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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2005 October 14 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

K. A. Whaler, *President*
in the Chair

The President. A very warm welcome to the new session of meetings. It's nice to see lots of people here. We've had a summer recess and sadly over that recess some notable former members of the Society have passed away. I have to announce the deaths of Dr. Richard Twiss, who was awarded the Society's Eddington Medal in 1968, Professor George Isaak, who was awarded the Society's Herschel Medal in 1995, Professor Bruce Bolt, who was an Associate of the Society, and Professor Sir Hermann Bondi, who was awarded the Society's Gold Medal for Astronomy and who was also a Council Member, a Vice President, and a Secretary of the Society. In fact there were also two significant Committee Members of the Society who passed away: Dr. Dennis Walsh, who for a number of years was Treasurer as well as being a Member of Council, a Foreign Correspondent, and Chair of both the Finance and House Committees; and Dr. Richard Bingham, who was also a Member of Council. I would ask you all to please stand for a minute. Thank you.

Moving on, it gives me great pleasure to be able to present some awards. The RAS Gold Medal for Astronomy is awarded jointly to Professors Geoffrey and Margaret Burbidge. The Burbidges began their astrophysical work with the observation and analysis of stellar spectra. Early came α^2 CVn with its 10^5 enhanced abundance of europium, and then some barium stars like HD 46407, and the recognition that the two classes are different, with abundances tied to nuclear properties only in the latter. Their first paper on the abundances of the elements came in 1953, and the culmination of this effort was, of course, the well-known Burbidge, Burbidge, Fowler & Hoyle paper. Margaret's primary contribution to this was the stellar spectra, and Geoffrey's most important contributions the ideas that most heavy elements could be formed only by neutron capture and that the exponentially-decaying light curves of supernovae might well reflect the decay of some radioactive nuclide.

Next came rotation curves and masses for spiral galaxies, a series of papers beginning in 1959, with Kevin Prendergast and other young collaborators. Their papers, all joint, repeatedly made the point that nothing could be said about mass outside the largest radius where they were able to observe emission lines and plot a velocity. But they handed on the problem to the person who showed that a little light and lots of mass existed at larger radii: Rubin, together with the Burbidges, produced at least eight joint papers dealing with various galaxies.

By 1964 the Burbidges had begun to turn their attention to various forms of what we now call active galactic nuclei. Geoffrey first wrote on radio emission from our own and other galaxies and the connections with magnetic fields and cosmic rays in 1956, including the first calculation of the total minimum energy that must be present in some combination of magnetic field and relativistic electrons to account for a given flux of synchrotron radiation. The amounts turned out to be tremendous: 10^{60} ergs, the input of a billion supernovae or the rest-mass energy of a million Suns.

They wrote, with A. R. Sandage, on the evidence for violent events in galactic nuclei and were part of the joint discovery of absorption lines in the spectra of quasars. These are seen by most astronomers as steps on the march to a consensus model of active galaxies at their cosmological distances and fuelled by accretion on to a massive central black hole.

Their sheer productivity is sobering. Between 1960 and 1969, the Burbidges were authors or co-authors of more than 130 papers in refereed journals and major conference proceedings, plus a book or two, and short reviews in places like *Comments on Astrophysics*. They looked at pulsars and the Crab Nebula, the first generation of strong X-ray and infrared sources, and even Eta Carinae, as well as rotating galaxies and active nuclei.

The Burbidges have compiled an impressive record of service to the community as well — for instance, Geoffrey's 30-year tenure as editor-in-chief of *Annual Review of Astronomy and Astrophysics* and a quinquennium as director of Kitt Peak National Observatory. Margaret served the American Astronomical Society as its first woman president — just in time to introduce the first woman Russell Lecturer. Her non-confrontational ways of working around the barriers faced by women scientists have served as inspiration for several generations of women scientists. An RAS Gold Medal awarded to them jointly would be a fitting capstone to their lives and work.

I'll just add that I overheard Geoffrey talking at tea, saying "you'd think we hadn't done anything since 1969", but I can assure you that they have continued with their productivity! [Laughter.] I'd like to invite them both to come up and collect their medals. [Loud and prolonged applause.] And it's lovely to have three generations of the family here to celebrate the award.

Next, we turn to the Price Medal. This is to be awarded to Professor Gillian Foulger. Professor Foulger's research on Iceland and other hotspots has led to a major rethinking of a pillar of modern geophysics, the until-recently widely held view that hotspots, regions of long-lived excess volcanism such as Iceland, Hawaii, or Yellowstone, result from plumes of hot material upwelling from great depth in the mantle. In the plume model, plate motion over fixed or slow-moving plumes causes age-progressive linear volcanic chains and topographic swells that identify plumes and yield inferences about their properties. This model has been widely accepted because it gives an elegant explanation of how diverse volcanic regions have similar origins, an absolute reference frame describing plate motions relative to the deep mantle, and implies an important mode of mantle convection with major implications for the Earth's thermal and chemical evolution. However, Foulger's work has raised serious doubts about the model, especially in Iceland. She has made a strong case that the low-velocity material is restricted to the upper mantle, and that the island's great volcanic thickness is not due to melting at high temperatures, but instead due to melting of mantle that produces an unusually high melt fraction. In her model, Iceland results from excess magmatism as the ridge migrates over the Caledonian suture, containing rocks remaining from an earlier subduction period. These arguments — which at first seem surprising — have been supported by other studies. She has also been involved in studies of Yellowstone, which find even stronger evidence that the volcanism does not result from a deep mantle plume.

Foulger has taken the lead in a multidisciplinary reassessment of these issues *via* many publications, summarized in her recent (2002) paper in *Astronomy and Geophysics* and her website www.mantleplumes.org. She has organized international conferences and edited an upcoming GCA volume. These efforts have made the plume controversy one of the 'hottest' topics in global geophysics. However all this plays out, her creativity, energy, and willingness to tackle a complex multidisciplinary issue will have a major impact on geophysics for years to come. So I'd like to invite Gillian to come up and collect her medal. [Applause.] And those of you who have looked further ahead on the programme will have seen that we will be hearing from her a little bit later.

Next, the citation for Professor Rudolph Kippenhahn, who is awarded the Eddington Medal. Rudolph Kippenhahn was, until his retirement, Director of the Max-Planck-Institut-für-Astrophysik at Garching, and is nominated in recognition of his contributions to theoretical astrophysics, his leadership of both his research group and his Institute, and his several books on modern astronomy for the layman. The link with Eddington is manifest: Kippenhahn's principal research area has been the structure and evolution of the stars, like others building on the foundations laid down by Eddington in his seminal 1926 text *The Internal Constitution of the Stars*. The developments of the next generation were summed up in Martin Schwarzschild's 1958 monograph *The Structure and Evolution of Stars*, which has rightly become a second classic text. The phenomenal expansion of our knowledge during the second half of the last century led Kippenhahn and his late collaborator Ali Weigert to produce their 1990 text *Stellar Structure and Evolution*, a noteworthy companion to Eddington and Schwarzschild. While glad to present the results of their own and other numerical studies, the authors' pedagogic style echoes Eddington's insistence on 'insight'. In their words, "often enough, a simplified analytical example can be more helpful than the discussion of an exceptionally beautiful numerical solution", as exemplified by their use of a 'piston model' to mimic the mechanical and thermal properties of stars.

Kippenhahn and his collaborators have studied particularly the pulsations of δ Cephei stars, the evolution through the hydrogen-burning and helium-burning phases of upper-main-sequence stars, and evolution by mass exchange in close-binary systems. He has also written (with C. Moellenhoff) a book on elementary plasma physics, and very interesting papers on solar filaments, on the effect of anisotropic turbulence on the rotation of convective zones, on the non-linear development of non-adiabatic instabilities in rotating radiative zones, and on winds driven by photoelectric absorption of ultra-violet and X-radiation. And in his later years, like Eddington, he has reached out to the lay public through his non-technical writings, such as *Discovering the Secrets of the Sun*, *100 Billion Suns*, and *Light from the Depths of Time*. He is altogether an eminently suitable person to be recognized as one of Eddington's intellectual progeny. I'd like to invite him to come forward, please. [Applause.]

Now we have the RAS Awards for Services to Astronomy and Geophysics. Firstly, for services to astronomy, we recognize Guy Hurst. In all senses, Guy Hurst has been a leading member of the amateur astronomical community for over 30 years. Guy established the UK Supernova Patrol in 1976, encouraging amateur astronomers to search for these objects. His outstanding skills as a coordinator continue to ensure that amateur discoveries of supernovae, novae, and comets are rapidly verified and reported to the international community, resulting in a huge benefit to both professionals and amateurs alike. He has also been the editor of *The Astronomer* magazine for 30 years, providing a major forum for amateur astronomers involved in detailed observational work. Guy, being more than 'just' the editor of a magazine, takes a direct interest in the observations that are reported, and has devoted significant effort and a considerable amount of his own time to the promotion and organization of amateur astronomy. Serving as President of the British Astronomical Association from 2001–2003, he remains Vice-President, and continues to conduct and promote active observing of the night (and daytime!) sky. It is widely recognized that the work of active amateur astronomers is of considerable value to professionals, and it is fitting that Guy Hurst's work in bringing together the communities should be recognized with the Award for Services to Astronomy. I'd like to invite him to come forward. [Applause.]

And the Award for Services to Geophysics is to Professor Alan Douglas. Alan Douglas is the former Head of Forensic Seismology at AWE Blacknest. Since 1964 he has dedicated his career to using seismological observations to detect, locate, and identify underground nuclear explosions. The scientific challenge has been to differentiate such explosions from other man-made disturbances and, critically, from the background of thousands of earthquakes. The result has been to develop a new area of geophysical knowledge, namely forensic seismology, which is now applied worldwide to the benefit of society. Forensic seismology allows us to detect man-made explosions, ascertain that they are not earthquakes, and provide information to policy makers in international and governmental agencies. A recent example was the sinking of the Russian submarine *Kursk* in 2000 August. Analysis of the seismic observations determined that there were two explosions, 135 seconds apart, that critically damaged the submarine. These results provided a clear timeline of events that assisted later investigations.

Throughout his career Alan has maintained a close contact with universities. As Visiting Professor at London and Reading he has given a course of lectures, lectured in many different forums on seismology, and has personally supervised over 50 MSc and PhD students, a number of whom have become professors in Earth-science departments in the UK. He has frequently been asked by universities to give advice on the design of seismological experiments conducted within the UK and abroad. He has acted as PhD examiner and as external examiner for geophysics degree courses. He has made seminal contributions to both theoretical and observational seismology within the UK and enjoys an international reputation as an outstanding Earth scientist. In addition to this, Alan served as Chairman of the British Geophysical Association (BGA) for the years between 1996 and 1999, and although semi-retired maintains active contact with PhD students and their research as well as contributing to the Blacknest research programme.

By far the most important application of Alan's work is to detect and identify seismic signals from underground nuclear explosions. (Alan has published well over 100 papers on his research, most of them peer reviewed.) The application of the science of forensic seismology makes it much harder for anyone to carry out clandestine nuclear testing. This was the key technical step in building confidence that a comprehensive ban on nuclear testing could be enforced and thus in winning the international support that led to the signing of the Comprehensive Test Ban Treaty during 1996. Alan's scientific work has played a key rôle in bringing about this ban on nuclear testing. This was recognized by his appointment as a UK scientific delegate at nuclear test-ban debates at the Conference on Disarmament in Geneva, where he used his experience and wide professional knowledge to good advantage for the UK. In conclusion, Alan is being awarded the RAS Award for Services to Geophysics for his outstanding work, which has promoted the science of geophysics and developed its rôle in the life of the nation, in particular through his leading rôle in the development of forensic seismology. [Applause.]

Finally, in this section of the meeting, it's my great pleasure to award the RAS Michael Penston Prize to Dr. Haley Gomez from the University of Cardiff, for her thesis entitled *The Origin and Evolution of Dust.* Haley completed her PhD on the origin of interstellar dust at Cardiff University in 2004 November under the supervision of Professor Mike Edmunds and Professor Steve Eales. In her work she found that supernovae may produce a lot more dust than was previously thought, with about a solar mass of dust detected in the young remnants Cassiopeia A and Kepler (1000 times higher than previously detected). Haley was subsequently awarded a Research Fellowship from the Royal Commission of the Exhibition of 1851 to continue her research on dust formation in supernovae. She is now one year into this two-year Fellowship and hopes to continue her research afterwards. [Applause.] I should also point out that this prize is sponsored by PPARC and John Wiley & Sons Limited of Chichester, and we are very grateful for their continuing support, which allows us to make this prestigious award and enhance the careers of young astronomers. [Applause.] Haley is going to come and give us a talk about her research, which has led to this prize, in a future meeting.

I'll just announce a couple more things before I shut up for a while and you can hear some science, and that is to say that there are a couple of other congratulations that we can offer. These are to Dr. Guy Masters of the University of California and Professor Brian Kennett of the Australian National University, who have both been made Fellows of the Royal Society and both of whom are geophysicists with a strong interest in the Society. Brian Kennett, in particular, was the Pacific editor of *Geophysical Journal International* for many years. And also to congratulate Lord Rees on his elevation to the House of Lords. Apparently, the last former President of the Society to sit in the House of Peers was Lord Lindsay, the 25th Earl of Crawford, but that was well over a century ago. [Applause.]

I'd like to invite Dr. Danny Lennon to come up; he's going to talk about 'The Magellanic Clouds: laboratories for understanding massive stars'.

Dr. D. J. Lennon. I would like to start by acknowledging the contributions of Chris Evans and Carrie Trundle to the content of the presentation. This afternoon I wish to emphasize the importance of understanding massive-star evolution to our understanding of the Universe, but would like to point out that there are serious gaps in our knowledge of crucial processes which might affect massive-star evolution. I will focus mostly on mass loss and rotation and on the effect that chemical composition might have on stellar evolution. The Magellanic Clouds provide the ideal laboratories in which to study massive stars, the Clouds' known distances enabling one to determine radii and spectroscopic masses more accurately than for massive stars in our own Galaxy (which is important since the latest atmosphere/wind models require the radius as an input parameter). Further, the low foreground extinction makes the important UV and FUV spectral region more accessible to satellites such as *HST* and *FUSE*, enabling one to obtain estimates of the stellar wind velocities. Finally, the Clouds' chemical compositions enable one to study massive stars in metallicities lower than solar.

The Small Magellanic Cloud is of particular importance since its metallicity of one-fifth solar is comparable to that found in some high-redshift galaxies, and is low enough to allow meaningful comparisons with the predictions of stellar-evolution models. In the same vein, it has the additional advantage in that its present-day nitrogen abundance is only one-thirtieth solar, which permits secure identification of surface nitrogen enrichments that might result from mass-loss, mixing, and mass-transfer processes during stellar evolution. The use of *HST*/*STIS* and *FUSE UV* spectroscopy, combined with *VLT*/*UVES* studies in the optical, have recently provided key results raising important questions about our understanding of the evolutionary process affecting massive-star evolution, mass loss, rotational mixing, and metallicity. Many of these results were prompted by a high-resolution *HST*/*STIS* survey of massive stars in the SMC, which for the first time obtained UV spectra of metal-poor O-type dwarfs close to the zero-age main sequence. These spectra exhibit none of the usual wind features associated with such stars at solar metallicity, a characteristic echoed by their Hα line profiles. Attempts to quantify their mass-loss rates produce the surprising result that they are at least an order of magnitude lower than theoretical predictions, a result supported subsequently by other groups. It is also possible to show, from the same survey, how two O-type stars with similar luminosities and effective temperatures have radically different wind properties, though similar mass-loss rates. This was explained by supposing that one of two stars had evolved under the influence of rotational mixing, causing it to evolve towards higher luminosities and occupy the same locus in the HR diagram as a more massive non-rotating star.

Switching focus to the supergiant stars, we see how *HST* and *FUSE* observations show that wind terminal velocities in the SMC are similar to those in the Milky Way, in agreement with theory but in contradiction with some previous suggestions. In a *VLT*/*UVES* study of SMC B-type supergiants it was further demonstrated that Hα mass-loss rates are in substantial disagreement with theoretical values, theory predicting too low a value for earlier spectral types, but too high a value at later spectral types. The same trends were also shown to exist at higher metallicity in a study of supergiants in our Galaxy, this study also demonstrating that the standard assumption of a step-function in the trend of terminal velocity with effective temperature is incorrect. In summarizing O- and B-star abundances we find that out of a sample of 45 OB stars analysed to date, 42 of them have significantly enhanced surface nitrogen abundances. However, while the magnitude of these enhancements can be explained by models with rotational mixing, the observed rotational velocities of the stellar sample are well below those predicted by the models. The inherent biasses in stellar samples show that there is a very strong bias towards stars with low rotational velocities in all massive-star samples analysed to date. Of particular interest are new results for a pole-on Be star in the SMC, which showed no evidence for the nitrogen enrichment which one would expect from rotational mixing. I would like to suggest that magnetic fields might be relevant in this context.

The *VLT/FLAMES* Survey of Massive Stars is a project aimed at observing and analysing a large unbiassed sample of OB stars in the Large and Small Magellanic Clouds in order to try to resolve the problems raised by previous work. This project has already obtained high-quality spectra for 800 OB stars in young clusters by using ESO's multi-object spectrograph, *FLAMES,* on the *VLT.* The analysis of these data is currently underway but early highlights include the discovery of rare, newly discovered O2 and O3 stars, peculiar and normal emission-line stars, blue stragglers, and many massive spectroscopic binaries. I will conclude by emphasizing the potential of this large-scale spectroscopic survey.

The President. Thanks very much, Danny. Are there any questions? We've got time for one or two quick ones.

Rev. G. Barber. Could you say a little about what implications this has for Population III stars and required metallicities?

Dr. Lennon. For Population III stars there are no metals so there is no mass loss and, generally speaking, rotation is thought to be extremely important for these stars. So, for example, one explanation for the formation of GRBs at very high redshift is that very rapid rotation leads to homogeneous evolution, and essentially the star is fully mixed as it evolves along the main sequence. That is how something that looks like a fast-rotating Wolf-Rayet star without mass-loss would be formed and is one of the prerequisites in the collapsar model put forward for GRBs. It's also possible that there could be self-enrichment — that some metals could be mixed to the surface of the star during its lifetime in which there would be some mass loss. But generally speaking, for these Population III stars, rotation is a very important ingredient.

Rev. Barber. It doesn't work without it.

Dr. Lennon. That's right, nothing seems to work without rotation. I should have said that, although there are stellar-evolution models that include rotation, in general those are calibrated against the surface abundances of stars by comparing with observations. This is because the theories themselves are very uncertain and is another reason why we need those abundances, because, ultimately, that's how we're going to calibrate the models.

The President. Right, I think we should probably move on. Thank you very much for that. [Applause.] Our second speaker is Professor Jim Emerson, who's going to talk about 'Opportunities with *VISTA*'.

Professor J. Emerson. [The speaker began by explaining that *VISTA* stands for the *Visible and Infrared Survey Telescope for Astronomy*, which aims to do multi-colour imaging surveys from ESO's Cerro Paranal Observatory in Chile and is currently making good progress towards completion. The website is http://www.vista.ac.uk.

The 4-metre aperture on a good site with high-throughput optics will allow observers to go deep quickly whilst covering a large field of view (considerably larger than other comparable telescopes at present). The 1·65-degree-diameter-field-of-view IR camera will operate in the *Z, Y, J, H,* and K_S bands using 16

Raytheon HgCdTe 2k VIRGO detectors working between 0·84 and 2·5 microns, with excellent measured quantum efficiency and 0·34-arcsec pixels. The detectors will form a sparse 4×4 array and the speaker explained how six individual 'pawprints' (each with 0·6 square degree covered by detectors) at suitable offset pointings could be combined to produce a filled 'tile' (fully covering $1{\cdot}5 \times 1$ degrees), with each piece of sky being observed at least twice. There is the potential to add a visible camera later. Such deep surveys in several wavebands would allow astronomers to do statistical work thanks to the large numbers of objects imaged, look for rare objects, investigate large-scale structure, or repeat fields in order to find variable stars or transiting planets. The infrared allows observations of cooler objects and objects at high redshift where the dust is less obstructive.

ESO will allocate 75% of the observing time to public surveys, where the applications will presumably come from consortia, whilst the remaining 25% of time will be allocated to more-traditional PI-type survey work. An exposure-time calculator will be available to observers and it will also be able to give the efficiency of each observing method. There will also be a Survey Area Definition Tool to aid in tiling large areas. The advertised 15-minute, 5-sigma limiting magnitudes under specified conditions are 23·8, 22·5, 22·2, 21·0, and 19·8 in *Z*, *Y*, *J*, *H*, and K_S, respectively.

The enclosure was very close to completion last month and the telescope is being tested in Texas, whilst the camera is in integration and test in Didcot. The speaker guessed that a call for public-survey proposals would be made by ESO around 2005 December and a preliminary decision on which public surveys will be done would come around 2006 June. The integration of the whole system in Chile should be complete in about a year. The speaker guessed that the public surveys will start as soon as practicable in 2007 and that the first competition for 25% non-public-survey (PI-type) time will be completed in good time for the observing period starting 2007 September. The speaker concluded that *VISTA* will be the world's leading IR survey facility for a decade and now was the time for everyone in the ESO community to consider proposals for public surveys.]

The President. Thank you for a very quick tour through an exciting new facility. Are there any quick questions or comments?

Dr. G. Dalton. Jim, if I take off my lab coat for a minute and dig into my hat-box, I find I have a *VISTA* Science Committee hat from a long time ago. Could you comment on where that committee is currently, with respect to ESO and all of the work that was done in defining potential *VISTA* surveys back at the beginning?

Professor Emerson. The *VISTA* Science Committee has no formal rôle in allocating time. My guess is that the work the *VISTA* Science Committee did in formulating those surveys and the science that they would do would be something that people will build on when setting up the consortia that will apply to use the time. But the *VISTA* Science Committee has no special status with respect to ESO.

Dr. Dalton. I seem to remember that at one stage you and I and, well, ESO were talking about *VISTA* consortium membership on the allocation panel.

Professor Emerson. ESO will have a panel that recommends the time allocations and the *VISTA* Executive Board will appoint half the members of that panel, but that doesn't necessarily mean that they'll appoint members of the *VISTA* consortium — there are other suitable 'worthy' people out there as well.

The President. Right, I think we probably should move on, since we're a little bit pressed for time. Thanks very much again, Jim. [Applause.]

Our next speaker is one of our medallists from earlier. Gillian Foulger is going to talk about 'Hot spots and the mantle–plume controversy'.

Professor Gillian Foulger. Before we can discuss whether or not mantle plumes exist, we have first to define what they are. The term 'hot spot' was first coined by J. Tuzo Wilson in 1963. It was known that the Hawaiian Islands age to the northwest along the archipelago and Wilson suggested that this resulted from the Pacific plate moving over a region of the mantle beneath that was unusually hot. He termed this a 'hot spot'. The idea of mantle plumes came along eight years later, when W. Jason Morgan suggested that this, and 18 other 'hot spots', were fuelled by the continual influx of hot material from thermal plumes rooted in the deep mantle. The model quickly matured, and classical plumes were envisaged as bottom-heated convective up-wellings that rise through their own thermal buoyancy. It is generally assumed that they must rise from a major thermal boundary layer, and the only one known to exist in the Earth is at the core–mantle boundary.

