometers.

This symposium, held in 2012 May, follows NASA's decision to withdraw from the project and the resolution by ESA to ask the European participants to re-define the project to be compatible with the budget of an ESA-only mission. The resulting proposal is *eLISA* and this project is thus central to many of the discussions in this volume. The volume also covers the technical details of the technology-demonstration mission *LISA Pathfinder*, which is currently in final flight-build stage. A full technical description of this mission provides the first paper of the symposium. In a subsequent paper Professor Sumner points out that this technology demonstrator is also capable of some interesting science once the technical phase is complete: higher-precision measurements of G, searches for effects of modifications to Newtonian gravity, and searches for Dark Matter.

As well as the space projects the symposium participants also discussed the ground-based gravitational-wave observatories. The network of first-generation interferometers — *LIGO*, *VIRGO*, and *Geo 600* — have now run several series of coincident observing runs and have so far not detected gravitational waves; also searches of their data conducted over periods coincident with over a thousand gamma-ray-burst events have proved negative. This is consistent with the known sensitivities of these devices. The designs of the second-generation interferometers are now well advanced with a promised factor of ten increase in sensitivity. *VIRGO* is currently being decommissioned and the advanced *VIRGO* is well under way and expected to be installed in 2014 with commissioning observations in 2015.

The space-based system of functioning *eLISA* cannot fail to detect the gravitational radiation from eight known binary systems with orbital periods ranging from 5 to 27 minutes — those sources, known as the 'verification sources', are among the potentially thousands of sources of gravitational waves accessible to this high-sensitivity system.

Fundamental-physics instruments such as *eLISA* get to the heart of our understanding of some of the features of the Universe. As recent observations of the cosmic microwave background have demonstrated, a new observing technique inevitably results in a deeper understanding. Measurements based on something other than photons are our only hope to examine the Universe during its first four hundred thousand years, and rather directly the information carried by gravitational waves is now central to alternatives to inflation-based cosmologies.

We are used to successful space missions, such as *IUE* or *SOHO*, spawning a series of conferences but in spite of *LISA Pathfinder*, *eLISA* may still be a long way from launch and yet the proceedings of this 9th symposium clearly demonstrates the persistence and fortitude of the community of astronomers pursuing the goal of detecting gravitational waves. This persistence is to be applauded both for the technical excellence of the project but also the prospect of the truly exciting science that awaits. — BARRY KENT.

Doing the Impossible: George E. Mueller and the Management of NASA's Human Spaceflight Program, by A. L. Slotkin (Springer, Heidelberg), 2012. Pp. 306, 24 × 17 cm. Price £22·95/$39·95/€42·75 (paperback; ISBN 978 1 4614 3700 0).

It was 1963 September. The cold war had devolved into a space race and President Kennedy had risked national prestige by committing the US to land an astronaut on the Moon by the end of the decade, but America's spaceflight programme was in some disarray. The organizations that were collectively part of NASA had become too autonomous, contract over-runs were commonplace, and budget overspends were eating into the programme's future; prospects were deteriorating and relations between the spaceflight centres were suffering from increasing friction. Action was urgently needed, and there was no scope for further mistakes. Enter George Mueller.

How Mueller — a satellite-communications expert from the air force (which *did* have experience in the successful management of major projects), and with that much-needed ability to analyze and understand *systems* — took the reins and remodelled NASA's organization into something that not only placed people successfully on the Moon in 1969 July but also went on to develop its remarkable space-shuttle system, is something that only Slotkin could have told to the degree, finesse, and understanding that is presented in this book. Having served as special assistant to Mueller in the latter's immediately post-NASA years, Slotkin got to know his subject closely, and was well supported in his research for this highly detailed account of six hectic years in the life of one (senior) NASA staff member. To his further credit, while the motivation for the book seems to have arisen through sincere veneration of its subject, the writing is just and dispassionate, and quite untarnished by adulation or sentiment.

Mueller was incredibly hard-working, following a gruelling schedule yet able to think very positively, even to the extent of being willing to undertake personally the risks that astronauts were expected to undergo. Between endless meetings, reports, lectures, testimonies to Congress, and the daily rounds of management tasks, problems, and challenges, he made time for public outreach, often mediating between NASA and the American public. It is all the more remarkable that through it all he managed to remain a somewhat unsung hero. This book puts that to rights.

The story is told with amazing thoroughness, and involves so many citations, people, and events that any lingering shreds of belief in a "moon hoax" could not possibly survive. Yet while the account goes almost remorselessly through every decision, report, meeting, and speech at some level, there is also a strong underlying sense of urgency. The clock is ticking, and the calendar of reported events cannot disguise the approaching end of the decade, the deadline which Kennedy had set his country. In many ways it's a thriller, and a non-fiction one.

In other ways, though, it is more of a scholarly account than a thriller. Carefully written, and clearly a labour of love, the account sometimes lacks the essentials of a good story — actual climaxes get less cover than the issues that were being argued over, and one misses the wood for the trees. Because of its attention to detail it is also a bit more turgid than is good for a thriller. But having said that, not only is it a lesson in big-project management, professional relationships, and national priorities, it is also a splendid tribute to its subject, the "father of the Space Shuttle", who rescued NASA and the American people from ridicule and subsequently slid back behind his horn-rimmed glasses into relative oblivion. It will delight all who study space exploits, and will become a milestone in the history of the development of human spaceflight. — ELIZABETH GRIFFIN.

CORRIGENDUM

In the report of the 2012 November Meeting of the RAS (*The Observatory*, **133**, 136, 2013), Mr. Osmaston's remark to Professor Liddle should have concluded, ". . . the redshift is a transmission effect."

Here and There

CLEARLY THEY FORGOT THEIR MAP

Scott and Amundsen ..., who pushed through blizzards, mountains and ice fields to reach an unremarkable location at zero degrees latitude south. — *Saanich News* (British Columbia), 2013 May 10, page A3.

SINGING FROM THE SAME (WRONG) SHEET

Goodricke, J. B., & Staatsoper, B. 1786, Phil. Trans. R. Astron. Soc., 76, 48 — reference in *ApJ*, **725**, 2399, 2010.