Plumes were originally proposed to explain the excess volcanism observed at places such as Hawaii and Iceland, relative fixity between 'hot spots', and the regular time-progression of volcanism along island chains, especially in the Pacific Ocean. Laboratory experiments were performed that involved injecting coloured, buoyant material into fluid-filled tanks. These experiments produced vivid images of mushroom-shaped up-wellings and were influential in popularizing the model. Later, when powerful computers became available, numerical images and movies were produced.

Unfortunately, as is often the case, the observational evidence was less compliant. Despite a lot of searching, little evidence was found from petrology and other methods that 'hot spots' are hot. Many proposed 'hot spots' produce very small volumes of magma. Even Hawaii, the best candidate for a plume, has only produced an amount of magma corresponding to approximately one marine spreading segment for most of its ~ 80-million-year history. Only in the last five million years did it suddenly increase its output by an order of magnitude. Many 'hot spots' are not fixed relative to one another. The most spectacular example of this is Iceland, where volcanism has been centred on the mid-Atlantic ridge ever since its beginnings about 54 million years ago. This means that it has moved with respect to other Atlantic 'hot spots'.

A lot of age dating of island chains has been done during the last three decades to look for the predicted time-progressions. However, many chains were found to be not time-progressive, *e.g.*, the Line Island chain south of Hawaii, and the Cameroon volcano line in west Africa. Lastly, despite over three decades of seismology of increasing sophistication, the pipe-like structures predicted to lie in the deep mantle have not been reliably imaged. Again, Iceland provides a good example of this, and there, multiple local and global seismic experiments essentially all agree that the anomaly is confined to the shallow mantle and does not extend deeper than about 650 km.

Despite these difficulties, the reaction of many Earth scientists has been one of bewilderment that the evidence for 'what we know is out there' has not been found. Creativity has been applied in adapting the model to fit the observations. 'Contemporary' plumes can come from essentially any depth; magma can flow for thousands of kilometres laterally, accounting for volcanism that doesn't occur where expected. For example, it has been proposed that 'the' Iceland plume has in fact been fixed relative to other Atlantic hotspots but that magma from it has persistently flowed laterally to the mid-Atlantic ridge. Plumes have been proposed to be either narrow — a useful feature if they can't be seen in seismic experiments — or broad. Types that have no 'mushroom head', one head, or multiple heads have been proposed, and pulsing in the 'mushroom stem' has been proposed to account for volcanic production rates that deviate from the monotonic decrease with time predicted by the original model. The list goes on and on.

It's reasonable to adapt an original model in the light of new data. However, in the case of the plume hypothesis many scientists feel that things have gone too far and have reached the point where the model has become unfalsifiable — it is essentially observation-independent. Many papers amount to little more than reporting new observations and selecting from the rich palette of variants available to explain how the particular plume studied differs from the others. This approach is adding little to our understanding. As a result, plume scepticism has grown in recent years and a new generation of models has emerged that doesn't involve exceptionally high temperatures or the tapping of material from the deep mantle.

These models are based on the suggestion that anomalous volcanism is driven by plate-tectonic processes at the Earth's *top* thermal boundary layer, *i.e.*, its surface. There are two key elements — variations in lithospheric stress, and mantle composition. Simply put, the state of stress in the plate controls the location of volcanism, and the fusibility of the source material beneath governs the volume produced. The mantle is dehomogenized by processes at mid-ocean ridges, where basalt magma is extracted from the mantle to create new crust. This separates mantle material into fusible basalt and refractory residue. The fusible basalt crust, along with material accreted to its underside during its journey across the ocean, is re-circulated back into the mantle at subduction zones. Other processes such as lithospheric delamination can also introduce fusible material back into the shallow mantle. Experiments show that recycled fusible material can produce several times as much melt at a given temperature as peridotite, the rock commonly assumed to comprise the bulk of the mantle.

The most significant processes that cause stress to vary in the lithosphere are differential cooling and variable plate-boundary types. The great Pacific plate comprises about a third of our planet. Stress within it due to spatial variations in cooling rate has been modelled using finite-element analysis. This shows that stress from this source is orientated optimally for encouraging the propagation of a crack in the line of the Hawaiian Island chain. It has also been suggested that volcanism along the Cameroon volcanic line in Africa occurs because of the almost ninety-degree change in direction of the continental edge there. It would be interesting to model this using the finite-element approach as well.

In summary, the alternative proposal to plumes for the origin of anomalous volcanism is that it occurs where the stress field is extensional and where the mantle is unusually fusible. So how does this model fit the observations? 'Hot spots' preferentially occur where stress in the crust is extensional. One third of all 'hot spots' are at or near spreading plate boundaries. Others are in continental extensional zones such as the East African Rift and the Basin Range province in the western USA. The plume model can only be tested if it makes predictions. Currently it does not do this because in practice all its predictions are negotiable. The 'plate-tectonic-processes' model predicts extension and evidence for unusual fertile source composition. It does not require unusually high temperature in the mantle source.

The President. Thank you very much, Gillian, for a very clear exposition to a general audience. Are there any questions or comments?

Professor D. Lynden-Bell. To bring this matter home to astronomers, on the island of La Palma there is a big old crater with some doubt over whether it's volcanic or not, though most of us think it is. Then there is a hot spot of volcanism at the extreme bottom end of the island, which is now obviously active. It is reasonably clear that in some sense, though I'm not necessarily saying in a plume-sense, that the volcanism does move. Do you believe that this is true of La Palma?

Professor Foulger. We know that volcanism moves, especially volcanoes. I'm not sure of the distance between your two volcanoes, but the lifetime of a typical volcano is a few million years and eventually they just clog themselves up because of intrusions and more magmatism cannot occur there, so the magmatism has to occur somewhere else. Magmatism is very mobile.

Professor Monica Grady. Have you used your models to look at volcanoes on any other planets; for instance, Olympus Mons on Mars?

Professor Foulger. Yes, of course that's very important. People have exported the plume hypothesis to other planets and there are communities arguing vigorously about Venus and communities arguing vigorously about Mars. It has been suggested that Mars is a one-plume planet, but recently a paper has been published suggesting that Olympus Mons was caused by a huge impact. It's also been suggested that some hotspots on Earth are caused by large impacts, so occasionally I wander over to the astronomy department, as Roger Davies knows, and rattle a few cages there, asking if they can help us out with these impacts. It's been suggested that all the circular structures on Venus are due to impacts and there's a very adamant opposition group that wants to explain them as plumes that are generated internally. I think that there's scope for collaborative work in that respect.

Mr C. J. North. You did ask about an engineer; well I'm not an engineer but I was wondering whether you had considered naval architects because they've got experience of crack propagation causing ships to fall apart.

Professor Foulger. I think that's what's happening to the African plate! [Laughter.] I should try to find one.

The President. So hopefully you've learnt from Gillian's excellent presentation why we are living in interesting times in Earth sciences at the moment. Thank you very much again. [Applause.]

Our last speaker of the afternoon is Dr. Rene Rutten, who is talking about 'Scientific prospects of the laser guide-star system on the *WHT*'.

Dr. R. Rutten. Adaptive optics (AO) has become a well-established tool at several ground-based telescopes around the world, including the 4·2-m *William Herschel Telescope* (*WHT*) on the island of La Palma. Although the potential of AO is now well demonstrated, it is also well known that the scientific exploitation of 'classical' AO is severely limited by the need to have a bright star close to the science target for the purpose of measuring the wavefront distortions caused by atmospheric turbulence. In order to alleviate this problem drastically, we initiated a project to develop a Rayleigh laser beacon at the *WHT* that will deliver a bright artificial point source projected along the telescope's optical axes, thereby taking away the need to have a natural guide star. Here I will highlight a few of the key technical aspects and the new scientific opportunities that will follow from them.

The practical challenge is to project a bright laser beam to an altitude of 20 km above the observatory, bringing it to a sharp focus, and use the Rayleigh back-scattered return signal for sensing atmospheric turbulence. To implement a Rayleigh laser-beacon system with the existing AO instrumentation suite we need to design and build three main components: (*i*) a laser unit and laser-beam relay system that will transfer the laser light to behind the *WHT* secondary mirror, (*ii*) a beam-projection telescope that will be mounted behind the secondary mirror, and, (*iii*) a laser-beacon wavefront-sensor system.

Unfortunately, having a laser beacon will not completely take away the need for having a natural star in the field. Overall fast image motion cannot be sensed using a laser beacon and still requires a 'real' star. However, such a star can be quite faint and at a relatively large distance from the science target, thus increasing the sky coverage for the AO system from of order one percent to something close to 100 percent. Another drawback with the use of laser beacons results from its finite altitude, resulting in sub-optimal illumination of atmospheric turbulence, in particular when this turbulence occurs many kilometres above the observing site. Often on good observing sites, such as the one on La Palma, turbulence predominantly occurs at low altitude, which bodes well for the use of laser beacons.

In our case, the choice of laser hardware is defined primarily by the availability of suitable (and affordable!) solid-state lasers. Our choice has been a 30W pulsed Yb:YAG laser producing 515-nm green light. The back-scattered laser light will return from the whole of the atmosphere. In order to produce a tight spot for the purpose of wavefront sensing a fast shuttering system is used that will admit back-scattered light to enter a very brief period after the laser pulse went out. That way only light returning from around 20 km distance, set by the light-travel time, will enter the wavefront sensor. The complete system we are developing is quite complex, and therefore we have carried out many model calculations in order to understand its performance and limitations. The key results can be summarized by saying that under nominal seeing conditions the resulting image correction will approach 0·1 arcsec at near-IR wavelengths, while in the *R* band the image width will reduce by typically a factor of two. Although this performance in the visible wave bands is moderate in terms of Strehl ratios, it still results in a very attractive improvement for astronomical observations.

Once operational, the main scientific advances we expect will come from use of the *OASIS* integral-field spectrograph, as this instrument delivers rather unique capability and its design is optimized for use with adaptive optics. The Rayleigh laser-beacon system will open nearly the full sky to AO exploitation, providing new opportunities for large surveys that require the sky coverage.

I mention here a few examples of potential large science programmes that would make good use of the laser system. In recent years a large survey to study the kinematic signatures of the cores of nearby galaxies has been carried out using natural, seeing-limited, integral-field spectroscopy. This survey has provided exciting new insights into the dynamic properties and formation history of elliptical galaxies. One of those galaxies was also studied at higher spatial resolution, revealing a counter-rotating core that could only be uncovered thanks to the higher resolution attained. The systematic use of AO with laser guide stars would allow extension of this single example to much larger samples, allowing firmer conclusions to be drawn in a statistically meaningful manner. Another example where adaptive optics combined with a laser beacon could make a difference is the search for intermediate-mass black holes. These elusive objects can be revealed through their gravitational interaction with stars. A good place to search for these objects is the centres of globular clusters, where there are also many test particles around to trace out the gravitational potential. But at the same time the high star density results in confusion under normal seeing conditions. Adaptive optics will help here, but a laser is required since the available stars tend not to be bright enough to serve for wavefront sensing. Deployment of the laser will facilitate a systematic search. As a third and final example, maybe demanding for a 4-m-class telescope but worthy of an attempt, is spectroscopy of lensed high-redshift galaxies seen in galaxy clusters. In order to study the lensed galaxies in detail one usually requires high spatial resolution. Observations from space are of course possible, but access both to suitable spectroscopic instrumentation and to observing time is an important limiting factor. Adaptive optics could also help here, but again the scarcity of bright stars implies that a laser beacon will often be necessary. Our laser guide-star development will open up a new window in this field as well.

Apart from the examples mentioned above there are similarly exciting fields of observational astronomy that would profit enormously from the laser guide-star development at the *William Herschel Telescope*. I mention studies of jets and outflows in stars and active galaxies, the study of star-forming regions, classical-nova ejecta, population studies of dense stellar clusters, *etc*. At the Isaac Newton Group of Telescopes we are looking forward to the new and exciting opportunities that the laser guide star will bring. Last but not least, I would like to note that the work reported here has been made possible through a grant from the Netherlands Organisation for Scientific Research, and it reflects the combined effort of many people. We hope to be able to start to reap the results of the hard work towards end of 2006.

The President. Thank you very much. Are there any quick questions or comments?

Dr. F. J. Lowes. Is there any simple answer as to why seeing is more affected by wavefront retardation? You were saying that the twisting of the wavefront is much easier to handle than the retardation?

Dr. Rutten. It's not so much the retardation that occurs in the atmosphere, but the fact that temperature and density fluctuations in the atmosphere cause the wavefront to corrugate.

Professor M. J. Rycroft. How did you decide that 20 kilometres was the right height for your laser pseudo-star? Is it that it's in the lower stratosphere and so above the troposphere where all the turbulence exists?

Dr. Rutten. That's a very good question, actually, and there's not an easy answer, but to give you a flavour of the discussions when we were choosing the altitude, we wanted to go as high as possible and 20 km seemed an achievable goal once we had done the calculations for the expected return from that altitude. If we could go to 25 or 30 km, we would do it. The reason for going as high as possible is that you approach infinity and get a better illumination of the atmospheric turbulence, just as stars at infinity will see all the turbulence. So from our perspective, the higher the better and that's what we'll aim for.

Professor Grady. You mentioned safety considerations. Are there any problems with shining a powerful laser into the sky?

Dr. Rutten. There are many problems! [Laughter.]

Professor Grady. How long is the laser on for?

Dr. Rutten. Once you're observing, permanently. For many minutes or hours.

Professor Grady. So if an aeroplane flies through?

Dr. Rutten. The observatory is in a no-fly zone, so that helps. [Laughter.] Actually, we did the calculations and also took satellites into account. Luckily, legislation in Europe is not as strict as it is in the US. [Laughter.] Certainly, as far as satellites are concerned, there is no danger, but if you do the calculations, the chance of doing real damage to anything flying very fast in the sky is extremely small. But nevertheless, you need to consider it.

The President. I think on that note we should conclude! As a new feature, the librarian is offering to run tours of the library for new members or for members who have not previously taken the opportunity to look around. Meet him outside the Council Rooms across the courtyard at 6:15 pm and you'll be very welcome to have a tour of the library and some of the rare holdings in the archives that we have. And now, it's beyond the time we should have adjourned to the drinks party in the RAS apartments on the other side of the courtyard. I'll remind you that the next monthly A&G Ordinary Meeting will be held on Friday November 11.

Summary of the RAS Discussion Meeting

CONNECTING THE SUN TO THE EARTH

It is well known that the Sun has a significant impact on the near-space environment of the Earth. However, direct causal links between events on the Sun and those observed at 1 AU are not well understood. So, for example, accurate prediction of space-weather effects is still a distant goal. However, the ESA *Solar Orbiter* mission, which is approved with an earliest expected launch in 2013, will allow us to address this problem through a payload providing both remote-sensing observations of the Sun and *in-situ* observations of the solar wind at distances as close as ~ 0·2 AU. The success of this mission will depend on the two related sets of expertise, namely those of the solar-physics and space-plasma-physics communities. A timely RAS discussion meeting was held on 2005 October 14, which aimed to promote the links between the two communities and provided some groundwork for the critical bridge between the remote sensing and *in-situ* measurements.

The meeting opened with an invited review of the science goals of *Solar Orbiter* by Dr. Alan Gabriel from IAS, Paris, emphasizing the remote-sensing aspects. He noted that the 70-km equivalent pixel size of the high-resolution images on *Solar Orbiter* will provide an improvement of a factor of five better spatial resolution over previous missions. We know from recent improvements in spatial resolution (*i.e.*, from *SOHO–EIT* to *TRACE* resolutions) that with more detail we see the finer magnetic-flux-tube structures. These signatures will give us clues to understand processes such as nanoflare heating, the morphology of the streamer base (the source of slow solar wind), and the structure of active regions related to eruptive events, flares, and coronal mass ejections. Considering larger-scale magnetic fields, we will be able to observe the boundary of the fast and slow solar wind, with *Solar Orbiter* sitting just outside the *SOHO–LASCO C3* field of view (~ 45 solar radii). It is expected that the current sheet will be very thin this close to the Sun, and certainly will not extend the ±20 degrees seen at 200 solar radii. Switching back to small-scale magnetic-field structures, the low-level loops identified from *SOHO–MDI* field studies show 'salt and pepper' effects which constitute 80% of the field. This demonstrates that the magnetic field is closed, and this could be the source of coronal heating. The magnetic-field spatial resolution from *MDI* is 1500 km. Later in the planned mission, the spacecraft will gradually climb out of the ecliptic (up to 38 degrees solar latitude). This will provide a completely new viewpoint of polar plumes. There is a suggestion that the interplume regions are the real source of the solar wind. The polar view is the only way to make progress with this problem.

The next speaker was Dr. Tibor Toerek (MSSL). He described the elusive initiation mechanism of solar eruptions. This is the most poorly understood part of the standard models that describe flares and coronal mass ejections. He showed simulation and modelling work describing the kink instability, and illustrated how it occurs in many different examples of solar eruptions, discussing, for example, its rôle in both confined and ejective events. He is working on predictions from simulations on what will be seen at 0·2 AU by *Solar Orbiter*.

Dr. Josef Khan (Glasgow) followed with a discussion of whether coronal mass ejections (CMEs) or flares produce gradual solar energetic particle events (SEPs). The gradual events are often associated with CMEs (produced at a shock in front of the CME) and the impulsive ones are often related to flares. Dr. Khan has produced an alternative model of blast shock waves, propagating across the corona as fast-mode shocks, as a source of SEPs. This model appears consistent with the observations of coronal waves seen in soft X-rays, but *Solar Orbiter* observations would be ideal for validating this proposed mechanism for SEP production.

Dr. Giulio Del Zanna (MSSL) described solar EUV spectral irradiances, and their importance related to climate change. He has measured EUV irradiances with the *SOHO–CDS* instrument. The irradiances show a clear increase with the solar cycle, with more pronounced spectral lines formed at temperatures of over 1 MK.

Gemma Attrill, a PhD student also from MSSL, described measurements of changes in the solar atmosphere related to coronal mass ejections. These take the form of localized dimming in both EUV and X-rays, which appears to be caused by a mass outflow from the region. She has measured the dimming regions seen in the EUV and calculated the magnetic flux in these regions. It is found that the positive and negative magnetic fluxes balance, indicating a closed structure which may be the source region of the elusive flux rope that forms part of the CME.

Dr. Eduard Kontar from the University of Glasgow discussed the energetic electrons related to solar flares by using data from the *RHESSI* mission. The most popular flare model is the 'thick-target' model. However, none of the observed electron distributions had power laws which correspond to the thick-target model! Moreover, most power laws fell between the predictions of the 'thick-' and 'thin-target' models of solar flares. The conclusion is that there is some new physics going on that we do not yet understand. Further studies of the dominant energy-loss processes or the escape process, which change the electron spectra, are needed.

Professor Eckart Marsch from the Max-Planck Institute, Lindau, then gave an invited talk on *in-situ* observations that will be made with *Solar Orbiter.* He noted that *Solar Orbiter* will explore an uncharted region of the heliosphere as it moves in to 0·22 AU. A further key advantage will be the ability to examine both the solar surface and the space above from a co-rotating vantage point during certain phases of the mission. He emphasized the importance of being able to connect the solar surface to the heliosphere, for example, to understand the magnetic-field structure of coronal holes and its relationship to solar wind acceleration. As a further example, to understand the formation of the solar wind from active regions, we need reliably to extrapolate magnetogram results from the surface outwards.

Professor Marsch also discussed the dynamics and interactions of the solar-wind fields and plasmas — their plasma microphysics. He noted that temperature profiles for various isotopes have not been well measured in the inner-heliosphere solar wind. Kinetic processes are also important in the multi-component, non-uniform, and collisionless solar-wind plasma: there are many wave–particle interaction processes, and lots of free energy for instabilities. The solar wind also shows a breakdown of classical transport theory and the variation of electron distributions with heliospheric distance shows scattering by meso-scale magnetic structures. There are also lots of open questions concerning turbulence in the heliosphere (nature and origin of fluctuations, spatial evolution, origin of intermittency, microphysics of dissipation). *Solar Orbiter* will provide an opportunity to look at the radial development of these phenomena and allow detailed investigations into the underlying mechanisms.

In summary, Professor Marsch argued that some big themes for *Solar Orbiter* will be solar activity and the solar-wind response, the heliosphere as a turbulence laboratory, transportation by wave–particle interactions, the Sun as a particle accelerator, SEPs created at shocks, the sources of minor ions (*e.g.*, from dust?), dust from Sun-grazing comets, and possibly the neutrons and gamma rays from nuclear reactions.

Immediately before the lunch break Dr. Andy Breen (Aberystwyth) gave a brief talk on solar-wind structure as determined from radio scintillation experiments. In this technique, the solar-wind speed is determined along a ray-path by modulation of signals from a quasar by turbulence in that wind. He reported evidence of both fast and slow solar wind along the ray-path, and also of two modes in the fast wind, which may be dependent on the size of the associated coronal hole. The fast wind is also seen to be expanding equatorward, while the slow wind has more variable behaviour. In addition, the geometry of the streamer belt is important, with strong non-radial flows seen in association with interaction regions and CMEs.

Following the lunch break, Dr. Barbara Bromage (University of Central Lancashire) presented a study in which the occurrence of trans-equatorial coronal holes are used to predict the arrival of high-speed solar-wind flows at Earth, and therefore to predict the occurrence of auroral substorms. She presented data from 1996 demonstrating the connection between the holes, the fast solar wind, the impact on the terrestrial magnetopause, and auroral substorm activity. During 2005, the development of a coronal hole observed in May and June was used to predict and co-ordinate an *EISCAT* campaign in July. Preliminary results show that fast solar wind was indeed observed at the *ACE* spacecraft at the predicted time, and that subsequently there was a large expansion of the auroral oval.

Dr. Lidia van Driel-Gestelyi (MSSL) presented studies of solar eruptive events which were followed by observations of flux-rope structures in interplanetary space. She has calculated the magnetic flux and helicity of the active regions before and after eruption and in the flux-rope structures. In one event, the rotational component of magnetic flux within the cloud amounted to ~ 10% of the estimated ejected flux. However, for a reasonable estimation of the length of the cloud, the total flux therein reaches 90% of the ejected flux. A second, smaller event also showed roughly the same ejected flux as was subsequently observed in the magnetic cloud. Again this is an area for which *Solar Orbiter* data will prove invaluable.

Dr. Andrew Fazakerley (MSSL) described the characteristics of a region on the Sun which could be identified as the source of an interplanetary coronal mass ejection (ICME) that subsequently reached Earth. He demonstrated that a halo CME detected on 2004 January 20 most probably caused a shock that passed the Earth on January 22. He described the plasma and magnetic-field properties of the following ICME and showed that they relate closely to characteristics observed at the CME source region. In principle, solar remote-sensing data could have been used to predict correctly the related magnetospheric storm.

Dr. Steve Milan (Leicester) talked about a related spaceflight opportunity — the proposed Chinese *Kua Fu* mission — which addresses the Sun–Earth connection with three separate spacecraft. *Kua Fu A* is an L1 solar monitor, with a highly complementary payload to *Solar Orbiter. Kua Fu B1* and *B2* are Earth polar-orbiting spacecraft, phased in their orbit and instrumented to provide continuous imaging of the northern auroral oval. The launch is anticipated for 2012. Auroral imaging from past and current missions generally provides sufficiently long coverage (a few hours) to capture a full substorm cycle, but continuous coverage over days is required to capture a magnetic storm cycle. The Chinese are considering UK involvement in the fields-and-plasmas instrumentation, together with a wide-field camera.

Dr. Peter Young (RAL) discussed plans for a UV spectrometer on *Solar Orbiter*. The UK has a long history with this type of instrumentation, for example, on rockets, on *SOHO,* and on *Solar-B*. The *Solar Orbiter* instrument will give five times better resolution, plus provide the high-latitude spectroscopy above the poles — this viewpoint down into the coronal holes will enable us to understand the relationship between surface structure and solar wind acceleration. RAL is leading a consortium for this instrument and trade-off studies are underway.

The oral programme was completed by Richard Marsden, the ESA project scientist for *Solar Orbiter*. He provided an update of the current status of the mission and the ESA assessment studies which aim to consolidate the science requirements, define the spacecraft requirements, the orbit, *etc*. Launch is currently baselined for 2015 May (a 2013 opportunity is still possible, but less realistic, 2017 January is the backup) on a *Soyuz–Fregat*. The cruise phase is 3·4 years with chemical propulsion and gravitational assists at Venus and Earth. The science phase will last ten years while the spacecraft is in a 3:2 resonance orbit with Venus (149·8-days period). *Solar Orbiter* will go as close as 0·22 AU to the Sun and will reach 34 degrees solar latitude. On three of the orbits the spacecraft will orbit in close co-rotation to the Sun, when it is expected that the best links between the surface dynamics and the *in-situ* measurements can be made. Finally, Dr. Marsden outlined the likely timeline for the forthcoming Announcement of Opportunity from ESA.

In addition to the oral programme, nine poster presentations were made. Dr. David Williams (MSSL) presented a study of the eruption of a kink-unstable filament in region NOAA 10696. Dr. Adam Rees (Imperial) described multi-spacecraft observations of a complex solar-wind–ICME interaction. The complexity of the ICMEs demonstrates the necessity, in order to model fully these phenomena, to understand the source regions, the launch process, and the interaction with the solar wind. Dr. Chris Goff (MSSL) described the eruption of a flux rope and the rising of a plasmoid by using multi-wavelength observations of a CME. Dr. Jackie Davies (RAL) showed multi-satellite observations of the near-Earth plasma sheet and flank magnetopause during the passage of a CME past the Earth. Prof. L. Culhane (MSSL) showed that it is possible for confined flares to produce a disruption of the streamer belt.

Dr. Robert Bentley (MSSL) described recent advances in the European grid of solar observations, while Silvia Dalla (Manchester) presented the capabilities of the *ASTROGRID* framework, the UK's contribution to a global Virtual Observatory, for data analysis for Sun–Earth connection studies. A science case studying the geo-effectiveness of CMEs, based on solar, interplanetary, and geomagnetic data was demonstrated. Dr. Richard Stamper (RAL) described preparations for the International Heliophysical Year in 2007. The overall scientific objective of the IHY is to advance our understanding of the heliophysical processes that govern the Sun, Earth, and heliosphere; the Sun-to-Earth connection is thus a central part of its remit. Finally, Dr. Mike Hapgood (RAL) presented a poster describing the monitoring of the Sun–Earth connection for research and applications. He stressed the importance of the monitoring data, and its crucial rôle in providing both the context for and the link to space-weather applications. — LOUISE HARRA & CHRIS OWEN.

UNDERSTANDING ASTRONOMICAL REFRACTION

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Introduction

For the past two centuries, monographs[1,2,3,4] and textbooks[5,6,7,8] on spherical astronomy have all presented astronomical refraction in much the same way. The differential equation for the refracted ray is developed; series expansions are introduced that allow calculation of numerical values in the part of the sky where most astronomical observations are made; and the region near the horizon is usually ignored. Because these series expansions diverge before reaching the horizon, the few authors who treat refraction near the horizon have used entirely different expansions than the ones valid near the zenith, so that no unified picture emerges. In any case, the mathematical transformations used to evaluate the integrals entirely disguise the physics.

This standard treatment, while sufficient for the calculation and use of refraction tables, completely violates the spirit of Hamming's motto[9] that "The purpose of computing is insight, not numbers". In fact, the textbook presentation of refraction not only hides the physics of refraction behind changes of independent variable, but also misleads the reader by emphasizing small and moderate zenith distances, where refraction behaves quite differently than it does near the horizon. For example, most astronomers suppose that refraction is always proportional to the refractivity of air at the observer, even at the horizon. This approximation is useful at large altitudes, but is not exact anywhere; nor is it even roughly correct near the horizon. The result is a widespread misunderstanding of astronomical refraction, exemplified by Simon Newcomb's widely quoted[10,3,11] statement[6] that "There is, perhaps, no branch of practical astronomy on which so much has been written as this and which is still in so unsatisfactory a state". The truth of Newcomb's remark is underscored by the recent discovery[12] of both conceptual and numerical errors in Newcomb's textbook. Similarly, a textbook of spherical astronomy[13] has recommended, and the US Naval Observatory adopted[14], a refraction formula that is in error by more than Cassini's homogeneous model in *every* part of the sky. These errors result from a traditional emphasis on calculating refraction in a restricted part of the sky, while excluding the apparently uninteresting (but conceptually essential) region near the horizon. To overcome these mistakes, it is necessary to consider refraction more generally, paying particular attention to low, and even negative, altitudes; for it is only in this region that the *structure* of the atmosphere influences refraction appreciably, and effects that are present (but numerically negligible) near the zenith become large and obvious.

Those whose interest is confined to numerical values will find Fletcher's superb review[15] and Bennett's numerical comparisons[16], possibly supplemented by some more recent discussions[17,18,19] of approximate formulae, to be sufficient. Although calculating refraction is no longer "the foundation of astronomy", as Isaac Newton[20] called it, it remains essential for telescope pointing and control systems. Furthermore, refraction is needed to determine airmasses in correcting photometric observations for extinction, because the argument of the airmass function is refracted rather than geometric zenith distance[18]. Finally, refraction errors usually set the limit of accuracy in satellite geodesy, and in the use of the *Global Positioning System*. So a good understanding of refraction is required in observational astronomy, astrophysics, geophysics, and geodesy. Traditional textbooks do not provide this understanding, so a clearer treatment of refraction is needed. This article is meant to fill that need. As overemphasis on numerical calculations has obscured the optics of refraction, it is helpful to begin with some basic principles of symmetry, geometry, and physics.

Symmetry principles

Reversibility: The most basic symmetry property is the reversibility of light rays: light follows the same path between two points, regardless of the direction of propagation. This allows us to trace rays from the observer backward to their source — an extremely useful technique in what follows.

Path symmetry: If the atmosphere is horizontally stratified, so that the surfaces of constant density are concentric spheres — a good approximation — the path of a ray of light is symmetrical about its lowest point, where it is nearest the

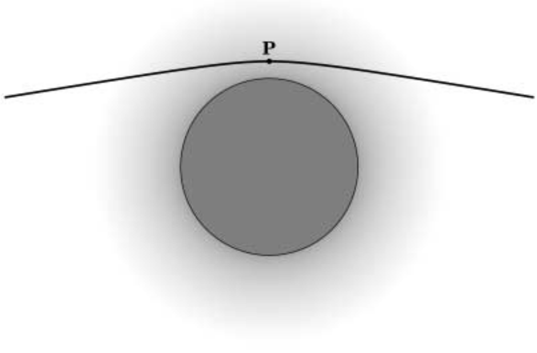

FIG. 1

The path of a ray (heavy line) is symmetrical about its perigee point, **P**. The shading represents the increasing density of the atmosphere toward the Earth's surface. The entire diagram is symmetrical about a vertical line through **P**, where the ray is horizontal. The height of the atmosphere and the curvature of the ray are greatly exaggerated.

Earth's centre (see Fig. 1). This point, **P**, is sometimes called the *apex* of the ray's trajectory; but as it is the lowest rather than the highest point, and need not even be the point of maximum curvature, the term *perigee* seems more appropriate.

Because of symmetry, the ray makes equal angles a at the points $\mathbf{O}_1$ and $\mathbf{O}_2$ where it crosses a level surface at a height h above the Earth's surface (see Fig. 2). If the Earth's radius is R_E, this surface has radius $R = R_E + h$. An observer at either of these crossings sees an object on the ray at an altitude a above the astronomical horizon when looking away from **P**, or at a depression of a when looking toward **P**.

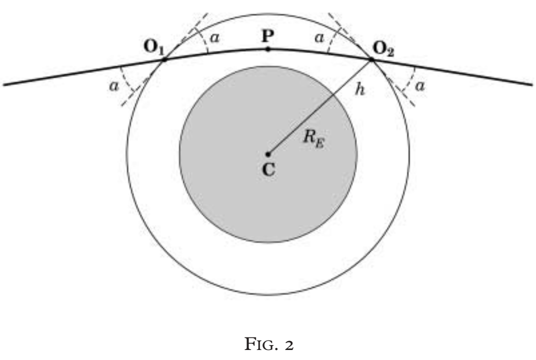

FIG. 2

The intersections $\mathbf{O}_1$ and $\mathbf{O}_2$ of the ray and a level surface of radius R at height h above the surface of the Earth, with radius R_E and centre at **C**. Dashed lines represent the planes of the astronomical horizons at $\mathbf{O}_1$ and $\mathbf{O}_2$.

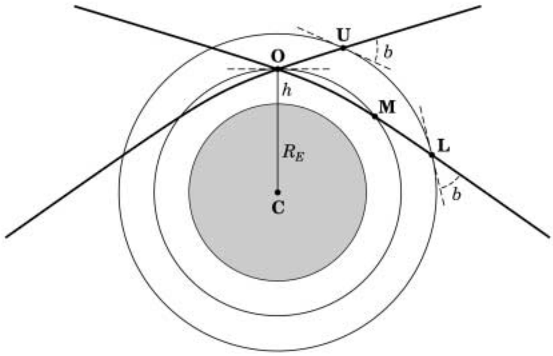

FIG. 3

Two rays (heavy curves) with the same perigee distance, seen by an observer at **O**. The entire figure is again symmetrical about the vertical line **OC**, but only the right half of the figure is labelled to avoid clutter. The observer's distance from the centre of the Earth is $R_o = R_E + h$, the radius of the circle through **O**. The outer circle is a level surface above the observer, at some larger radius $R_2 > R_o$. The plane of the observer's horizon, and those of the local horizons at **U** and **L** where the upper and lower rays cross the upper surface, are shown by dashed lines. The height h is smaller than in the previous figures, to keep the angles reasonably small.

This symmetry about **P**, where the ray is horizontal, means that no special method is needed to compute refraction below the astronomical horizon. If we can calculate the horizontal refraction from **P** up to $\mathbf{O}_2$, the refraction from $\mathbf{O}_1$ to $\mathbf{O}_2$ is just twice that. Then the total astronomical refraction at a depression of a is this double amount, plus the astronomical refraction at an altitude of a (*i.e.*, the ray-bending to the right of $\mathbf{O}_2$) — a fact first emphasized by Bouguer[21]. That fact was used extensively in the fourth paper[12] in this series.

Rotational symmetry: If the surfaces of constant density are concentric spheres, the shape of a refracted ray is independent of the position where it crosses one of these surfaces; the rays can be rotated rigidly about the centre of the Earth. So suppose we rotate two copies of Fig. 2 about **C**, so that the points $\mathbf{O}_1$ and $\mathbf{O}_2$ coalesce, as at **O** in Fig. 3. An observer at **O** sees two rays with the same shape and the same perigee height, one (**OU**) above the local horizon and the other (**OML**) an equal angle below it. The angular altitudes of the rays at **O** are the angles a in Fig. 2 (left unmarked in Fig. 3 to avoid clutter). These two rays cross a level surface **UL** above the observer at equal angles b. So rays at equal distances above and below the astronomical horizon meet any level surface above the observer at equal angles.

The equality of the angles b on the upper and lower rays is obvious in Fig. 3, where the symmetry of the rays is evident. In particular, one sees that the segment **OU** of the upper ray is identical to the segment **ML** of the lower ray. But if the parts of the rays to the left of **O** were omitted, the symmetry would be concealed, and it might seem surprising that the angles b are identical, because the ray segments **OU** and **OL** are so different in size and appearance.

Wegener's Principle: The fact that rays with apparent altitudes of $+a$ and $-a$ at the observer both meet any higher layer at the same angle b has important consequences. The equality of the b values for rays symmetrically placed above and below the observer's astronomical horizon means that a plot of b as a function of a is symmetrical about $a = 0$, the observer's astronomical horizon. So the horizon ray itself meets any level surface above the observer at a local minimum of b (*i.e.*, a locally maximal angle of incidence, measured from the local normal).

This fact was emphasized by Alfred Wegener[22]. Because of this extremum in incidence angle, the refraction contribution from the whole atmosphere above the upper layer is nearly constant within a small zone of sky centred on the observer's astronomical horizon. We may call this *Wegener's Principle*.

Thus, the upper atmosphere plays essentially no rôle in the rapid variations of refraction with angular altitude near the horizon that distort the setting Sun. Therefore Wegener[22] explained distorted sunsets by treating in detail only the refraction produced near eye level, and used standard tables for the refraction produced by the bulk of the atmosphere. Even though most of the atmosphere ordinarily produces most of the astronomical refraction, sunset distortions are almost entirely due to atmospheric structure near and below eye level. In general, Wegener's Principle lets us separate the atmosphere into a large, boring upper part, and a small lower region that produces interesting effects near the horizon.

Geometry

Flattening: One of these interesting effects at the horizon is the flattening of the setting Sun, which Kepler[23] first treated quantitatively using Tycho's empirical refraction table. The flattening is due to a *gradient* in refraction at the horizon that raises the lower limb more than the upper limb. This refraction gradient is due entirely to the density gradient of the air just below the observer. To see why, consider first Fig. 4a, in which the lower air has constant density, so that the rays are straight. Just as in Fig. 3, the ray above the horizon at **O** corresponds to the ray extending to the right at **M**, so we can regard them as the *same* ray, rotated about **C** by the angle between the two rays — *i.e.*, an angle of $2a$. This angle is equal to the central angle **OCM**, because the ray **OM** is straight.

Equivalently, if we draw the perpendicular from **C** to the perigee point **P**, the angle **OCP** is equal to a, because both are complements of angle **COP**. Again, the central angle **OCM** equals the apparent angular separation $2a$ of the two rays at the observer. Because the refraction of the two rays is equal, and occurs entirely above the observer, their angular separation outside the atmosphere is also $2a$. Therefore the apparent separation of the rays seen by the observer at **O** is exactly the same as their separation above the atmosphere, and celestial objects are not flattened at the horizon. The angular magnification of objects at the astronomical horizon is unity.

Curvature and magnification: Now consider a ray passing through the same points, **O** and **M**, but bent by refraction (Fig. 4b). The curvature of the ray decreases the angles it makes with the local horizons at **O** and **M**. Once again, the refraction above the observer's level is exactly the same for the two rays at **O**. Now, however, the refraction of the lower ray at **O** is larger, by just the ray-bending θ between **O** and **M**. Thus the separation of the rays at the observer is smaller than their separation outside the atmosphere by this angle, θ.

Of course, the angular separation of the rays *outside* the atmosphere is still the central angle **OCM**, as can be seen from the similarity of the upper ray at **O**

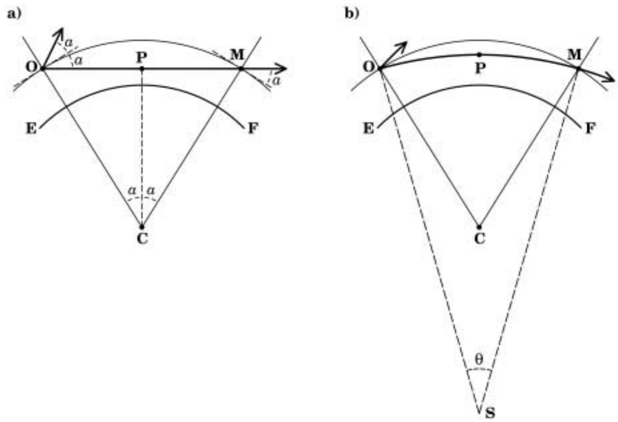

FIG. 4

a) A straight ray **OPM** in a constant-density atmosphere above the surface **EF** of the Earth, with centre at **C**. The upper ray at **O** corresponds to the ray **OU** in Fig. 3; the lower ray **OM** corresponds to **OML** in Fig. 3. Dashed lines again indicate the planes of the local horizons at **O** and **M**, and the angles *a* are indicated as in Fig. 2. b) A curved ray in an atmosphere with a density gradient below the observer. **S** is the centre of curvature of the refracted ray **OPM**, drawn here as the arc of a circle: note that **OS** is perpendicular to the ray **OPM** at **O**, and **SM** is perpendicular to this ray at **M**.

and the ray to the right of **M**, as before. So the apparent angular separation of the two rays at **O** has been diminished by exactly θ. As the true angular separation of the rays at infinity is **OCM**, and the apparent angular separation of the rays at **O** is (**OCM** – θ), the angular magnification m at the horizon is (**OCM** – θ) /**OCM**. But this ratio depends only on the ratio of the curvature of the ray to that of the Earth. The curvature of the ray is 1/**OS**, and that of the Earth is 1/**OC**; so the ratio of the curvatures is **OC**/**OS**. As the angles are small — a fraction of a degree — the length **OM** is practically the same, whether it is measured along the level surface or the ray. So, as the arc length **OM** is just the angle times the radius, the ratio θ/**OCM** = **OC**/**OS**. If this ratio is k, then $m = (1 - k)$. That is, if the ray curvature is half that of the Earth, $k = \frac{1}{2}$ and $m = \frac{1}{2}$. If the ray curvature were ⅓ that of the Earth, $k = \frac{1}{3}$ and $m = \frac{2}{3}$. In the real atmosphere, k is about ⅙; so the Sun at the horizon should be flattened by about ⅙ of its horizontal diameter. (Kepler actually got ⅙ by interpolating in Tycho's table.) The ratio k (or its reciprocal, $K = 1/k$) is used in surveying to correct observed terrestrial altitudes for refraction.

If the ray curvature equals the Earth's, $k = 1$ and the magnification is zero: the Sun is flattened into an infinitely thin line at the horizon. This actually occurs at the upper edge of a duct, where rays are trapped below a layer that produces strong bending. (Ducts will be treated in more detail later.) Such phenomena were noticed by Chappell[24], whose photographs of sunsets at Lick Observatory often showed a "final singular long line, which oddly enough is substituted for the small tip of light that could reasonably be expected as the final glimpse of a bright descending sphere". To relate magnification at the horizon to local atmospheric structure, we need the value of *k*, or the actual value of the ray's radius of curvature. This curvature depends on the vertical gradient of refractivity, and hence on the gradient of density, at the observer.

Ray bending

Density gradients: Consider an atmosphere at rest, in hydrostatic equilibrium. If the atmosphere were isothermal, the density would simply be proportional to the pressure at every level. Because the isothermal scale height is $H = kT/mg$, where *m* is the mass of an average molecule and *g* is the acceleration of gravity, *H* is very nearly 8 km. Then the density would diminish by nearly 1 part in 8000 for every metre of height. The refractivity $(n - 1)$ is very nearly proportional to the density of air, so the refractivity would also decrease by 1 part in 8000 per metre.

Before computing the ray bending due to this density gradient, let us consider the temperature gradient required to keep the density constant. If the surface temperature is 300 K, we would need to decrease the temperature by 1 part in 8000 per metre, giving a lapse rate of 300 K/8000 m = 0·0375 K/m, or 37·5°C/km, to maintain a uniform-density atmosphere in hydrostatic equilibrium. (This would be unstable against convection, but that is of no concern here.) Rays in this constant-density atmosphere would be straight, as in Fig. 4a. The value usually given[2,22] as the lapse rate corresponding to constant density is 34·2 K/km, corresponding to STP. As the value is proportional to the assumed surface temperature, which is usually above freezing, we can adopt 35 K/km as a more typical value. (Newcomb's value[6] of 1° in 34 metres at 10°C is too small; perhaps he meant 34°/km.)

Circular rays: Next, consider a ducted ray, which exactly follows the curve of the Earth. This case is easier to understand if one remembers that 'rays' of light are a non-physical abstraction; the closest thing to a 'ray' in reality is the central line of a beam of light. (This avoids the confusion that some people[25,26] have had in trying to apply the sine-law of refraction to horizontal 'rays' — although such problems had been correctly treated a century earlier[27,28].) Fig. 5 shows a horizontal beam of light, whose wavefronts are everywhere vertical. On the left,

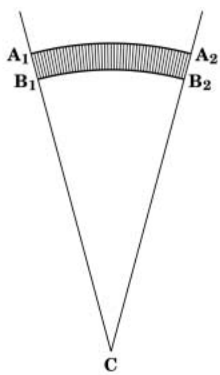

FIG. 5

A collimated horizontal beam of light, following the curve of the Earth, with centre at **C**. A few vertical wavefronts are shown; the surface of the Earth is omitted.

the vertical wavefront is $\mathbf{A}_1\mathbf{B}_1$; on the right, it has moved to $\mathbf{A}_2\mathbf{B}_2$. Obviously, the upper side of the beam moves farther (from $\mathbf{A}_1$ to $\mathbf{A}_2$) than the lower, which goes only from $\mathbf{B}_1$ to $\mathbf{B}_2$. The upper and lower edges of the beam traverse their distances in the same time, and contain the same number of wavelengths. So the speed at each edge must be proportional to its distance R from the centre of the Earth, **C**. But the speed of light in some medium, such as air, is inversely proportional to n, the refractive index of the medium. As the speed is proportional to R, the refractive index at each radial distance from **C** must be inversely proportional to R; so the product nR must be constant. This is the condition for a beam (or ray) to remain horizontal as it bends around the Earth.

Now consider the temperature gradient required to produce this condition. As R is about 6400 km, nR will remain constant if n decreases by 1 part in $6 \cdot 4 \times 10^6$ for each metre increase in height. But the refractivity $(n - 1)$, which is proportional to the air density, is only about 1/3200 of n; so the density must decrease by $3200/6 \cdot 4 \times 10^6$ m, or 1 part in 2000 per metre of height. The decrease in density due to the pressure gradient alone is 1 part in 8000 per metre, or ¼ of the required amount. So the temperature gradient must supply the remaining 3 parts in 8000. At 300 K, the temperature must increase upward by 3/8000 of the 300 K, or 900/8000 = 0·1125 degree per metre. As this gradient has the opposite sign from the usual lapse rate, it is a temperature inversion; the lapse rate is negative.

The argument presented here is crude, but close to the truth. For comparison with the rough value of –0·1125 K/m just derived, Lehn[29] gives –0·1127 K/m as the critical lapse rate, Wegener[22] gives –0·114 K/m, Newcomb[6] gives –117° per km and de Graaff Hunter[30] gives –0·066°F/foot, which corresponds to –0·116 K/m. Let us adopt –115 K/km in what follows. Notice that this critical temperature gradient is over 17 times larger in magnitude than the 6·5 K/km lapse rate of the Standard Atmosphere[31]. That means that the standard lapse rate has hardly any effect on ray curvature, which is due almost entirely to the pressure gradient under normal circumstances. (This fact justifies the use of an isothermal model at the start of this section.)

Bending and lapse rate: Now that we know the lapse rates required to produce straight rays and rays that circle the Earth indefinitely, we can calculate the bending that corresponds to any given lapse rate. The curvature of a horizontal ray is proportional to the vertical density gradient of the atmosphere. The density is inversely proportional to the temperature, so the density gradient is the negative of the temperature gradient or lapse rate, offset by the contribution of the pressure gradient. So, if a lapse rate of 35 K/km would produce straight rays ($k = 0$), and –115 K/km would produce circular rays ($k = 1$), a lapse rate of γ will produce a relative curvature of

$$k = \frac{\gamma - 35}{-115 - 35} = (35 - \gamma)/150.$$

Consequently, the standard lapse rate of 6·5 K/km corresponds to a ray curvature $K = 1/k$ about 5·3 times less than the Earth's curvature, while an isothermal atmosphere would produce a ratio of only 4·3. On the other hand, a convective atmosphere, with a lapse rate near 10 K/km, would give a curvature ratio of 6. As the atmosphere is near convective equilibrium during the day, this explains the typical flattening of the setting Sun. Surveyors and geodesists usually assume[32] a still larger value, $K = 7$, because their observations are usually made on warm afternoons at moderate elevations above sea level, and the higher temperature and lower pressure than assumed above both decrease the curvature of the refracted ray. Lapse rates in the free atmosphere are limited by convection to the adiabatic lapse rate, though it can be exceeded within a few metres of a warm surface, which inhibits convection. But there is no limit in thermal inversions, where lapse rates exceeding a degree per metre are common. An inversion gradient of 20 K/m has been measured directly[33], corresponding to $K = 133$ and a radius of curvature of 48 km for a horizontal ray.

Historical remarks: This method of calculating the relation between lapse rate and radius of curvature of a ray is similar in spirit to that offered by Thomas Young[34], though he used the 'projectile hypothesis' for the propagation of light (*i.e.*, the emission model of Descartes and Newton).

The relation between the density gradient at the observer and the gradient of refraction at the astronomical horizon was first proved by J. B. Biot[35,36]. After mentioning the theorem (first proved by Oriani[37,38], and discussed in detail below) that the refraction out to zenith distances of 74° is approximately independent of the structure of the atmosphere, Biot[35] said, "But what has not been noticed is that there exists, ... the singularity of always being realised, in all possible constitutions of atmospheres, not just approximately, like that which we have just mentioned, but in an absolute and rigorous manner. ... Besides the unexpected singularity of finding an element of the horizontal refraction, independent of the state of distant layers, and of obtaining it, in all possible cases, without integration; besides the connection which results between the increase of refraction near the horizon and the equally observable variations of the refractive power starting from the bottom layer, the theorem which I am announcing has still other useful applications." As the magnification at the horizon is so obviously related to the refraction gradient, we may fairly attribute the magnification theorem to Biot, though he did not mention this particular "useful application" of his discovery.

The relation between ray pairs that are symmetrical about the astronomical horizon applies to all finite differences, as well as to the infinitesimal ones required to demonstrate Biot's magnification theorem. Because the atmosphere above the observer contributes equally to the refraction of the two rays, the difference in refraction at these two altitudes depends only on atmospheric structure between eye level and the height where the lower ray is horizontal. This explains why the inverse problem (of determining the temperature profile from the refraction profile) is well-posed below the astronomical horizon, even though it is ill-posed above it. The history of this problem, and the methods of solving it explicitly, have been discussed by Bruton and Kattawar[39,40].

Refraction

Approximations: The practical calculation of refraction always involves approximations. What level of accuracy is useful? Positional observations are rarely more precise than 0·1 arc second near the zenith, and the errors grow with about the square root of the airmass. So there is no practical use for calculations much better than a second of arc or so at the horizon, where the refraction is about 2000 arcsec; or a part in a thousand at moderate zenith distances, where the refraction is on the order of 100 arcsec. Generally, a part in a few thousand is the useful limit of accuracy for astronomical refraction calculations.

The sine law: We have been able to find the flattening of the setting Sun without calculus, and without actually calculating the refraction. It is even possible to calculate refraction for a simple atmospheric model without calculus, as Cassini[41] did, before calculus had been invented (see below for details). Calculating refraction requires the sine law discovered empirically about 1600 by Harriot[42,43], rediscovered by Snel some 20 years later, and finally published in 1637 by Descartes[44], who had read Snel's unpublished manuscript. Although Descartes, Newton, Laplace, and many others pretend to 'derive' the law of refraction from theoretical considerations, it is really an experimental fact that all theories of light must accommodate.

The law of refraction is simply that

$$\frac{\sin\theta_1}{\sin\theta_2} = n,$$

where the angles of incidence (θ_1) and refraction (θ_2) are measured from the normal to the surface separating any two media. The constant ratio n is the *relative* index of refraction of the media. It is conventional to call the index of any material relative to a vacuum the *absolute* index of the material. This makes the refractive index of a vacuum exactly unity.

The plane-parallel model: Newton[45] showed that the sine law may be extended to a series of plane-parallel layers, so that the product $n \sin z$ (where the local zenith distance z is the angle from the normal at any interface) is conserved throughout the stack of layers. As he put it, "the Sum of all the Refractions will be equal to the single Refraction which it would have suffer'd in passing immediately out of the first Medium into the last." That is, refraction in a plane-parallel atmosphere is the same as in a single homogeneous layer having the refractive index at the observer.

In this single-slab model (Fig. 6), the angle of refraction inside the atmosphere is identical to the observed zenith distance z_o, and the law of refraction is just

$$n \sin z_o = \sin z_t,$$

where z_t is the object's true (unrefracted) zenith distance; so

$$z_t = \arcsin(n \sin z_o).$$

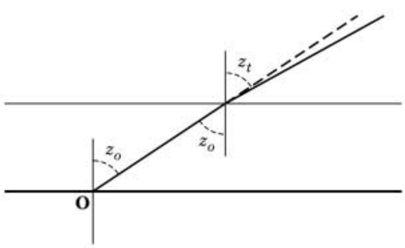

FIG. 6

Refraction in a plane-parallel slab. The observer is at **O**, on the surface of the (flat) Earth. Refraction occurs at the top of the slab of atmosphere. The dashed line is the extension of the observer's line of sight; the angle between it and the ray (solid) above the atmosphere is the astronomical refraction.

As the refraction $r = (z_t - z_o)$, it is exactly

$$r = \arcsin(n \sin z_o) - z_o, \qquad (1)$$

which is clearly a nonlinear function of both n and z_o — particularly near the horizon, where the angles are near 90°, and the sine and arcsine functions have their greatest curvature.

On the other hand, near the zenith, the angles are small, and the small-angle approximation $\sin x \approx x$ is useful. This linearization gives

$$z_t \approx n\, z_o,$$

so that

$$r = z_t - z_o \approx n\, z_o - z_o = (n - 1)\, z_o.$$

Some textbooks[6,8] carry the small-angle approximation one order further, expanding $\sin z_t = \sin(z_o + r) = \sin z_o \cos r + \cos z_o \sin r$. Then, since r is always small (so that its cosine can be set to unity), one obtains $r \approx \sin r \approx (n - 1) \tan z_o$. This still hides the actual nonlinear dependence on refractivity.

However, Delambre[5] shows that $\tan(r/2)$ can be developed in a power series in $\tan z_o$. In this series, the coefficients of the terms involve successive powers of $(n^2 - 1)$, which he uses for the refractivity instead of $(n - 1)$ — a minor modification. The pairing of higher powers of the refractivity with higher powers of $\tan z_o$ shows how the rule that the refraction is approximately proportional to the refractivity breaks down near the horizon. Furthermore, the Earth's curvature makes the coefficient of the asymptotic formula for refraction near the zenith slightly different from the $(n - 1)$ of this plane-parallel model, even when the tangent approximation is usable.

A peculiarity of the plane-parallel model is that rays with grazing incidence ($z_t = 90°$) at the top of the atmosphere are seen at an apparent zenith distance $z_o = \arcsin(1/n)$, which corresponds to an angular altitude of about 1° 23′. Rays

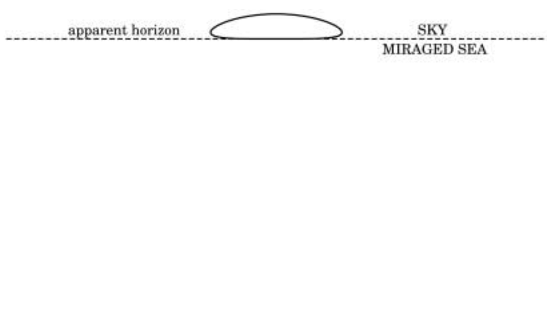

FIG. 7

The form of the setting Sun in the plane-parallel atmosphere. The 'horizon surface' (dashed) is nearly three solar diameters *above* the astronomical horizon; below it is a superior mirage of the Earth's surface.

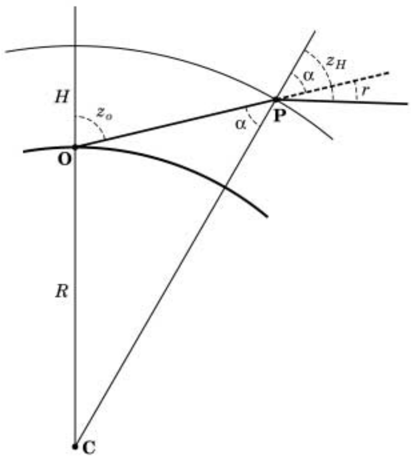

FIG. 8

Refraction at the top of a uniform spherical atmosphere. The refraction occurs at **P**, a height H above the observer at **O**. The dashed line is the extension of the refracted ray **OP**.

closer to the observer's horizon suffer total internal reflection at the upper surface of the slab atmosphere; in this zone of the sky, no astronomical objects are visible. Instead, a superior mirage of terrestrial objects would appear there, because the observer would be inside a duct. Such mirages were treated by Wegener[22] for a more realistic model, in which a smaller refractive-index jump produces the reflection, and the Earth's curvature is taken into account. Not only would the setting Sun disappear at the top of the duct, well above the astronomical horizon but, in addition, the vertical tangent of the arcsin function at an argument of unity causes infinite compression of the solar image at this elevated 'horizon surface' — see Fig. 7. (Such highly-flattened images are in fact seen just above ducts in the real atmosphere; see Fig. 8 of ref. 46 for an example.) The fact that the setting Sun is visible at the actual sea horizon, below the astronomical one, contradicts this model, and shows that the Earth's surface is convex, not flat.

Cassini's homogeneous model: It is slightly more realistic to bend the uniform slab to fit the curvature of the Earth (see Fig. 8). The ray **OP** inside the atmosphere of constant refractive index n is straight. In triangle **OPC**, the law of sines gives

$$\frac{\sin\alpha}{R} = \frac{\sin z_o}{R+H},$$

so

$$\sin\alpha = \frac{R\sin z_o}{R+H}.$$

The law of refraction, applied at **P**, is

$$\sin z_H = n \sin \alpha,$$

where z_H is the local zenith distance of the ray outside the homogeneous atmosphere. We now have expressions for $\sin \alpha$ and $\sin z_H$.

But $z_H = \alpha + r$, where r is the refraction; so

$$r = z_H - \alpha = \arcsin\left(\frac{nR \sin z_o}{R + H}\right) - \arcsin\left(\frac{R \sin z_o}{R + H}\right) \quad (2)$$

is the refraction for this model. Again, the nonlinear dependence on n is obvious. This result is exact, and is obtained with just trigonometry.

The homogeneous model was first worked out by G. D. Cassini[41], and so is often called 'Cassini's model', though it was also assumed by Kepler[23], and can be traced back to Ptolemy[47]. Some authors[4,7] call Eqn. (2), or some approximation to it, 'Cassini's formula'; but Cassini never published a formula, just a verbal description of how the model works, with a table derived from it.

This model is surprisingly accurate out to moderate zenith distances, if we choose values of n and H that reproduce the actual conditions (refractive index, pressure, and density) at the observer. Ivory[48] first noticed that "The simple hypothesis of Cassini seems hardly to have met from astronomers with the attention it deserves; for, if we use accurate elementary quantities in the computation, it will determine the refractions to the extent of 74° from the zenith with the same degree of exactness as any of the other methods, without even excepting the formula of Laplace." Radau[2] also recognized its accuracy. Its errors (compared to the Standard Atmosphere) are only[12] 51 milli-arcsec at 74° zenith distance, 17 mas at 70°, and still smaller higher in the sky. The error remains below a second of arc out to $z_o = 81°$; but beyond that, the model quickly becomes useless, having an error of 13 arc minutes at the horizon.

The reduced height, H: As was pointed out above, the atmosphere would have constant density if the lapse rate were about 35°/km. This homogeneous atmosphere comes to an abrupt end where $T \to 0$. Rather than use this rough temperature gradient to compute the height of the atmosphere, it is more instructive to invoke hydrostatic equilibrium. The pressure at the bottom of the atmosphere is the weight per unit area of the material, *i.e.*,

$$p_0 = \rho_0 g H,$$

where ρ_0 is the density at the surface and g is the acceleration of gravity; so

$$H = p_0 / \rho_0 g \, .$$

This expression for H is equivalent to the earlier version involving temperatures, if one assumes the ideal gas law. The "height of the homogeneous atmosphere" is a rather cumbersome phrase; Radau's term[2] "reduced height" is more concise. This height, which we first encountered in calculating ray curvatures, appears often in refraction theory, even in non-uniform atmospheres.

The refractive invariant

Geometric invariants: In the plane-parallel case, simple geometry relates the angle of refraction at one surface to the angle of incidence at the next interface:

these angles are equal. Within a homogeneous layer, the ray is straight, so the value of $\sin z$ remains constant; we may call $\sin z$ a *geometrical invariant* within such a layer. As the refractive index n is also constant, the product $(n \sin z)$ is conserved within each layer. But the law of refraction makes $(n \sin z)$ the same on both sides of each interface; so $(n \sin z)$ is conserved throughout the whole plane-parallel atmosphere. We may call it the *refractive invariant* for the plane-parallel model.

Refraction in a spherical atmosphere still involves a conserved quantity, but it is not $(n \sin z)$, because the curvature of the atmosphere makes z change along a straight ray within a layer of constant n. The angles at successive interfaces differ, even though the intervening layer is homogeneous. We need a geometric relation between these angles that takes account of the layer's curvature. So, what is the geometrical invariant for a straight ray in a curved layer? In Fig. 9, the length p of the perpendicular from the centre of the Earth **C** to the ray is $(R \sin z)$ at any point **P** on the ray, if z is the local zenith distance of the ray at **P**. Physicists like to call p the *impact parameter*; it is our required geometric invariant.

Refractive invariant: Now consider a single refraction at the top of a uniform layer with refractive index n. In Fig. 10, the ray is refracted at $\mathbf{P}_1$, where the local zenith distance on the vacuum side is z_1. The law of refraction tells us that z_2, the angle of refraction inside the atmosphere, is given by

$$n \sin z_2 = \sin z_1.$$

If we multiply this equation by R_1, the radius of the refracting surface, we have

$$nR_1 \sin z_2 = R_1 \sin z_1;$$

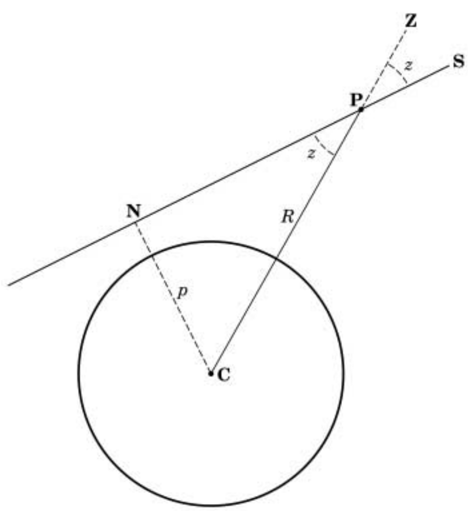

FIG. 9

The geometrical invariant is $p = R \sin z$ along an unrefracted ray, **SPN**. The local zenith distance is z at any point **P**, a distance R from the centre of the Earth, **C**.

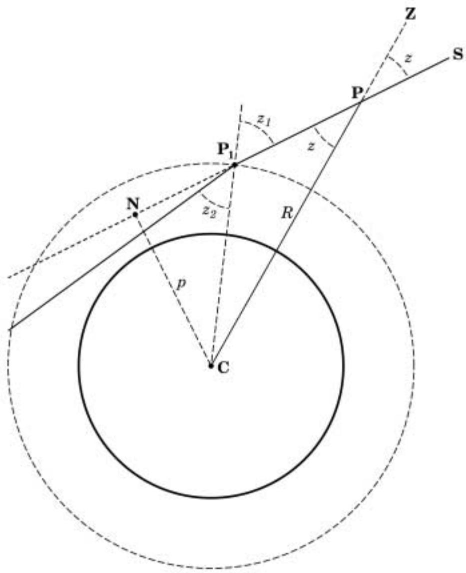

FIG. 10

The refractive invariant is $nR \sin z$ along a refracted ray, $\mathbf{SPP_1N}$, in a uniform atmosphere. The local zenith distance at point **P** is z. Outside the dashed surface, the refractive index is unity; inside, it is n. The ray is refracted at $\mathbf{P}_1$, where the angle of incidence is z_1, and the angle of refraction is z_2.

but $(R_1 \sin z_1)$ is just p, the geometric invariant for the incident ray. So we have

$$nR_1 \sin z_2 = p.$$

And of course a geometric invariant applies to the refracted part of the ray, as well: the product $(R \sin z)$ remains constant along the refracted ray inside the atmosphere. But then so does $(nR \sin z)$, if n is constant; and if $(nR \sin z) = p$ at $\mathbf{P}_1$, it must remain equal to p inside the refracting atmosphere. Thus $(nR \sin z) = p$ all along the ray, both inside and outside the atmosphere. This is the refractive invariant for the spherical model.

If we add a second refracting surface inside the first, the same argument can be repeated, just as it was for the plane-parallel atmosphere by Newton (see Smart[8] for the details). The geometric invariant $(R \sin z)$ in each spherical layer takes the place of the geometric invariant $(\sin z)$ for each plane-parallel layer, but the rest of the argument remains the same as before: the law of refraction allows us to propagate the refractive invariant from layer to layer, and to show that it remains conserved throughout the whole atmosphere, no matter how many layers there are. So the product $(nR \sin z)$ remains constant all along the ray, even in the limit of an infinite number of refractions; $(nR \sin z) = p$ is the refractive invariant. It involves only geometry and the law of refraction.

Applications: If we know the refractive invariant for a ray, we can calculate its local zenith distance z at any distance R from the centre of the Earth, provided we have an atmospheric model that supplies the refractive index n as a function of R. For, if $nR \sin z = p$, we can solve for $\sin z = p/(nR)$, if p, n, and R are all known. A particularly convenient use of the refractive invariant is to find the radius R_{hor} where a ray is horizontal. At that point, $z = 90°$, $\sin z = 1$, so we have $nR_{hor} = p$. This implicit equation for R_{hor} is easily solved numerically by assuming that the product nR is a locally linear function of R.

Conversely, if we know where a ray is horizontal, we immediately have its refractive invariant, and can calculate its slope at every point in the atmosphere. For example, the horizon ray must be horizontal where it touches a level surface, such as the sea; this allows calculation of the (refracted) dip of the sea horizon seen from any higher elevation. If the values of n and R at the observer are n_o and R_o, and the refractive invariant for the ray is the nR product at the sea surface, we must have $n_o R_o \sin z_o = (nR)_{surface}$, from which $\sin z_o$ and hence z_o, the zenith distance of the horizon at the observer, can be found. Of course, z_o is just 90° plus the dip of the horizon; so $\sin z_o = \cos d$, where d is the dip. The refractive invariant has the important consequence that images of astronomical objects are necessarily single and erect above the astronomical horizon. Conversely, the inverted and multiple images of mirages are possible only below the horizon. This theorem was first found by Biot[49], though it has been repeatedly forgotten and rediscovered; Meyer[50] gives a simple proof by contradiction.

Suppose two different rays from an object point **P** could arrive at the observer's eye at **O** from above the horizon. As the observer would see the same object in two different directions, the rays have different zenith distances at **O**; so they have different refractive invariants (as nR is the same for both rays at the eye). However, if both rays connect **O** and **P**, the one with bigger slope at the eye must have a smaller slope somewhere else, as the mean slopes of the two rays must be equal. But nR remains equal for both rays at *every* level in the atmosphere; so the ray with the greater slope at the eye has the greater slope everywhere. As it can never have the smaller slope required to reach **P**, there cannot be two rays above the horizon.

We can avoid the contradiction if one of the rays passes through a range of heights that the other does not — either above the height of **P**, or below the eye at **O**. In either case, the ray must become horizontal at some limiting height, where nR equals the ray's refractive invariant. This can only happen for astronomical objects if that range of heights is below the observer; then the ray must be horizontal somewhere below eye level. The symmetry of rays about their perigee points means that this ray arrives at the observer from below the astronomical horizon: it belongs to the inferior mirage. (Terrestrial objects can have the second ray horizontal above the observer; this ray is ducted, and belongs to a superior mirage.) A similar argument shows that the single image above the astronomical horizon is erect. For, if the image of an object is to appear inverted, rays from the top and bottom of the object must cross, somewhere between object and observer, to reach the eye in the inverted order. Then the crossing point takes the place of **P** in the above argument; as such a point cannot exist, the rays cannot cross, and the image must be erect, if it is above the horizon. But of course the intersection can occur if one of the rays arrives from below the astronomical horizon; and in fact mirages of astronomical objects do occur there. Evidently the refractive invariant gives a special significance to the product nR. If we plot nR as a function of height for any atmospheric model, we can determine the local zenith distance of any ray whose refractive invariant is known. Clearly, a ray cannot penetrate into regions where nR exceeds its refractive invariant, for that would require $\sin z > 1$.

Dip diagram: Plotting nR as a function of R or height in the atmosphere produces a useful diagram, whose properties were discussed in an earlier paper[51]. It allows a simple graphical determination of z along a ray. Because $(nR)_{horizon}/(nR)_{observer} = \sin z_{horizon}$ is the cosine of the dip, this is sometimes called a *dip diagram*. Any ray can be represented as a horizontal line at $nR = p$ in the dip diagram; it intersects the sloping curve that represents the atmospheric model at the point where the ray is horizontal, so that $\sin z = 1$. The ray is confined to heights where $nR > p$, so that $\sin z < 1$; that is, the ray cannot cross the model curve. Ordinarily, n decreases so slowly with increasing R that the product nR increases monotonically. However, as was mentioned above, the condition for a ray that follows the Earth's surface is $(nR) = const.$, which means the curve representing the atmospheric model is locally horizontal in the dip diagram. This occurs if the dip diagram has a local maximum or minimum.

Ducting: Because a ray must have $p < nR$, a local minimum in the dip diagram creates a region at smaller heights where rays with $p > (nR)_{min}$ can be trapped (see Fig. 11). This is a duct. For observers within the duct, celestial objects are blocked by a zone of sky centred on the astronomical horizon. The angular half-width Δz of the forbidden zone of sky is given by $\cos(\Delta z) = (nR)_{min}/(nR)_{observer}$. The symmetry of this 'blank strip'[22] about the astronomical horizon is due to the equality of the angles b in Fig. 3; see Fig. 8 of ref. 46 for photographs of an example. This symmetry can also be regarded as a consequence of the symmetry of the sine function about 90°: equal angles above and below the astronomical horizon have the same value of $\sin z$.

In the schematic dip diagram of Fig. 11, the heavy curve **ABCDE** shows the run of nR as a function of height in the atmosphere. The duct extends from the point **A** at height h_1 to **E** at h_2, where the horizontal line marked p_{min} is tangent to the local minimum in nR. All horizontal rays between these two heights are trapped in the duct, because $p = nR \sin z$ for a ray must be less than nR in the

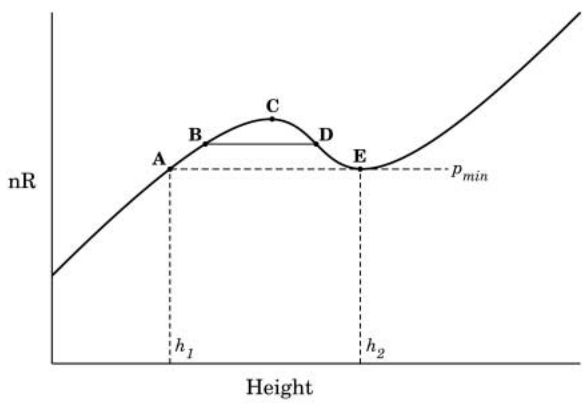

FIG. 11

A schematic dip diagram for a duct. The model atmosphere is represented by the curved line **ABCDE**; the duct extends from h_1 to h_2 in height.

atmosphere at every height. The dashed line **AE** has the minimum refractive invariant, p_{min}, that can be ducted. A ray such as **BD** within the duct oscillates between the heights of **B** and **D**; at those heights, the ray is horizontal, because ($p_{\rm ray} = p_{model}$) implies $\sin z = 1$.

The ray **C** is horizontal at the height where the duct has maximum angular width, *i.e.*, at the height h_{max} where nR has a local maximum. If the value of nR at **C** is p_{max}, the angular halfwidth of the blank strip as seen by an observer at h_{max} is $\arccos(p_{min}/p_{max})$; this corresponds to the ray **AE**. When the ray **BD** passes through this height, its angular slope is $\arccos(p_{BD}/p_{max})$. For more detailed discussion of dip diagrams, see ref. 51.

The refraction integral

The differential of refraction: Obviously, the refractive invariant contains enough information to calculate the slope of a ray at every height in an atmosphere, if the run of nR with height is known: the refraction is just the total change in slope of the ray. Knowing the slope at every point, we can write down the differential equation for the refraction. The only problem is that the dip diagram (or its equivalent, the model atmosphere) gives the slope of the ray with respect to the *local* zenith, whose direction changes along the ray; see Fig. 12, which shows the differential triangle at a distance R from the centre of the Earth. The zenith distance of the ray at R is z, and at $R + dR$, it is $z + dz$. The differential of refraction is

$$dr = dz + d\theta,$$

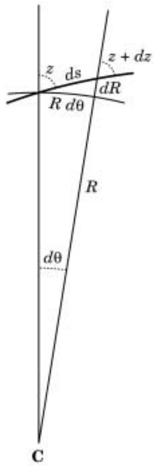

FIG. 12

The differential triangle for refraction. The heavy curved line represents the refracted ray, with zenith distance z at a distance R from the Earth's centre **C**, and zenith distance $z + dz$ at $R + dR$. The element of path length ds subtends an angle $d\theta$ as seen from **C**; $d\theta$ is the change in direction of the local zenith in the interval ds.

where $d\theta$ is the differential change in the direction of the local zeniths, *i.e.*, the angle at the centre of the Earth subtended by the element of path length ds. (If the ray follows the curve of the Earth, so that z remains constant, dr is just $d\theta$.)

The tangent of the ray's altitude is the cotangent of its zenith distance, so the little differential triangle gives

$$\frac{dR}{R\,d\theta} = \cot z.$$

We will shortly need dR/R, so we rearrange this equation:

$$\frac{dR}{R} = \frac{d\theta}{\tan z}.$$

Now use the fact that the refractive invariant ($nR \sin z$) is constant, so that its logarithmic derivative is zero:

$$\frac{dn}{n} + \frac{dR}{R} + \frac{d(\sin z)}{\sin z} = 0,$$

or

$$\frac{dn}{n} + \frac{dR}{R} + \frac{\cos z}{\sin z}\,dz = \frac{dn}{n} + \frac{dR}{R} + \frac{dz}{\tan z} = 0.$$

Then combine the value of $dR/R = d\theta/\tan z$ with the fact that $d\theta = dr - dz$, so that $dR/R = (dr - dz)/\tan z$, and substitute this into the last equation:

$$\frac{dn}{n} + \frac{dr - dz}{\tan z} + \frac{dz}{\tan z} = 0.$$

Finally, cancel the two dz terms, and solve for dr:

$$dr = -\tan z\,\frac{dn}{n}.$$

Physically, the minus sign occurs because n decreases as R increases. The factor $\tan z$ shows how sensitive refraction is to the *local* zenith distance along the ray: where the ray is horizontal, $\tan z$ becomes infinite. Although this infinity requires transforming dr to handle horizontal rays, the present form with $\tan z$ is the most informative expression for the refraction differential.

Integrating the refraction: If we leave the differential of refraction in the form just derived, the whole refraction is just

$$r = \int_{n_o}^{1} dr = -\int_{n_o}^{1} \tan z\,\frac{dn}{n} = \int_{1}^{n_o} \tan z\,\frac{dn}{n},$$

where n_o is the value of the refractive index at the observer, and 1 is its value above the atmosphere.

The independent variable here is n, the refractive index. Note that n varies only from 1·0000 in space to not quite 1·0003 at sea level. Because the refractivity $(n - 1)$ is very nearly proportional to the density ρ of the air, $dn \propto d\rho$. So n is a linear function of the density. It is instructive to plot the refraction integrand as a function of n, bearing in mind the linear relation between n and density. When we do so, we find that the refraction integrand (and hence the refraction itself) behaves very differently in different parts of the sky.

Small zenith distances: Fig. 13 shows the refraction integrand for the Standard Atmosphere[31], for a few zenith distances up to 60 degrees. In this range, the refraction is less than 2 minutes of arc, so the ray is nearly straight. As 2 minutes is 1/1800 of 60°, a straight-line approximation to the ray should nearly meet the required degree of accuracy out to this zenith distance. If we neglect ray curvature entirely, the local zenith distance of the ray at any level is independent of the atmospheric structure. In this approximation, $\tan z$ can be computed from the geometric invariant $p_g = R \sin z$. Then at height h, $(R_E + h) \sin z_h = R_E \sin z_o$ (assuming the observer at the Earth's surface); so

$$\sin z_h = \frac{R_E}{R_E + h} \sin z_o.$$

But for small z, $\sin z \approx \tan z \approx z$; so we can also write

$$\tan z_h \approx \frac{R_E}{R_E + h} \tan z_o.$$

Furthermore, the atmosphere is so shallow that $h \ll R_E$ everywhere: there is practically no refraction above 100 km height, so $h/R_E \leq 1/64$. This means that $\tan z$, which provides most the variation of the refraction integrand, hardly varies by 1% across Fig. 13. Because the integrand is so nearly constant, we can replace both n and $\tan z$ with their average values, leaving only dn inside

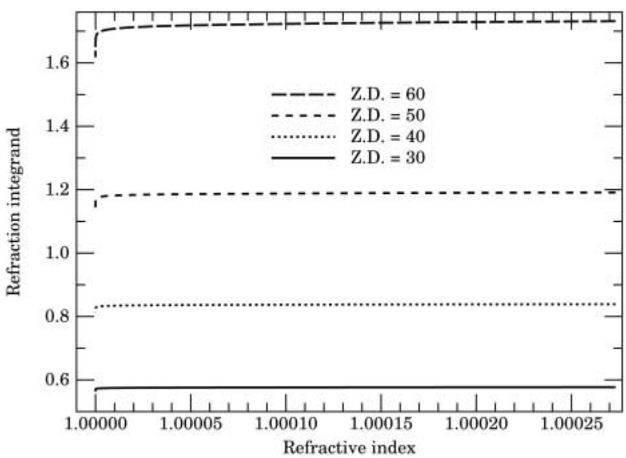

FIG. 13

The refraction integrand for the Standard Atmosphere, for zenith distances at the observer from 30 to 60 degrees. The Earth's surface (at the right edge) corresponds to $n = 1 \cdot 00027$, and the top of the atmosphere (left edge) to $n = 1 \cdot 00000$.

the integral. The refraction becomes simply

$$r = \frac{<\tan z>}{<n>} \int_1^{n_o} dn = (n_o - 1) \frac{<\tan z>}{<n>}.$$

The mean $<n>$, which is $(n_o + 1)/2 \approx 1{\cdot}00014$, obviously corresponds to the atmospheric level where the density is half the surface density, about 6·7 km above sea level. If the integrand were a straight line, the mean $<\tan z>$ would correspond to this same level. But Fig. 13 shows that the integrand is slightly concave downward, because of the nonlinear dependence of h, and hence z, on density; so the mean $<\tan z>$ is smaller, and corresponds to a higher level. This effective height turns out to be the reduced height $H \approx 8$ km. At that height, the mean value $<\tan z> = \tan z_H$ is

$$\tan z_H = \frac{R_E}{R_E + H} \tan z_o.$$

So the refraction at small zenith distance is well represented by

$$r = \left(\frac{R_E}{R_E + H}\right)\left(\frac{2}{n_o + 1}\right)(n_o - 1) \tan z_o,$$

or just

$$r = \left(\frac{R_E}{R_E + H}\right)(n_o - 1) \tan z_o,$$

if we neglect an error of 1 part in 7000 and set $<n> = (n_o + 1)/2$ to unity. This approximation represents the refraction of the Standard Atmosphere within 0·1 arcsec to 48·8° zenith distance, and within 1 second to nearly 68°. The coefficient $(n_o - 1)\ [R_E/(R_E + H)]$ is often called the *refraction coefficient* or the *refraction constant*. Notice that it differs by the factor $R_E/(R_E + H)$ from the coefficient $(n_o - 1)$ in the tangent approximation for the plane-parallel atmosphere. This curvature correction factor is less than unity by about $H/R_E \approx 1/800 = 0{\cdot}00125$, which is larger than the acceptable relative error; so it must be taken into account.

Physically, the curvature of the Earth decreases refraction (compared to the flat case) because the change in direction of the local vertical along the ray reduces angles of incidence, and hence local values of $\tan z$, in the upper atmosphere. The curvature correction factor in $<\tan z>$ represents the average effect of the tilted verticals along the ray relative to the observer's zenith. The greater the reduced height, the smaller is the refraction for a fixed n_o. For example, if we raise the temperature of the atmosphere, we must increase the surface pressure to keep ρ_o and hence n_o fixed. This requires a larger mass of gas above the observer. At every $R > R_E$, there is now a greater density than before, so n is (slightly) higher. This makes $\sin z$ and hence $\tan z$ smaller; so the left side of the integrand (corresponding to the upper atmosphere) moves downward in the plot, decreasing the area under the curve (*i.e.*, the refraction). The warmer atmosphere has a bigger reduced height, H, and its curvature correction factor $R_E/(R_E + H)$ is correspondingly less. On the other hand, if we keep both the temperature and pressure at the observer fixed, so that the refractivity at the observer stays fixed, changes above the observer are constrained by hydrostatic equilibrium. We can move gas up and down by

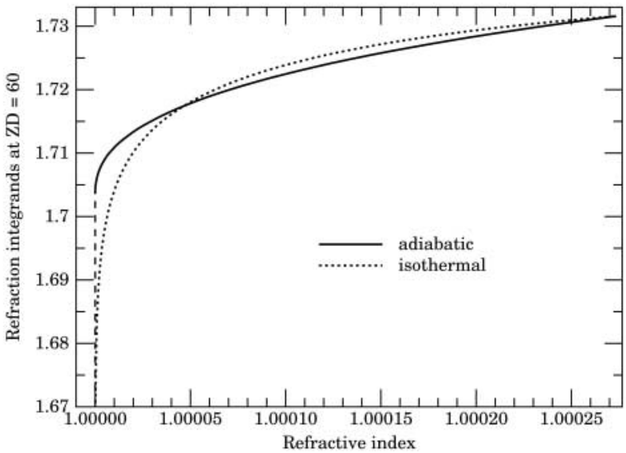

FIG. 14

The refraction integrand for adiabatic and isothermal atmospheres, at 60° zenith distance. The top curve in Fig. 13 would fall between these; note the greatly expanded vertical scale here. The dashed vertical line at $n = 1 \cdot 0000$ marks the lower limit of the refraction integration.

altering the temperature profile, but the total mass of gas in the column, and the reduced height, H, remain the same. Then changes in the integrand in one region are nearly balanced by opposite changes in another region, and the refraction is almost unaffected.

Fig. 14 compares the integrands for isothermal and adiabatic atmospheres with the same conditions at the observer. Both atmospheres have the same temperature and pressure at the bottom; the adiabatic model has a lapse rate of 10 K/km. The bottom part of the adiabatic model, at the right and centre of the figure, is cooler and denser than the isothermal model at the same height; so nR is larger, and consequently $\sin z$ and $\tan z$ are smaller in this region for the adiabatic model. As $\tan z$ is the dominant variable in the integrand, the adiabatic integrand lies *below* the isothermal one in this part of the diagram. But, because the upper parts of the adiabatic model are much colder than the isothermal one, the layers of lowest density are much closer to the surface of the Earth: the adiabatic model terminates below 30 km. In these low-density layers, at the left side of Fig. 14, n is very nearly unity, and R dominates the nR product. So in this region, $\sin z$ and $\tan z$ are larger for the adiabatic model than for the isothermal one. The two curves cross where the density is about ⅙ of the surface density ($n \approx 1 \cdot 00004$). The areas between the two curves are almost exactly equal on either side of this crossing. That is, the areas under the two curves — *i.e.*, the refractions in the two models — are almost exactly equal. If the average ordinates of the two parts were the same, the cancellation produced by the crossover would be exact. Near the zenith, the integrands are in fact nearly flat, and this cancellation is nearly perfect: all models give almost exactly the same refraction, regardless of the temperature profile. Even at 60° zenith distance, the average ordinates on the two sides of the crossing differ by only about 1% (note the expanded scale of Fig. 14). So the imbalance is very slight here: the refraction is about 98 seconds, but the two models differ by less than 0·002 arcsec — about 2 parts in 10^5.

Moderate zenith distances: However, at larger zenith distances, the disparity between the upper and lower parts of the atmosphere rapidly increases, because of the growing change of $\tan z$ along the ray within the atmosphere. At $z_o = 85°$, the curves for the adiabatic and isothermal models cross at $\tan z \approx 9$, and the average ordinates for the left and right portions are about 8 and 10 — a difference about 20 times larger than at 60°. The mean refraction is 9·6 arc minutes; the difference between the models has risen to nearly 4 arcsec, about 7 parts in 10^3, which is quite significant. Fig. 13 shows that at large zenith distances, the decrease of $\tan z$ in the upper atmosphere gives appreciable slope to the integrand, especially at the upper left corner of the curve, which is concave downward. The unequal weighting of upper and lower regions when this slope is appreciable makes the refraction sensitive to atmospheric structure: a density gradient in the lower atmosphere, where $\tan z$ is larger, contributes more to the refraction than the same gradient higher in the atmosphere. However, this sensitivity is constrained by the refractive invariant. A change in atmospheric density at a given R makes a proportional change in the refractivity $(n - 1)$; but that is, at most, only a part in 3000 of n itself, which appears in the invariant $nR \sin z$. Thus, changes in refractivity at a given R produce changes in $\sin z$ that are thousands of times smaller. As $\tan z$ is less than 10 times larger than $\sin z$ at 84° zenith distance, the changes in $\tan z$ also remain small until $\tan z / \sin z = \sec z$ appreciably exceeds 10.

Series expansions: In this region, where the sensitivity to atmospheric structure is small, it is tempting to extend the $\tan z$ approximation, which works well near the zenith, to a power series in $\tan z$. Delambre[5] gives numerous examples of such developments, based on trigonometric identities. The more usual approach[1,2,6,7,8] is that introduced by Lambert[52]: first, replace the $\tan z$ in the integrand by $\sin z / \sqrt{1 - \sin^2 z}$. Then, replace $\sin z$ by its equivalent p/nR by using the refractive invariant $p = (n_o R_o \sin z_o)$, so that the refraction integral $r = \int_1^{n_o} \tan z \, \frac{dn}{n}$ becomes

$$r = \int_1^{n_o} \frac{p}{\sqrt{(nR)^2 - p^2}} \, \frac{dn}{n} \, ,$$

which of course looks much more intimidating if we write out p in terms of n_o, R_o, and $\sin z_o$, as is customary:

$$r = \int_1^{n_o} \frac{n_o \, R_o \sin z_o \, dn}{n \sqrt{(nR)^2 - (n_o \, R_o \sin z_o)^2}} \, .$$

The final step is to expand the square root in the denominator by using the binomial theorem, and introduce some closed-form relation between n and R to express the series terms as functions of a single variable. The expansion produces very complex expressions even for simple atmospheric models. The series, involving powers of a 'small quantity' such as $[(H/R) \tan^2 z]$, can be integrated termwise, but even then the individual terms involve integrals that must themselves often be approximated by series expansions. This process allows the numerical calculation of refraction tables out to zenith distances of about 82°, but at the expense of mathematical abstractions that obliterate all traces of the physics. Some people have carried it to ridiculous extremes:

Bauernfeind[53] extended the series to the 28th power of sec z. Worse yet, the series expansions are only semi-convergent, as was first pointed out by Ivory[54]. Unfortunately, the concept of semi-convergence had only been introduced a dozen years earlier by Legendre[55], and Ivory's warning escaped the notice of most astronomers.

Strictly speaking, these Lambert series diverge at *every* zenith distance — not just at the horizon, where tan z blows up. The real difficulty lies in the coefficients of the terms, which increase like factorials with the order of the term. So, no matter how small tan z may be, the higher terms eventually increase without limit: the series diverges. Because only odd powers of the small argument appear in the series, the unwary often suppose that convergence is rapid. This odd-power property is sufficiently un-obvious that Bradley laboriously established empirically[56] that the coefficient of $\tan^2 z$ is negligible. But it is simply a result of symmetry: if the refraction is regarded as a continuous function through the zenith, with zenith distances counted positive on one side and negative on the other, it is obvious that the refraction must also change sign on passing through the zenith, where it is zero. Therefore the refraction is an odd function, and can involve only odd powers of tan z, which itself is odd. What makes the series useful numerically is its alternating signs (due to the binomial expansion of a negative power — the square root is in the denominator), which allow truncation after a few terms, so long as tan z is not too large. Then the partial sums are accurate enough for practical work, which only requires three or four significant figures. But, as successive terms decrease more and more slowly, many are required for high accuracy, even at moderate zenith distances. (For example, two terms approximate the refraction of a realistic atmosphere considerably less accurately[12] than does the Cassini model, at *all* zenith distances.) And expansions fail entirely around a zenith distance of 82°, where the smallest term in the series becomes unacceptably large.

Oriani's theorem

Evidently, this is not a very instructive approach to the problem. However, it does produce one remarkable and well-known result, which can be neatly demonstrated[7] by setting $R/R_o = 1 + s$, so that s is just height measured in Earth radii. Note that s is always small: it is 0·01 at 64 km height, and we can neglect the refraction above $s = 0{\cdot}02$. So, set $R = (1 + s)\, R_o$ in the refraction integral, and keep only the first-order terms in s, so that $(nR)^2 \approx (1 + 2s)\,(nR_o)^2$. Then, cancelling R_o factors in numerator and denominator, we have

$$r = \int_1^{n_o} \frac{n_o \sin z_o \, dn}{n\sqrt{n^2 + 2n^2 s - (n_o \sin z_o)^2}}\,.$$

The argument of the square root can be rewritten as

$$(n^2 - n_0^2 \sin^2 z_o) + 2n^2 s = (n^2 - n_0^2 \sin^2 z_o)\left(1 + \frac{2n^2 s}{n^2 - n_0^2 \sin^2 z_o}\right);$$

then the refraction integral becomes

$$r = \int_1^{n_o} \frac{n_o \sin z_o \, dn}{n\sqrt{n^2 - n_0^2 \sin^2 z_o}}\left(1 + \frac{2n^2 s}{n^2 - n_0^2 \sin^2 z_o}\right)^{-1/2}.$$

Now, expand the expression in large parentheses on the right, using the binomial theorem; keep only the first-order term in s, and integrate the resulting terms separately:

$$r = \int_1^{n_o} \frac{n_o \sin z_o \, dn}{n \sqrt{n^2 - n_0^2 \sin^2 z_o}} - \int_1^{n_o} \frac{s n n_o \sin z_o \, dn}{(n^2 - n_0^2 \sin^2 z_o)^{3/2}} .$$

The first term is an elementary integral, whose value,

$$\arcsin(n_o \sin z_o) - z_o ,$$

is exactly the refraction (Eqn. 1) for the plane-parallel atmosphere. Thus the second integral can be regarded as the first-order correction for atmospheric curvature. This correction term can be evaluated by setting both n and n_o to unity in its integrand. (The error made is of higher order, as this term is already of order s.) Then the correction term becomes

$$-\frac{\sin z_o}{\cos^3 z_o} \int_1^{n_o} s \, dn .$$

Next, we use the Gladstone-Dale rule that the refractivity $(n - 1)$ is proportional to the density, ρ. But if $(n - 1) = c\rho$, then $dn = c \, d\rho$. This converts the correction term to

$$-c \frac{\sin z_o}{\cos^3 z_o} \int_0^{\rho_o} s \, d\rho .$$

Finally, integration by parts gives

$$-c \frac{\sin z_o}{\cos^3 z_o} \int_0^{s_{max}} \rho \, ds ,$$

where s_{max} is the largest normalized height that contributes appreciably to the refraction — essentially, the top of the atmosphere. But this integral of density through the whole atmosphere is just the mass of a unit column; so this last integral is proportional to the surface pressure at the observer, or to the ratio H/R_o. Furthermore, the factor $\sin z_o / \cos^3 z_o = \tan z_o \sec^2 z_o$; and if we replace $\sec^2$ with $(1 + \tan^2)$, this factor is just $(\tan z_o + \tan^3 z_o)$. The plane-parallel term can also be expressed as a sum of tangent and tangent-cubed terms, if we expand its arcsine in a Taylor series and neglect higher powers of the refractivity. So the sum of the two terms is of the familiar form

$$r = A \tan z_o - B \tan^3 z_o ,$$

where the coefficients A and B involve only conditions at the observer and are independent of the density distribution. This result was first proved by Oriani[37,38], who stated that "This expression depends on no hypothesis about either the law of heat in the atmosphere or about the density of the air at various distances from the surface of the Earth". Laplace[1] provided a more rigorous and complete proof of Oriani's theorem.

One important practical consequence of Oriani's theorem is that all possible atmospheres in hydrostatic equilibrium produce the same astronomical refraction, up to the zenith distance where the $\tan^5 z_o$ term becomes appreciable. (For example, it explains the near-perfect compensation shown in Fig. 14.) Therefore it is convenient to derive the exact expressions for the coefficients A and B from (for example) Cassini's model — as Ball[7] does. This limiting zenith distance is typically in the range 70° to 74°, depending on the size of the $\tan^5 z_o$ term and the required accuracy. Oriani's theorem explained why all previous refraction calculations had given very similar results out to about 74° — a fact that had puzzled many earlier workers.

Of course, the $\tan^5 z_o$ term does depend on atmospheric structure, so different models diverge rapidly beyond this limit. Because $\tan z$ is inversely proportional to altitude a at large zenith distances, the initial differences are approximately inversely proportional to a^5. So the divergence increases by a factor of 3 between 70° and 74°, and is still faster beyond that. In fact, one sees from the derivation of the series expansion that successive terms involve integrals of consecutive powers of s with respect to n; but because dn is proportional to $d\rho$, these integrals are all of the form

$$\int s^j \, d\rho, \quad j = 1,2,3,\ldots,$$

i.e., they are successive height-moments of the density distribution.

In an exponential atmosphere, $d\rho \propto \exp(-s)\, ds$, and these moments become essentially

$$\int_0^\infty x^j e^{-x} \, dx = j!$$

(Indeed, it was while studying the refraction integral for an exponential atmosphere that Kramp[57] developed the theory of factorials and introduced the function we know today as the Γ function.) Hydrostatic equilibrium forces the real atmosphere to be nearly exponential, so the coefficients of Lambert's series-expansion terms increase nearly factorially. This is why the series diverges for all zenith distances. As the $\tan^5 z_o$ term depends on the second moment of the density distribution, it is similar for all realistic model atmospheres. This term depends mainly on the average lapse rate in the troposphere, so the differences in refraction between 70° and 80° for different models depend almost entirely on this mean lapse rate. The similarity of the second moments of all realistic models means that their refractions differ only a little out to 80° zenith distance, somewhat beyond the range where Oriani's theorem guarantees complete independence from atmospheric structure.

Refraction near the horizon

Beyond Oriani: The independence of refraction from atmospheric structure, in the region where Oriani's theorem applies, is due to the negligible variation in $\tan z$ along the ray. At moderate altitudes, $\tan z$ is nearly constant (*cf.* Fig. 13), so the refraction depends on a nearly equally-weighted average density or temperature gradient through the whole atmosphere. But near the horizon, $\tan z$ is large and nearly equal to $\sec z = 1/\cos z = 1/\sin a \approx 1/a$. So the $\tan z$ weighting along the ray changes significantly if the local altitude a changes along the ray.

The transition from small to large variation of tan z along the ray can be found from the refractive invariant.

How near the horizon is 'near'? The refractive invariant is $(nR)\sin z$, so fractional changes in $\sin z$ correspond to similar fractional changes in (nR). Now, $\sin z = \cos a \approx 1 - a^2/2$, for small a; so the upper and lower parts of the atmosphere are equally weighted if their fractional difference in (nR) is comparable to $a^2/2$. Because the refractivity is less than 3×10^{-4}, changes in (nR) are mostly due to changes in R. Half the mass of the atmosphere is above the level where the pressure is half the surface pressure, near 6·7 km height. This corresponds to a fractional change in R of about 1 part in 10^3; but the fractional change in n there is only $1{\cdot}5 \times 10^{-4}$, nearly an order of magnitude smaller. So the lower atmosphere becomes disproportionately important when $a^2/2 \approx 10^{-3}$, corresponding to $a \approx 0{\cdot}05$ radians or 2·6°. This agrees with the altitude where nocturnal inversions are found to become important[12]: numerical integrations show large differences in refraction among different models only below 2 or 3 degrees altitude.

Fig. 15 shows how tan z blows up in the lowest layers near the horizon. The top two curves, for altitudes of 1° and 2°, have altitudes below the critical value just calculated; and it is just these that show a nonlinear increase in tan z in the lowest layers (right-hand side). In this zone of sky, tan z is much larger near the observer than in the upper half of the atmosphere. In fact, the refraction integrands all have nearly the same values at the left side of the figure; they differ by only a factor of 2 at the tropopause. The upper atmosphere contributes a nearly fixed amount to the refraction at all altitudes near the horizon. This behaviour follows from the refractive invariant: $\sin z > 0{\cdot}995$ at the observer for all these curves, so the local zenith distance depends more on R than on z_o above the height where $R_o/R > 0{\cdot}995$ (about 32 km). This is another example of Wegener's Principle: the horizon ray meets the tropopause (or any surface

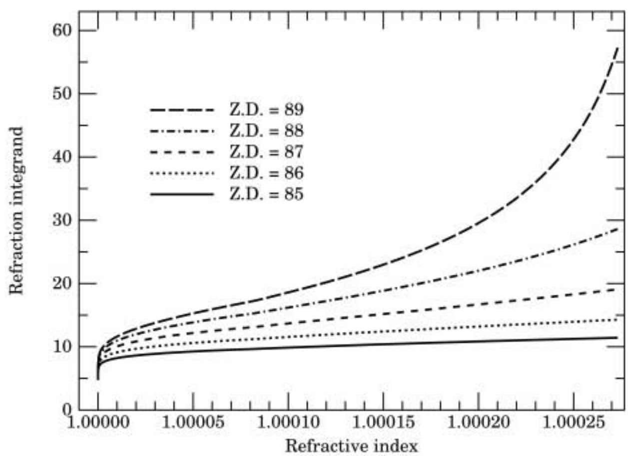

FIG. 15

The refraction integrand for the Standard Atmosphere, for zenith distances at the observer from 85 to 89 degrees. (*cf.* Fig. 13.)

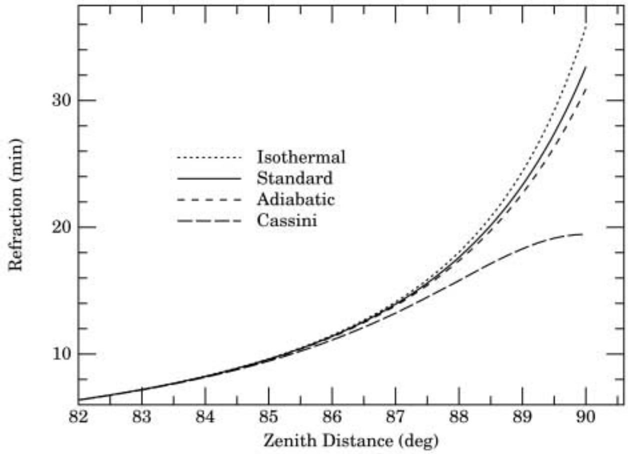

Fig. 16

The refraction for the Standard Atmosphere and three other polytropic models compared. All models have the same pressure and temperature at the observer; only their lapse rates differ.

overhead) at a local maximum value, so nearby rays also meet that surface at almost the same angle, and have similar refractions at greater heights. So the heavily-weighted lowest layers dominate the refraction at low altitudes. And the lapse rate in the boundary layer becomes more and more important in the total refraction (not just its derivative, as Biot's theorem tells us) as we approach the horizon[12]. Fig. 16 compares low-altitude refractions for the Standard Atmosphere and a few other canonical models with constant lapse rates. Because horizontal-ray curvature depends on the lapse rate, Biot's theorem gives the slope of each curve at the horizon; the zero slope of Cassini's model there can also be regarded as an example of Wegener's Principle. Oriani's theorem forces the curves to converge at the left side of Fig. 16. These curves, which meet only at the zenith, form a one-parameter family that illustrates many of the principles discussed above.

Refraction below the horizon: Between the astronomical horizon and the sea horizon is a zone in which rays are horizontal somewhere below eye level. This zone includes the whole disc of the setting Sun for observers above 220 m in the Standard Atmosphere, and even for lower observers with thermal inversions. Below the astronomical horizon, the refraction is dominated by the perigee layer, where the ray is horizontal. In this region, we invoke Wegener's Principle to separate the refraction contributions of the layers above and below eye level. To avoid problems with the infinite value of $\tan z$ at the perigee point, it is easier to work with ray bending in this lower region than with the refraction integral in the whole atmosphere; Wegener's principle shows that the upper part contributes only a small variation with altitude. If the ray were straight (*cf.* Fig. 4a), the thickness t of the layer between the heights of the observer and the perigee would be just $\mathbf{OC} - \mathbf{PC}$ or $R\,(1 - \cos a) \approx Ra^2/2$. If the lapse rate in this layer is constant, the ray is nearly a circular arc; then it can be shown[22, 51] that the same result applies, if we use an effective curvature $1/R_{eff}$ corresponding to the *difference* in curvatures of the ray and the Earth:

$$\frac{1}{R_{eff}} = \frac{1}{R} - \frac{1}{\mathbf{OS}}$$

(see Fig. 4b). In terms of the ratio of curvatures $k = \mathbf{OC}/\mathbf{OS}$ used in the discussion of magnification, the effective radius of curvature is

$$R_{eff} = R/(1 - k).$$

Then

$$t = R_{eff}\,(1 - \cos a) \approx \frac{Ra^2}{2(1 - k)}\,.$$

The ray-bending θ produced by the layer is just k times the central angle **OCM**, which can be found from the distance from **O** to the perigee point **P**:

$$\mathbf{OP} = \mathbf{OC}\,\sin(\mathbf{OCM}/2) \approx \mathbf{OC}\cdot(\mathbf{OCM}/2).$$

But **OP** can also be found from the formula for the distance to the horizon[51]

$$\mathbf{OP} = \sqrt{2tR/(1 - k)}\,;$$

so, equating the two expressions for **OP**, solving for the central angle **OCM**, and multiplying by k to get θ, we have the refraction contributed by the layer of thickness t:

$$\theta = 2k\sqrt{\frac{2t}{(1 - k)R}}\,.$$

This is very large when k is close to unity. For example, a layer with a lapse rate of $-0{\cdot}1$ K/m has $k = 0{\cdot}9$; a 1-m thickness of this layer contributes 10 minutes of arc to the refraction of a ray tangent to its lower surface. And because of the square root, even one centimetre of this layer contributes a minute of arc of refraction. As heat conduction forces the lapse rate to be continuous, there is always a place at the top of a duct where $k \to 1$ and the refraction becomes infinite. This is actually seen by an observer above the duct: the Sun flattens out into a line above the duct, where it gradually fades from view as the extinction also grows without limit. Evidently, the atmospheric structure must be resolved to better than a millimetre if precise results are to be calculated near the top of a duct below eye level.

Mirages and ducting: A constant lapse rate produces an erect but somewhat compressed image of the sky at the astronomical horizon. The air near the observer acts much like a prism, deviating the image of the setting Sun (for example) in proportion to the ray curvature. However, when the lapse rate changes with height — that is, when the temperature profile is curved — the refraction changes rapidly with apparent altitude. Above the astronomical horizon, where tan z varies relatively slowly along the ray, the refraction integral averages out the contributions from different layers so well that the refraction cannot change faster than the zenith distance. But below the horizon, where rays can be horizontal, the perigee layer dominates the refraction, which can change very rapidly with altitude. In particular, if the lapse rate below eye level decreases rapidly with height, as at the base of a low-lying thermal inversion, or immediately above a warm surface, the refraction decreases with increasing zenith distance. When refraction decreases faster than zenith distance increases, we see objects that are actually higher in the sky as we look lower: images become inverted, and we have a mirage. Such mirages[46] do not require ducting; they merely require rapid changes in lapse rate with height, so that the atmosphere acts like a positive, cylindrical lens, producing a real, inverted image of a zone of sky. The zigzag limb of the low Sun often displays a stack of thermal inversions below eye level; O'Connell's book on the green flash[58] shows many fine examples. If there is a duct below eye level, the image of the sky becomes discontinuous where the line of sight is tangent to the top of the duct. The refraction increases without limit just above this boundary, but is finite just below it. The resulting image discontinuities and inversions[22] produce the 'Chinese-lantern' effect on the setting Sun. O'Connell shows a few examples of such discontinuities, which he calls "surfaces of separation".

Calculating refraction near the horizon: Because Lambert's series expansion diverges more rapidly with increasing zenith distance, and becomes useless numerically well above the horizon, a different approach is required at low altitudes. The most useful one is that invented by Biot[36], and independently rediscovered by Auer & Standish[59]. It avoids the divergence of the integrand at the horizon (because of the $\tan z$ factor) by changing the variable of integration from n to the local zenith distance z. This method works well so long as the ray curvature differs appreciably from the Earth's. However, if the ray curvature approaches the Earth's, z remains nearly constant along the ray; the interval of z corresponding to some interval of height becomes vanishingly small, and the integrand must become enormous to keep the area under the curve finite. Mathematically, this is because the denominator of the transformed integrand is proportional to the difference of the curvatures. Computationally, the problem is numerical instability: the denominator is the small difference of two nearly-equal quantities, so the calculation is swamped by round-off error. It therefore becomes unusable when ducting occurs — as was already foreseen by Biot.

Infinite refraction: This 'corner case' (of horizontal rays with curvatures equal to the Earth's) is obviously intractable: it corresponds to rays that circle the Earth endlessly, giving infinite refraction. This situation was beautifully analyzed by Kummer[60], who discovered that an infinite number of infinitely thin images of the whole sky are produced — if we neglect extinction.

Refraction and extinction

Laplace's extinction theorem: The relation between extinction and refraction was established by Laplace[1] two centuries ago. His result is equation [8599] in Nathaniel Bowditch's admirable translation[61] of the *Mécanique Céleste*; in Bowditch's words, it is just: "... the logarithm of the intensity of light, of any heavenly body, is proportional to its refraction, divided by the cosine of its apparent altitude." Or, remembering that $\cos a = \sin z$, we can say that the refraction r is proportional to the product $M \sin z$, where M is the airmass.

To see why this is so, consider that the differential of refraction is the element of path length ds times the component of the refractivity gradient normal to the ray. As the gradient is vertical, the projection factor is just $\sin z$; and the refractivity gradient is proportional to the density gradient, $d\rho/dh$. So the refraction is

$$r \propto \int \left(\frac{d\rho}{dh} \right) \sin z \, ds .$$

But the airmass is just proportional to the integral of the density itself:

$$M \propto \int \rho \, ds .$$

We would have everything needed to obtain Laplace's theorem if $d\rho/dh$ in the refraction integral were just proportional to ρ in the airmass integral. In general, they are not proportional. But Laplace imposes a fairly mild condition: he assumes the atmosphere is isothermal, so the density decreases exponentially with height. And of course the derivative of an exponential is proportional to the exponential itself; so we have what we need. There is also the problem that $\sin z$ is not constant along the ray. However, it is nearly constant along the ray if the zenith distance is not too large, because z is nearly constant. And, near the horizon, $\sin z$ is nearly unity, even though z varies by a few degrees as the ray traverses the atmosphere. As a result, the variation of $\sin z$ is not a serious problem; as Laplace demonstrates, the theorem is a moderately good approximation.

Although the real atmosphere is not isothermal, most of the refraction (and airmass) is contributed by the bottom few kilometres[12], in which the temperature varies by only a few per cent. So Laplace's extinction theorem is really fairly accurate. As a trivial example, consider the simple approximations for the plane-parallel atmosphere: $r \propto \tan z$ and $M \approx \sec z$. Their ratio is just $\sin z$, as expected. Laplace's result has also been verified for more realistic model atmospheres[62]. A handy corollary of Laplace's theorem is that near the horizon, where $\sin z \approx 1$, the refraction is very nearly proportional to the airmass.

Series expansions: Lambert's series-expansion method can also be applied to airmass calculations[63]. Apart from the $\sin z$ factor, the terms are similar; however, because airmass has the density where refraction has the density *gradient*, the moments that appear in the coefficients of the terms are all one order higher in the airmass series. In particular, the coefficients begin with the first moment, not the zeroth moment, so there is no term in the airmass series that is independent of atmospheric structure, and we have nothing comparable to Oriani's theorem.

Who cares about extinction? Although refraction and extinction are intimately related, they have traditionally belonged to different fields. Refraction has been the concern of astrometrists; extinction, that of photometrists. However, refraction has to be taken into account in ground-based photometry — not only for telescope pointing, but because the blue image of a star lies above the red one, so that aperture errors and centring may differ, depending on the passband being measured. This is particularly a problem for the large-airmass observations needed to determine the extinction. Furthermore, it is the refracted and not the true zenith distance that is the independent variable in the airmass tables, so the wrong airmasses will be used if zenith distances calculated from times and positions are not corrected for refraction[18]. On the other hand, extinction is a problem for astrometry, because the refraction is wavelength-dependent; and the effective wavelength depends on atmospheric reddening, varying with zenith distance. Finally, the tropospheric corrections required in *GPS* calculations are more closely allied to airmass than to refraction. Consequently, all observers should understand both airmass and refraction.

Discussion

The physics of atmospheric refraction is simple. But it is obscured by the lengthy semi-convergent series expansions that were introduced by Lambert[52]. Because astronomers have concentrated on ridding observational data of the nuisance of atmospheric refraction, rather than regarding it as a subject to be understood in its own right, much effort has been mis-directed. For example, many people, including Simon Newcomb[6], have worried that the poorly-understood structure of the upper atmosphere was a major source of uncertainty in refraction tables; in fact[12] it is quite unimportant. The 19th-Century emphasis on analytical rather than numerical methods led to an emphasis on atmospheric models that made the series expansions tractable — even after Biot[36] had shown that accurate numerical integrations require only modest computational effort. Most of the competing models were polytropes[64,3] of various degrees. These all have a constant lapse rate, so they terminate with an absolute temperature of 0 K at a finite height — a feature that worried many workers.

Cassini's homogeneous model can be regarded as a polytrope of index zero; the Simpson[65]–Bradley[66]–Meyer[67] model has polytropic index one; and the isothermal model has an infinite polytropic index. The efforts of Laplace[1] and Ivory[48,54] went into constructing a sort of interpolation or hybrid formula between these extremes. Because of the poorly-determined position of absolute zero on existing temperature scales, Ivory decided the best polytropic index (to put his result in modern terms) was 3 or 4. Later, Bauernfeind[53] worked out the polytrope of index 5; and Radau[2] gives results for 4, 5, and 6. These workers all struggled with the conflict between the small height of the polytropic model, which ends at $(m + 1) H$ if m is the polytropic index, and the much greater height of the atmosphere inferred from meteors, aurorae, and twilight phenomena. Today, we understand that this is due to the isothermal stratosphere, which greatly extends the height of the upper atmosphere without[12] affecting its refraction. Indeed, Ivory's model gradually tails off into a nearly-isothermal upper extension, which he recognized was poorly constrained by refraction data. Another problem these workers had was that the mean tropospheric lapse rate produces less refraction within 2° of the horizon than is observed. This, we now understand[12], is due to the nocturnal thermal inversion, whose importance was first urged by Oppolzer[68], but without success.

If refraction corrections are needed for precise astrometric observations, Cassini's model is more than good enough[12] — as, indeed, is suggested by Oriani's theorem[37,38]. If the boundary layer were featureless, Biot's magnification theorem[35,36] would suffice to relate the local lapse rate to the Sun's flattening at the horizon. If the details of refraction near the horizon are required, as in explaining sunset mirages[22,24,46] and their associated green flashes[69,58,70,71], one must take account of the complex thermal structure in the boundary layer. And because of the dispersion that causes green flashes, the atmospheric reddening, and hence the linkage between refraction, airmass, and effective wavelength of observation, must always be kept in mind.

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'BEST TIME'
FOR THE FIRST VISIBILITY OF THE LUNAR CRESCENT

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The concept of 'best time' for the first visibility of the thin crescent moon developed by Bruin, Schaefer, and Yallop did not consider the elevation of the site of observation. Our first estimation — after analyzing some documented observations — is that the 'best time' is directly proportional to site elevation and inversely proportional to the Moon's altitude. For moderate-elevation sites (less than 1000 m) the crescent could first be seen shortly after sunset. However, for higher elevations (around 2000 m) the crescent could first be seen shortly before moonset.

By using our first-visibility photometric model, the extensive data of Blackwell's 1946 experiment, and the measured twilight-sky brightness of our site (1990 m), we find that the optimum lunar altitude for first visibility is about 2 degrees, no matter what the lunar elongation is.

Introduction

To determine the first visibility of the thin crescent moon, we have to know the precise point in the sky where the Moon is to be looked for. We also have to know when is the best time for making the observation, *i.e.*, when the contrast between the crescent moon and the twilight sky is becoming sufficient for the Moon to be seen.

Schaefer[1] calculated the best time from the logarithm of the actual total brightness of the Moon divided by the total brightness of the Moon needed for visibility for the given observing conditions. Based on Bruin's work[2], Yallop[3] found a simple formula for the 'best time' in minutes, after sunset, which is equal to 4/9 × the lag time of the Moon. The lag time of the Moon is the length of time in minutes between the time of sunset and moonset.

Pepin[4] reported two observations: Stamm of Tucson (Arizona), with elevation of 860 m, spotted the crescent moon just 7 minutes after sunset on 1996 January 21 (Yallop's 'best time' = 17 minutes). On the other hand, Patchick of Mt. Wilson (California), with elevation of 1740 m, saw the same crescent on the same day, but 28 minutes after sunset (Yallop's 'best time' = 18 minutes). Al-Mostafa[5] of Laban (Saudi Arabia), with elevation of 600 m, spotted the Moon only 5 minutes after sunset (Yallop's 'best time' = 10 minutes). All three above-mentioned observations were made with a telescope.

Those three observations show that 'best time' depends on the elevation of the observation site. When it is at moderate elevation (less than 1000 m), as it was in Stamm's and Al-Mostafa's observations, the crescent may first be seen shortly after sunset ('best time' is earlier than that predicted by Yallop). When the site elevation is around 2000 m, as it was in Patchick's observation, the crescent must first be seen shortly before moonset ('best time' is later than that of Yallop).

Discussion

At any instant, during or after new moon, there is a site on the Earth where it is sunset, and the Sun and the Moon are in perfect geometrical situation, *i.e.*, the difference in azimuth between the Sun and the Moon (DAZ) = 0°. We use standard software to locate this site and to find all parameters to be used in the six equations of our photometric model[6]. Then we can calculate the lunar altitude at which the Contrast Ratio, C_r, has a maximum value; C_r is defined as the ratio of the contrast, C, to Blackwell's Contrast Threshold[7], C_{th}.

The western-sky twilight (L_B) was measured at Mouneef (1990-m height, 44°E, 13°N). The measurements were achieved in 2002 October using a PHYWE selenium photocell (45-mm diameter, corrected to human eye; calibration with metal filament lamp at 2850 K); the measurement pointing was fixed where the Sun set with a depression of 0 to 8 degrees (Fig. 1).

Example: On 2002 March 14 the instant of new moon was at 0205 UT. Using MICA and/or IMCCE software, we find the site (67°S, 106°W) where it is sunset at the instant of new moon, and at which DAZ=0°. We can also find all the quantities needed for calculating the actual luminance of the Moon, $L_\star$, *i.e.*, the lunar magnitude = –4·43, the semi-diameter of the lunar disc = 14·68 minutes of arc, the topocentric lunar elongation = 4°, and the illuminated fraction of the lunar disc = 0·19%. Taking into consideration our site elevation, we can calculate the apparent luminance of the Moon, L, for different lunar altitudes.

Knowing the apparent luminance of the Moon, L, the width of the illuminated lunar disc, W, and the twilight sky luminance, L_B, we can calculate the contrast

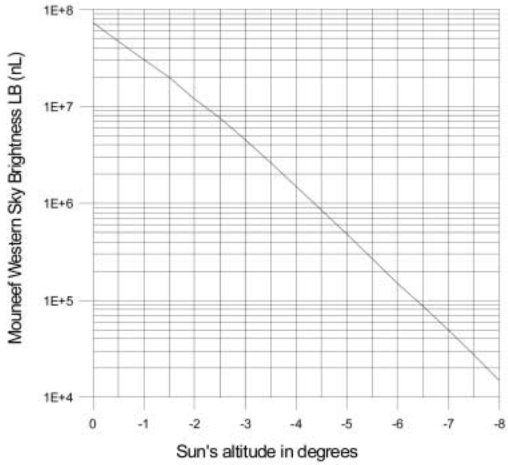

FIG. 1

This figure shows Mouneef western-sky twilight luminance in nL as a function of the Sun's depression in degrees.

TABLE I

Summary of lunar-visibility calculations

Circumstances
Observation time (UT): 2002 March 14, 0205
Phase angle: 175°
Semi-diameter of the lunar disc: 14′·68
Lunar apparent magnitude: –4·43
Elongation (including lunar parallax), with DAZ = 0°, and elevation 2000 m: 4°
Illuminated fraction of the lunar disc: 0·19%
Actual (extra-atmospheric) luminance of the Moon: $4 \cdot 3 \times 10^8$ nL

Moon Alt.,	*Sun Depr.*	L(nL)	$W(')$	L_B(nL)	C_{th}	C	C_r
3°	1°	$4 \cdot 5 \times 10^7$	0·06	$3 \cdot 0 \times 10^7$	46	0·5	0·011
2°	2°	$2 \cdot 4 \times 10^7$	0·06	$1 \cdot 2 \times 10^7$	60	1·0	0·017
1°	3°	$8 \cdot 4 \times 10^6$	0·06	$4 \cdot 5 \times 10^6$	67	0·9	0·013

between L and L_B. To obtain Blackwell's Contrast Threshold C_{th} for discs of diameter less than 0·6 minute of arc, we extrapolated[8] the data in Table VIII of Blackwell[7]. Finally, we calculate C_r, for different lunar altitudes. We find that C_r has a maximum value when the Moon's altitude is about 2 degrees; Table I summarizes the above example.

By repeating the steps of the above example for lunar elongations of 5, 6, 7, and 8 degrees, we get the same result, *i.e.*, C_r has a maximum value when the Moon's altitude is about 2 degrees, no matter what the lunar elongation is (Fig. 2).

Conclusions

From the discussion above, we may conclude that the 'best time' for the first visibility of the thin crescent moon is directly proportional to site elevation and

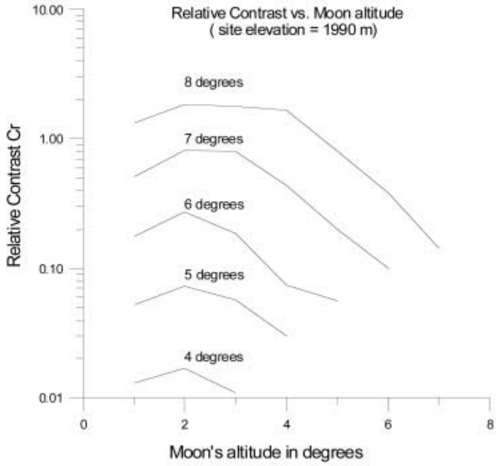

FIG. 2

At site elevation of around 2000 m, the optimum altitude of the lunar crescent to be first seen is about 2 degrees.

inversely proportional to the Moon's altitude. At a site elevation of 1000 m or less, the crescent could be first seen 5 to 10 minutes after sunset, whereas at a site elevation of around 2000 m the crescent could not be seen until about 30 minutes after sunset. We also conclude that, at site elevations of around 2000 m, the optimum lunar altitude for first visibility is about 2 degrees.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 187: HR 3936 AND HD 100215

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The two stars discussed here are γ Doradûs variables — A- or early F-type stars within the instability strip, showing a particular type of pulsational variations with very small photometric amplitudes. Their radial velocities are only marginally measurable with the *Coravel*, but the stars are in need of reliable orbital determinations which this paper sets out to supply. The periods are about 34 days for HR 3936 and 48 days for HD 100215; two erroneous periods presently exist in the literature for the latter. Both stars have been described as double-lined, but *Coravel* traces show only one dip for HR 3936 so from our own observations we can give only a single-lined orbit for that star. By the rather extreme expedient of rejecting two of the four velocities found in the literature for the secondary star, however, we can suggest a first estimate of the mass ratio. HD 100215 clearly has a large Δm, but there are enough secondary velocities in the literature, slightly reinforced by an exiguous second dip seen in sufficiently well-integrated *Coravel* traces, that a double-lined orbit can be given. The eccentricity of the orbit of HR 3936 is small (0·06) but significant; that of HD 100215 is moderate (0·24).

Introduction — γ Doradûs stars

The γ Doradûs stars constitute a rather recently-recognized type of variable star. The discovery of the variability of γ Doradûs itself was made by Cousins in the course of his work at the Cape Observatory on southern bright stars and was published[1] as long ago as 1952, in this *Magazine*. Cousins & Warren[2] noted that the changes cover a total range of $0^{m}\cdot04$ or $0^{m}\cdot05$ but could not identify any periodicity. Cousins would not let the matter rest, and (still active despite terrific age) finally published[3], again in this *Magazine*, in 1992 a paper giving the full picture; he identified two principal periods (0·733 and 0·757 days) which beat together, producing a variation of amplitude with a periodicity of 23·5 days. By that time γ Doradûs, although noted in the *Bright Star Catalogue* as being of type F4 III, was known to be a main-sequence star, as was subsequently confirmed by *Hipparcos*. Cousins[3] recognized the likelihood that the photometric variations arise from some sort of pulsation, but pointed out that they were much slower than those normally found in δ Scuti stars.

Certain other stars were found to exhibit variability of somewhat analogous character. The present writer provided slight assistance in elucidating its cause in the case of one of them, 9 Aur, by making at Haute-Provence an intensive series of hourly radial-velocity observations, whenever possible during a *Coravel* observing run lasting from 1994 December 25 until 1995 January 9; the 83 traces documented not only a radial-velocity variation with a period identical with one of those found photometrically but also variations in the depth and width of the line profile[4]. Eventually Kaye *et al.*[5] took upon themselves in 1999 to define the characteristics of a new class of variable, of which the type star would be γ Doradûs, identifying the underlying cause of the variations as non-radial gravity (g-) modes of high order (n) and low degree (l). At much the same time Handler[6], after searching the whole of the *Hipparcos* 'epoch photometry' for stars that showed variations of the appropriate character, published a list of 70 γ Doradûs candidates, of which 46 were considered to be 'prime candidates'. The two stars that form the subject of the present paper are among those 46, and are discussed successively below.

HR 3936

HR 3936 is a 6½^m star, to be found about 2° north-following the fourth-magnitude μ Leo, of 'super-metal-rich' fame. Its broad-band magnitudes have been given by Oja[7] as $V = 6^m\cdot48$, $(B-V) = 0^m\cdot34$, $(U-B) = -0^m\cdot01$. *Hipparcos* agreed with Oja's V magnitude, and gave (from *Tycho*) $0^m\cdot362$ for $(B-V)$. It has been classifed F1 V by Anne Cowley[8], and subsequently by her in collaboration with Bidelman as[9] F3 V; Abt & Morell[10] have given it as F2 V. The *Hipparcos* parallax is $0''\cdot01498 \pm 0''\cdot00082$, leading to a distance modulus of $4^m\cdot12$ and thus to an absolute magnitude of $+2\cdot36$.

In 1985 Eggen suggested[11] that HR 3936 was a member of the 'Hyades supercluster'. The 'cluster parallax' required to produce the correct transverse velocity from the proper motion was $0''\cdot0157$ — very close, as we see, to the value later determined by *Hipparcos*, and leading to a distance modulus of $4^m\cdot02$. Just how Eggen deduced from that modulus and, presumably, the apparent magnitude of the star, that M_V had to be $3^m\cdot05$ is far from clear, but that is what he did (it is an unproductive effort to try to tie up the loose ends in such a paper, even in the context of just one star), and he compared that 'astrometric M_V' with a 'photometric' value, from Strömgren/Hβ photometry, of $2^m\cdot9$. He gave the radial velocity corresponding to cluster membership as $+26\cdot7$ km s^{-1}. Shortly afterwards, he repeated the modulus and the computed radial-velocity figure in another paper[12], one that is not retrieved by *Simbad*. On a later occasion when he included HR 3936 in another table of 'Hyades supercluster' members, Eggen[13] appeared to correct the observed magnitude of HR 3936 for interstellar absorption, but confusingly obtained a corrected magnitude of $6\cdot80$, substantially *fainter* than the observed value, and he did so on the basis of a reddening that he gave as $E(b-y) = 0\cdot00$, so one might well think that no correction was warranted at all. He had revised his figure for the modulus to $3^m\cdot93$, and that for the 'photometric' absolute magnitude to $+2^m\cdot75$. In the later paper[13] the required radial velocity for cluster membership had changed slightly from the previous figure to $+25\cdot9$ km s^{-1}; in place of the observed value there is the entry 'Note', but actually there is *no* corresponding note. In any case the required radial velocity differs by 13 or 14 km s^{-1} from the γ-velocity derived below, so any idea of 'supercluster' membership can be considered dead.

The radial velocity of HR 3936 was first determined in 1930 by Shajn & Albitzky[14] at the Simeis station of the Pulkova Observatory. They obtained spectrograms at a reciprocal dispersion of 36 Å mm^{-1} at Hγ with a prismatic spectrograph at the Cassegrain focus of the 40-inch Grubb reflector (a telescope which subsequently acquired the doubtful distinction of being destroyed during the Second World War). They found a mean radial velocity of +35 km s^{-1} from four plates, and noted, "S.B. Two spectra. Separation is difficult." Shortly afterwards they amplified the account in their *Monthly Notices* paper[14] with a more detailed one[15] in the *Publications* of their own observatory; the velocities are there tabulated individually, and it is seen that they show a range of only 13 km s^{-1} — less than a quarter of the $2K$ range demonstrated below. There is a note, "The duplicity is difficult. Several lines on [the first plate] are double, but the measurements are uncertain. The depth of the lines varies."

There has been an unusual amount of interest in the rotational velocity or velocities of HR 3936. The *Bright Star Catalogue* tabulates a value of 75 km s^{-1} in the body of the *Catalogue*, and includes a note at the back giving 30 km s^{-1} for the secondary component. The present author does not know where that information came from: the only known paper early enough to have served as the source, by Danziger & Faber[16], lists "90, 30*", but does not give any indication of what the asterisk is intended to convey. Abt & Morrell[10] gave $v \sin i$ as 25 km s^{-1}, Wolff & Simon[17] 38, Fekel, Warner & Kaye[18] 25 and 30:, and Mathias *et al.*[19] 37 and 25. The latter syndicate included in their tabulation a column to indicate whether stars listed are binaries. The entry in it for HR 3936 is simply "Binary", whereas for some other stars "SB1" or "SB2" is specified; it is puzzling that there should be two entries for $v \sin i$ but that the opportunity to say definitely that the object is double-lined was passed over. Royer *et al.*[20], in a table accessible only from a remote computer, appeared to interpret Abt & Morrell's 25 km s^{-1} as really meaning 33.

HR 3936 has been viewed with renewed interest since Handler[6] identified it as a 'prime candidate' for a γ Doradûs variable. He found periodicities of 0·775 and 0·844 days in the *Hipparcos* photometry. The 0·775-day period was independently detected by Koen & Eyer[21] in a separate general investigation of *Hipparcos* photometry (their actual tabulation gives the reciprocal number as a frequency). In a more limited effort of the same nature, Adelman, Coursey & Harris[22] searched the *Hipparcos* photometry to identify variability among the bright stars having spectral types from F1 to F9 V. HR 3936 features in their list of about 100 such objects; it is attributed an amplitude of $0^m \cdot 07$ and a type of I. What those authors mean by 'amplitude' is not clear — it surely cannot be equivalent in character to the amplitude K of radial velocity in the orbit of a spectroscopic binary, and seems more likely to represent the extreme range of variation. The type 'I', of which HR 3936 is the only example in their list, is presumably the one designated by that symbol in the *General Catalogue of Variable Stars*[23], where it is defined (under the seemingly inappropriate heading "Eruptive variable stars") as "poorly studied irregular variables with unknown features of light variations and spectral types. A very inhomogeneous group of objects."!

Following up the identification by Handler[6] of so many γ Doradûs stars, Fekel, Warner & Kaye[18] undertook at the Kitt Peak coudé-feed system a spectroscopic investigation of 34 of them, including HR 3936 and HD 100215. For the former star they were able to see in the spectrum two components, to which they assigned spectral types of F0 and F5:, with rotational velocities of 25 and 30: km s^{-1}, respectively. They assigned to both components the luminosity class "Dwarf", on the basis of the absolute magnitude resulting from the *Hipparcos* parallax, not from direct classification, because they were unable to find suitably luminosity-sensitive pairs of lines in the red region, where they observed, of the spectrum. Rotational velocities were estimated in a somewhat indirect manner by the use of a line-width calibration set up previously by Fekel[24]. Radial velocities were tabulated for both components from three spectra, two of which were taken on consecutive nights. Fekel *et al.* described them as showing asymmetrical lines which they interpreted as blended pairs, with a continuum Δm of $0^m \cdot 9$, but they also expressed some residual hesitation as to whether the object is really double. The final spectrum, observed two seasons later, was taken in the blue and is described in the paper as showing similar line profiles but with the asymmetries reversed; part of it is illustrated in the paper[18] and looks to the present writer to be convincingly double-lined without any possible doubt. Despite their hesitation, Fekel *et al.* gave twin velocities from all three spectra.

Fekel next teamed up with Henry[25] to investigate certain of the stars photometrically as well as spectroscopically. They gave a (V, B) photometric model for the HR 3936 system on the basis of the previously estimated[18] Δm of $0^m \cdot 9$, and identified four different photometric periods, two of them being those already found by Handler[6] and the other two being slightly longer; in order of decreasing amplitude they are 0·7753, 0·8921, 0·8750, and 0·8435 days. They also gave one more pair of radial velocities.

In view of the clear interest in HR 3936 and the desirability of determining its orbit, in 2004 April the writer made an effort to see whether its radial velocity could be measured with the Cambridge *Coravel*. The star was found to give a trace showing a wide and very shallow dip, that could be measured although not with great accuracy. The depth of the dip, from the continuum, is less than 3%, in comparison with normal depths ranging from about 20% for type G0 V to more than 40% for late-K giants. The first few trials included a wide range of velocity space in the hope of allowing the secondary to be observed too, but if the secondary appeared at all it could not be reliably measured: velocities of extremely weak features that were tentatively measured in the hope that they represented the secondary did not show an anti-phase relationship with the primary, so the attempt to obtain a double-lined orbit was abandoned. It proved, however, quite possible to obtain a single-lined one, and to that end 28 velocities have been obtained with the *Coravel* for the primary star. They are listed in Table I, at the head of which are included the earlier velocities published by Albitzky & Shajn[15] and by Fekel and his collaborators[18,25]. They have all been increased by 0·8 km s^{-1} above the published values, in an effort to account for the offset often found between Cambridge velocities and others.

The Cambridge data alone readily yield an orbital solution whose period is 33·716 ± 0·018 days. The Kitt Peak primary velocities fit it well; when they are incorporated into the solution they improve substantially the precision of the period, which becomes 33·703 ± 0·005 days, owing to the great increase (from 17 to 58 cycles) that they offer in the time base. It has not been considered a good idea to attribute higher weight to them than to the Cambridge observations, despite their much smaller residuals, because (*a*) their phase distribution is far from ideal and could conceal significant error in the zero-point offset that has been applied, (*b*) two of them are close to the γ-velocity and (if the system is really double-lined with a Δm of only $0^m \cdot 9$) they must be expected to be 'dragged' appreciably towards the γ-velocity because of blending with the secondary, and (*c*) in any case there are so few of them that the

TABLE I

Radial-velocity observations of HR 3936

The sources of the observations are as follows:
1930/1931 — Simeis[15], not used in orbital solution;
2000–2003 — Kitt Peak[18,25]; 2004/2005 — Cambridge Coravel.

Date (UT)			*MJD*	*Velocity* *km s^{-1}*	*Phase*	*(O – C)* *km s^{-1}*
1930	Apr.	22·27	26088·27	+29·6	$\overline{802}$·478	−21·9
		24·29	090·29	+39·8	·538	−19·6
	May	6·75	102·75	+42·3	·907	+1·1
1931	May	8·28	26469·28	+30·6	$\overline{791}$·783	−31·3
2000	July	9·13	51734·13	+41·8	$\overline{41}$·421	−0·9
		9·13*	734·13	+4·1	·421	—
		10·14	735·14	+47·8	·451	+0·3
		10·14*	735·14	−1·2	·451	—
2002	Feb.	24·28	52329·28	+11·9	$\overline{23}$·080	−0·3
		24·28*	329·28	+74·7	·080	—
2003	Mar.	7·34	52705·34	+15·7	$\overline{12}$·238	+0·6
		7·34*	705·34	+75·7	·238	—
2004	Apr.	21·92	53116·92	+47·7	0·450	+0·3
		25·95	120·95	+62·9	·570	+0·2
	May	10·90	135·90	+20·0	1·013	−0·6
		18·89	143·89	+17·0	·250	+0·5
		21·92	146·92	+29·6	·340	+0·2
		22·89	147·89	+34·3	·369	+0·2
		24·90	149·90	+42·8	·429	−1·2
	Nov.	14·25	323·25	+60·4	6·572	−2·6
	Dec.	26·19	365·19	+54·7	7·817	−2·8
		27·16	366·16	+51·8	·845	−1·1
2005	Jan.	2·09	53372·09	+20·4	8·021	+1·1
		5·10	375·10	+11·2	·111	+1·0
		9·08	379·08	+13·9	·229	−0·3
		11·17	381·17	+21·4	·291	−0·4
		13·12	383·12	+31·1	·349	+0·3
		14·17	384·17	+35·6	·380	−0·3
		19·16	389·16	+59·5	·528	+1·3
		22·12	392·12	+66·9	·616	+0·7
		23·17	393·17	+69·4	·647	+1·8
		26·05	396·05	+67·6	·732	+1·3
	Feb.	9·03	410·03	+10·1	9·147	+0·5
	Mar.	25·97	454·97	+54·4	10·480	+2·5
	Apr.	3·94	463·94	+65·3	·747	0·0
		4·94	464·94	+62·8	·776	+0·2
	May	11·94	501·94	+49·5	11·874	+1·8
		13·90	503·90	+36·8	·932	+0·8
	Nov.	4·24	678·24	+8·3	17·105	−2·2
		19·25	693·25	+58·3	·551	−2·5

*Measurement of secondary star, not used in solution of orbit.

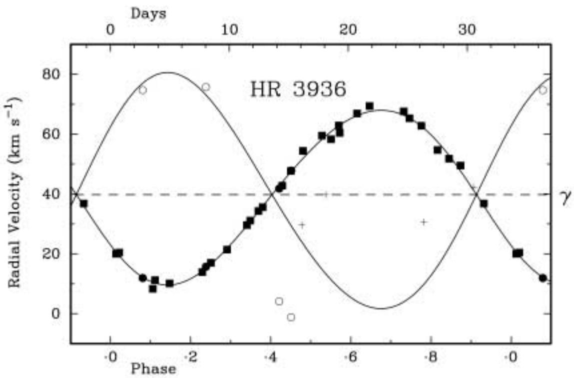

FIG. 1

The observed radial velocities of HR 3936 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The filled symbols (circles for Kitt Peak[18,25], squares for Cambridge) represent observations of the primary star and are the sole basis for the orbit. The + symbols plot the Simeis observations[15], which clearly could not be taken into account in the orbital solution. The open circles refer to the Kitt Peak measurements of the secondary star; it is conjectured that the two near the foot of the diagram, made at phases when the primary was close to the γ-velocity and the spectrum could be expected to be single-lined, must have been illusory, but the other two are plausible and form the slender basis upon which a possible velocity curve for the secondary is sketched in.

smallness of their residuals could rather easily be a statistical fluke. The Simeis velocities show no very clear relationship with the orbit and so cannot be taken into account in its solution. They date from an epoch some 800 cycles ago, and extrapolation of the phasing to such a remote epoch increases the 1-σ uncertainty to about an eighth of a cycle, so the cycle count is not in doubt although the exact phasing is not well determined.

The computed velocity curve for the primary star is shown in Fig. 1, and the corresponding orbital solution has the following elements:

P = 33·703 ± 0·005 days	$(T)_3$ = MJD 53202·9 ± 1·2
γ = +39·78 ± 0·26 km s^{-1}	$a_1 \sin i$ = 13·50 ± 0·17 Gm
K = 29·2 ± 0·4 km s^{-1}	$f(m)$ = 0·087 ± 0·003 $M_\odot$
e = 0·063 ± 0·012	
ω = 123 ± 12 degrees	R.m.s. residual = 1·3 km s^{-1}

The four secondary velocities measured[18,25] at Kitt Peak are plotted in Fig. 1, where they can be seen to present an impossible relationship with the orbit of the primary. The first two, which were obtained from spectra taken on consecutive nights and concerning which the authors[18] expressed doubts, are represented in Fig. 1 by the two open circles near zero velocity, at phases when the primary was quite close to the γ-velocity. For the purposes of the present discussion we reject them. That leaves only two measurements of the secondary, but those two (near the upper left corner of the Figure) are plausibly related to the corresponding velocities of the primary, and in a moment of optimism could be used to estimate a mass ratio. That could be done either by running a double-lined orbital solution in which the two secondary velocities are attributed very small weights, or simply by considering the relative displacements of the primary and secondary velocities from the known γ-velocity on each occasion. The former operation has been conducted in order to produce the putative secondary velocity curve that is drawn in the figure, and the latter method has been used to obtain for the two observations the independent values of q (= m_1/m_2), which turn out to be 1·24 and 1·48. They are very much of the right order to be consonant with the mass ratio to be expected for a pair of main-sequence stars whose types are about F0 and F5. Too much should not be read into that coincidence, however, since it could justly be claimed that to choose two data out of four and then see that they reinforce pre-existing prejudice smacks of logical circularity. It may also be thought to be a bit surprising, if the *Coravel* traces really contain a secondary dip that is too weak to measure as an independent entity, that there is no sign in Fig. 1 of the points that are near to the γ-velocity being dragged towards that velocity by blending with the secondary; on the other hand the same could be said of HD 100215, where the secondary does appear to be marginally visible in the traces.

HD 100215

HD 100215 is an 8^m star of fairly early but much-disputed type. It is in Ursa Major, nearly 1° south-following 57 UMa, a 5^m object in the Bear's left hind leg. It has tended to feature in astrometric investigations (of no direct concern in this paper) owing to its declination of very nearly 39°, which results in its passing practically through the zenith of the 'latitude observatories' of Mizusawa and Washington where photographic zenith tubes (PZTs) are in operation.

Mendoza first listed HD 100215 in 1974 in what was entitled a photometric catalogue[26], but he did not provide any photometry of it; four years later, however, he recruited some collaborators and produced[27] the magnitudes V = 7·99, (B − V) = 0·31, (U − B) = 0·04. Shortly afterwards, very similar results were provided by Guetter[28] who, however, placed a colon after the V magnitude of 7·99, remarking that it was suspected of variation; values from $7^m{\cdot}95$ to $8^m{\cdot}02$ had been measured, and the star was one out of only six of the 259 'PZT stars' observed whose mean magnitudes had a standard error greater than 0·01 even after five or more measurements. *Hipparcos* gave exactly the same mean magnitude, 7·99, as had been found previously, but again noticed small variations; as in the case of HR 3936, first Handler[6] and then Koen & Eyer[21] identified periodicities in the *Hipparcos* photometry. It was Handler who recognized the the γ Doradûs nature of the variability, for which he found periods of 0·757 and (with less confidence) 0·434 days; the former period is the same as was found by Koen & Eyer. The subsequent photometric campaign by Henry & Fekel resulted in the finding of four different periodicities, with periods of 0·7564, 0·7029, 0·7824, and 0·6190 days, in order of decreasing amplitude. The first one, which is the one definitely identified by Handler, has much the largest amplitude, though it is still only $0^m{\cdot}034$; the other possible period suggested by Handler was not confirmed.

The spectral classification of HD 100215 has long been a bone of contention. Slettebak & Stock[29] included the star as no. 138 in 'A finding list of stars of spectral type F2 and earlier in a north galactic pole region' and assigned a type of F0(m:). That entry prompted Bertaud to include it (as no. 554) in the first supplement[30] to his catalogue and bibliography of peculiar A stars, perhaps on the ground that a metallic-line star, even if of type F0, could be considered an honorary A star; HD 100215 appears again in the second supplement[31] merely to note its BD number. Warren[32] gave the star a type of "F0 m-:", explaining that the "m-" meant 'mildly metallic-lined'; Fehrenbach, who made the classifications of objects for which his syndicate[33] obtained radial velocities, gave it as F2 V. The radial velocities were not measured, like most of those published by Fehrenbach, by the objective-prism method: they came from slit spectrograms taken at a reciprocal dispersion of 80 Å mm^{-1} with the *Marly* spectrograph[34] on the 1·2-m telescope at Haute-Provence, and we shall see below that in relation to their low dispersion they are remarkably accurate. Grenier *et al.*[35] gave a great number of classifications and radial velocities, similarly obtained with the *Marly* spectrograph; their spectral-type entry for HD 100215 is "A5mF0F2", which if it follows the usual convention means that they saw it as a metallic-line star of main-sequence luminosity, having a *K* line of the strength appropriate to type A5, a Balmer-line type of F0, and metallic lines appropriate to F2. They listed a mean radial velocity of −16·4 ± 14·2 km s^{-1} from three plates, and of course recognized the variability, but nevertheless did not list the individual velocities and dates. Sato & Kuji[36] provided classifications for a lot of PZT stars; one of them was HD 100215, which was called A7 V and A9 V from two different plates, and was explicitly noted as *not* being Am.

Fekel, Warner & Kaye[18] recognized the spectrum as being double-lined, belonging to a pair of dwarf stars with types of F1 and G0:, with rotational velocities of 25 and 15 km s^{-1}, respectively, and a Δm of $2^{m}\cdot 4$. They noted the variability reported in its radial velocity by Fehrenbach *et al.*[33] and Grenier *et al.*[35], and gave two radial velocities of their own from spectra taken at the Kitt Peak coudé feed only four days apart: one of them was at a single-lined phase, but in the other case velocities were measured for both components. Soon afterwards Henry & Fekel[25], as well as obtaining the photometry noted above, reported six additional spectra "which for the first time show resolved double lines"; the dates indicate that they were made in two separate observing runs, and from the new velocities plus the two obtained previously a tentative orbital period of 12·2 days was derived. The spectral types were revised to A9 and G5: V and the Δm value to $2^{m}\cdot 6$; on that basis a photometric model was constructed.

Mathias *et al.*[19] have had what is at the time of writing the final say on HD 100215. They obtained 11 spectra over a time interval of a little more than a year, and measured radial velocities from them but did not present them in their paper. On the assumption that the orbit would have zero eccentricity they produced from them an orbital solution with a period of 42·628 ± 0·053 days. They say that line-profile variations such as characterize γ Doradûs stars are "evident", and in another place they describe them as "strong"; they saw the spectrum only as single-lined, with a projected rotational velocity of 13 km s^{-1} — barely over half the value put forward by Fekel *et al.*[18]. Moreover, they rejected the previous attribution of duplicity, saying that "it is probable that there has been a confusion in Fekel *et al.*'s interpretation, as the travelling bumps at a given phase produce a profile similar to a double line. Therefore, this star is clearly confirmed as a new γ Doradus star in a SB1 system." Their dismissal of Fekel *et al.*'s work was so complete that they appear not to have taken into account the radial velocities that they would find there to assist with their determination of the orbit of the primary (or only?) star. Their orbit diagram shows that all but four of their velocities are bunched within a very small range of phase, so there could have been advantage in utilizing data provided by others; they do not refer to the velocities published by Fehrenbach *et al.*[33] either,

TABLE II

Radial-velocity observations of HD 100215

The sources of the observations are as follows:
1984/1985 — Haute-Provence[33], weighted 1/20 in orbital solution;
2000–2003 — Kitt Peak[18,25], weighted 1/2; 2004/2005 — Cambridge Coravel.
All observations of the secondary weighted 1/10.

Date (UT)			*MJD*	*Velocity*		*Phase*	*(O – C)*	
				Prim. *km s⁻¹*	*Sec.* *km s⁻¹*		*Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
1984	Mar.	13·92	45772·92	−9·6	—	$\overline{153}$·099	−8·1	—
		18·03	777·03	+10·2	—	·185	+1·8	—
1985	Feb.	3·06	46099·06	−39·4	—	$\overline{147}$·911	+5·6	—
2000	July	9·16	51734·16	−21·5	—	$\overline{29}$·601	−0·5	—
		13·13	738·13	−31·9	+12·5	·683	−1·1	+3·4
2003	Mar.	7·37	52705·37	−48·2	+37·7	$\overline{9}$·884	−1·7	+0·5
		8·37	706·37	−44·2	+35·5	·905	+1·2	+0·2
		10·39	708·39	−39·4	+25·0	·947	+0·7	−0·7
	Apr.	26·29	755·29	−45·7	+29·0	$\overline{8}$·927	−2·5	−2·4
		28·27	757·27	−37·0	+17·3	·968	−1·2	−0·7
		30·27	759·27	−23·1	+7·1*	$\overline{7}$·010	+1·6	(+9·0)
2004	Apr.	22·03	53117.03	−8·9	—	0·482	−1·0	—
		23·00	118·00	−8·4	—	·502	+1·6	—
		25·97	120·97	−15·0	—	·564	+1·8	—
	May	3·92	128·92	−37·2	—	·730	−0·9	—
		6·07	131·07	−41·3	—	·775	−0·2	—
		16·99	141·99	−26·8	—	1·003	−0·1	—
		21·93	146·93	+2·0	—	·106	+2·1	—
		23·99	148·99	+6·0	—	·149	+0·1	—
	June	4·90	160·90	−0·7	—	·398	−0·7	—
	Dec.	27·19	366·19	−31·8	—	5·686	−0·7	—
2005	Jan.	5·12	53375·12	−45·3	—	5·872	+1·4	—
		9·14	379·14	−39·7	—	·956	−1·3	—
		11·18	381·18	−27·1	—	·999	+0·8	—
		13·15	383·15	−17·1	—	6·040	−1·0	—
		14·19	384·19	−10·4	—	·062	−0·3	—
		19·18	389·18	+5·8	—	·166	−1·5	—
		22·15	392·15	+9·4	—	·228	+0·1	—
		23·18	393·18	+10·1	−64·5	·249	+1·1	−1·9
	Mar.	26·00	455·00	−13·8	—	7·540	+0·4	—
	Apr.	29·91	489·91	—	−59·3	8·270	—	+2·2
	May	2·95	492·95	+3·3	−56·9	·333	−1·8	−1·4
		8·02	498·02	−4·2	−27·6*	·439	−0·5	(12·1)
		8·94	498·94	−4·2	—	·458	+1·3	—
		27·92	517·92	−45·3	—	·855	+1·2	—

*Rejected observation.

although the precision that they quote for the orbital period is just about good enough to keep track of the cycle count back to the epoch of those velocities.

It was the unsatisfactory nature of the published orbit, and its discrepancy from the period surmised by Fekel *et al.* on the basis of independent (if similarly inadequate) data, that prompted the addition of HD 100215 to the

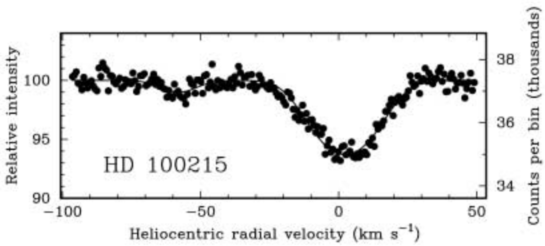

FIG. 2

Radial-velocity trace of HD 100215, representing the result of nearly an hour's integration with the Cambridge *Coravel* on 2005 May 2.

Cambridge radial-velocity observing programme; the first observations were made in the same month (2004 April) that Mathias *et al.*'s paper was published. HD 100215 is not such a difficult star as HR 3936 to observe with the *Coravel*, having a dip that, while still shallow, is not nearly *as* shallow, or as wide, as that of the HR star; on the other hand it is a magnitude and a half fainter and correspondingly more time-consuming to get a trace of any given S/N ratio. The radial velocities given by Fekel and his collaborators already showed the orbital period to be about $49/n$ days, where n was an integer that was almost certainly either 1 or 2. The initial *Coravel* measures immediately fixed n as 1, and it took only a fortnight, with five Cambridge observations, from the start of the observing campaign before the exact period of 47·87 days (divined, of course, with the additional help of the early observations by Fehrenbach *et al.*[33]) became

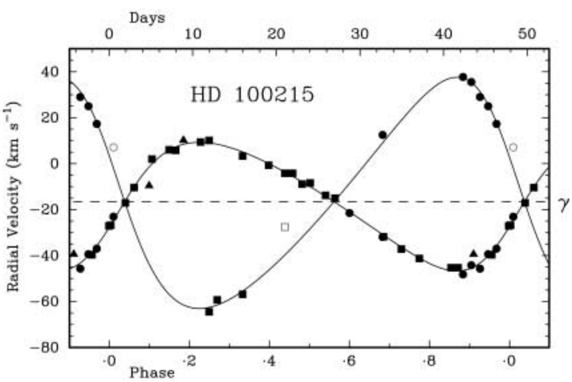

FIG. 3

The observed radial velocities of HD 100215 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Triangles identify radial velocities published[33] from Haute-Provence, circles those from Kitt Peak[18,25], and squares the Cambridge data newly presented in this paper. The curve with the smaller amplitude and the majority of the observations represents the variation of velocity of the primary star. The open symbols near the other curve (that of the secondary) plot measurements that have been rejected; they stem from observations that were made at times when the spectra of the components would have been somewhat blended together, and the extreme relative weakness of the secondary might well excuse serious error.

apparent. There are now 23 Cambridge velocities of the primary star, and they are listed along with the published ones in Table II. The orbit derived from them was readily extended to become a double-lined one by the insertion of the Kitt Peak velocities published by Fekel *et al.*[18] and Henry & Fekel[25] for the secondary. If the corresponding measurements were illusory, as Mathias *et al.*[19] asserted, it is amazing that they fit an orbit that was unknown at the time of their observation as well as they do. Efforts were made to see a secondary dip in *Coravel* traces; they were initially unsuccessful, but more resolute efforts, four of them, made late in the campaign did appear to show a marginally measurable feature. Only one of the four was obtained with a wide scan that covered both of the dips simultaneously when they were fully separated; it is pictured in Fig. 2. In two of the other cases the secondary dip was observed in a separate integration, the primary dip being outside the scan range, and in the fourth case the two dips were partly blended together and in the event the secondary velocity obtained on that occasion has had to be rejected on account of its unacceptable residual. Analogously, the secondary velocity from the last Kitt Peak observation, which was made similarly close to the γ-velocity, has been rejected. Although there is no question but that the secondary velocities are individually unreliable, at least as far as the *Coravel* ones are concerned, it seems likely that the secondary star really *has* been observed both by Fekel and his collaborators and with the *Coravel*, and that collectively the velocities establish a reasonably determined amplitude for that component. It remains, therefore, only to present the orbital elements below and to draw attention to Fig. 3 where they are illustrated graphically.

P	= 47·881 ± 0·006 days	$(T)_1$	= MJD 53141·8 ± 0·3
γ	= −16·56 ± 0·22 km s^{-1}	$a_1 \sin i$	= 17·90 ± 0·21 Gm
K_1	= 28·0 ± 0·3 km s^{-1}	$a_2 \sin i$	= 32·2 ± 0·9 Gm
K_2	= 50·3 ± 1·4 km s^{-1}	$f(m_1)$	= 0·100 ± 0·004 $M_\odot$
q	= 1·80 ± 0·05 (= m_1/m_2)	$f(m_2)$	= 0·58 ± 0·05 $M_\odot$
e	= 0·240 ± 0·011	$m_1 \sin^3 i$	= 1·40 ± 0·10 $M_\odot$
ω	= 251·5 ± 2·6 degrees	$m_2 \sin^3 i$	= 0·78 ± 0·03 $M_\odot$

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

In that solution, an adjustment of +0·8 km s^{-1} has been made to the zero-points of the already-published velocities; the *Coravel* velocities of the primary have been taken as the unit of weighting, and the variances of the three sources have been approximately equalized by the Fekel ones[18,25] receiving weight ½ and the Fehrenbach[33] ones 1⁄20. All the secondary measurements have been weighted 1⁄10. Mathematically the secondary weighting ought to be considerably heavier, but the doubts surrounding the observations do not encourage strict obedience to the statistics of the residuals. The secondary star is so faint in comparison with the primary that velocities that are close to the γ-velocity and must really represent blends have been deemed to be so little affected by blending that they can be utilized as velocities of the primary.

Discussion

If we judge from the spectral types that have been given[18] for the components of HR 3936 that the masses are something like 1·7 and 1·3 $M_\odot$, then we can use the mass function to find that $\sin^3 i \sim 0{\cdot}36$, so the orbital inclination is about

45°. In the case of HD 100215, the double-lined solution gives minimum masses that are about 20% smaller than stars of the relevant types could be expected to have, telling us that $\sin^3 i \sim 0\cdot8$, so i is nearly 70° though with appreciable uncertainty owing to the slow change of $\sin i$ with i at high inclinations. It is obviously not to be expected that eclipses should be seen in either system.

One thing that is rather surprising about the results found here is that the radial-velocity residuals are as small as they are. Speaking for the *Coravel* velocities only, one can remark that they are no worse than could be expected from the shallow and wide nature of the dips that have to be measured in the traces. The depression from the 'continuum' of the dip given by HR 3936 averages a little less than 3%; apart from a few outliers among the earliest measurements, the projected rotational velocities indicated by the individual traces are all within the range 20–25 km s^{-1}, with a mean near 23 and a formal standard deviation that would be misleadingly small as an estimate of the true uncertainty. The dip given by HD 100215 is slightly less wide, the $v \sin i$ averaging 19 km s^{-1} with an r.m.s. spread of 2·5 km s^{-1} among the 23 individual values and a formal standard error of only about 0·5 km s^{-1} — again unrealistically small — for the mean. The r.m.s. radial-velocity residuals for both stars are not much more than 1 km s^{-1}. In view of the γ Doradûs natures of the stars, and especially of the assertion by Mathias *et al.*[19] in respect of HD 100215 that the line-profile variations are "strong" and even that they could have confused Fekel *et al.*[18] into thinking that they were the secondary spectrum, it could be expected that there would be real line-shifts, variable on a short time-scale, of more than 1 km s^{-1}. The expectation could be illustrated by reference to 9 Aur, which is a single star, having in principle a constant radial velocity, but it is a γ Doradûs object that shows line-profile variations; it has been intensively observed with the *Coravel*s both at Haute-Provence and at Cambridge, and it is bright enough and gives a good enough dip that one can be sure that its measured variations are real. It has been found[4] to exhibit bodily shifts of the line profile mimicking radial-velocity changes reaching ±3 km s^{-1}, and variations of the profile sufficient to cause its apparent $v \sin i$, as determined from the *Coravel* traces, to range from 14 to 23 km s^{-1}. There is no scope, among the velocities and dip profiles observed for either HR 3936 or HD 100215, for there to be real variability on anything like such a scale.

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REVIEWS

The Physics and Chemistry of the Interstellar Medium, by A. G. G. M. Tielens (Cambridge University Press), 2005. Pp. 495, 25·5 × 18 cm. Price £45/$85 (hardbound; ISBN 0 521 82634 9).

To the naked eye, the space between the stars appears to be empty. Over the last century or so, evidence began to accumulate that this space was not quite empty, but contained atomic gas and dust grains. The dust was regarded simply as an irritating fog that prevented the proper examination of the main constituents of the Universe, *i.e.*, stars and galaxies. The elements in the gas were simple metal atoms and ions, and seemed to have little connection with the rest of astronomy. However, over the last four decades, this simple picture of the irrelevance of the interstellar medium has been transformed. The advent of astronomy at radio, infrared, and ultraviolet wavelengths has revealed an entirely new scene: we now know that interstellar space is filled with hydrogen gas in both atomic and molecular forms, that both the gas density and temperature vary widely, and that some of the most massive entities in the Milky Way Galaxy (and other galaxies, too) are in the form of (previously unrecognized) massive molecular clouds containing a wide range of molecular species. The interstellar medium is a dynamic place, stirred violently by stellar winds and by supernovae. It is the sink for all the gas and dust ejected from dying stars and the reservoir of matter from which all new stars are made.

Given the key rôle that the interstellar medium plays in the evolution of galaxies, stars, and planets, it is not surprising that the study of this essential dusty gas is now a huge enterprise in modern astronomy. The subject is observationally driven, with millimetre-wave and infrared observations providing much of the impetus. The processes occurring are fascinating from an intellectual point of view, and encompass both macroscopic and microscopic physics and the influence of one on the other. While a number of textbooks describing this area of astronomy exist and are widely used, most cover only limited parts of the subject and few of them are aimed specifically at the graduate student and research scientist. Alexander Tielens, a well-known and highly-regarded expert in the field, has written this book to meet the need for a comprehensive and wide-ranging high-level textbook on the subject. He has succeeded admirably.

A few introductory chapters on the Galaxy and on fundamental processes set the scene, and are followed by discussions of dust, chemistry, interstellar macrophysics, molecular clouds and their interaction with radiation. Two extensive chapters describe polycyclic aromatic hydrocarbons and photodissociation regions, areas (among many others) in which Professor Tielens has made major contributions. The text in each chapter is quite dense, and graduate students must expect to work hard; but each chapter is accompanied by a helpful discussion of further reading. The serious student will find the bibliography invaluable.

The interstellar medium plays such a variety of rôles in modern astronomy that it is difficult to limit the content of a book such as this. Professor Tielens has made the wise choice to concentrate on fairly well-established physical and chemical processes that will give his readers the foundation they will need to begin a career in research.

In these terms the book is comprehensive and destined to be a long-term success, both as a text for graduate students and as a reference for the established researcher who needs to fill a knowledge gap. This authoritative book, written by an expert in the subject, and — for these days — modestly priced, is highly recommended. — D. A. WILLIAMS.

Astronomical Enigmas: Life on Mars, the Star of Bethlehem & Other Milky Way Mysteries, by M. Kidger (Johns Hopkins University Press, Baltimore), 2005. Pp. 256, 24 × 18·5 cm. Price £20/$29·95 (hardbound; ISBN 0 801 88026 2).

Rupert T. Gould — who can be regarded as a precursor of Sir Patrick Moore inasmuch as in the 1930s he gave regular radio talks, principally on astronomical subjects, under the name 'The Stargazer' — wrote in 1929 a book called simply *Enigmas*. It discussed a number of puzzling past events such as the Cry of Memnon, the Landfall of Columbus, and the Canals of Mars, presenting the known facts, quoting the original sources, and then putting forward reasoned conclusions, and I found and still find it fascinating.

Hugh Ross Williamson was the author of a book published in 1974 called *Historical Enigmas*, which dealt in a similarly lucid and logical manner with a number of historical happenings which are either mysterious or commonly misunderstood, such as the death of William Rufus, the disappearance of the Princes in the Tower, and the assassination of Colonel Rainsborough, and this, too, I rated very highly.

Against this background I approached *Astronomical Enigmas* with a keen anticipation, but — and it is perhaps a little unfair to the author to say so — I did not find it as gripping as the books quoted above. The reason is that Mark Kidger's book is not concerned with mysteries in the taut specific way that the others were. It covers a much wider field, and is more a series of general astronomical essays than a critical analysis of conspicuously enigmatic situations.

The twelve chapters cover different sky-related subjects, ranging from Stonehenge *via* star naming, life on Mars, and mass extinctions, to end with a discussion of humans as stardust and an afterword in vigorous support of more manned spaceflight. There is an interesting chapter on the identity of the Star of Bethlehem — the author participated in a *Sky at Night* programme on this subject in 2001, and writes with authority about it, but none of the other chapters relate specifically to any particularly enigmatic astronomical event.

I suppose one could argue that almost everything connected with cosmology is enigmatic in that it is or has been difficult to understand, but on this basis the word becomes useless. Black holes are not enigmatic because they are the logical consequence of gravity; the question of life on Mars is precisely that — a question for solution by investigation, not an enigma; Stonehenge is not itself an enigma, though the purposes motivating its construction and various reconstructions could be considered enigmatic (and, to illustrate the point I am striving to make, Stonehenge would itself become an enigma if one of its component stones was found to be of extra-terrestrial origin). There are, though, many true astronomical enigmas crying out for reasoned discussion: dark matter, dark energy, the cause of the Big Bang, the means by which gravity actually exerts itself, string theory, the anthropic principle, *etc.*; but the author, to my personal disappointment, does not deal with these mysteries.

The book is well produced and is written with enthusiasm in a clear and informal style. It is a book for the layman rather than the already knowledgeable enthusiast, and I noticed one or two minor factual errors, such as the (rather touching) award of a knighthood to George Mallory of Everest fame, radar being derived from "radio location and ranging" rather than the more generally accepted "radio detection and ranging", and the attribution of the tidal theory of the Moon's origin as a spin-off from the Earth to Charles Darwin the naturalist rather than his second son Sir George.

The book has 64 pages of sharp and vivid illustrations, called 'color galleries', which accompany and enhance the interest of the text, and each chapter ends with a short and helpful list of books and internet references for further reading. The main text is followed by 18 pages of notes, and in summary, disregarding semantics, the book provides friendly and easily comprehensible answers to many of the questions people who would like to know more about astronomy will ask. — COLIN COOKE.

Annual Review of Astronomy and Astrophysics, Volume 43, 2005, edited by R. Blandford, G. Burbidge, J. Kormendy & E. van Dishoeck (Annual Reviews, Palo Alto), 2005. Pp. 957, 23·5 × 15·5 cm. Price $86 (individual; about £48), $194 (institutions; about £110); add $5 for overseas orders (hardbound; ISBN 0 824 30943 X).

With more pages even than the enormous *Volume 38* in the series, this substantial tome carries 20 benchmark articles on a wide range of topics; it is equally significant in apparently marking the swan-song of Geoffrey Burbidge after a magnificent 32 years as editor of the *Annual Review*.

As usual, the book begins with an autobiographical chapter, and this year it is the turn of Riccardo Giacconi. Although entitled 'An Education in Astronomy', it is more about large-scale project management and would be useful preparatory reading for anyone wanting to get embroiled in the development of international facilities for astronomy, be they on the ground or space-based.

Turning then to the hot topics in science, we find first a timely, thoughtful, and comprehensive essay on astrobiology, followed by one on Sun-grazing comets; if the late Fred Hoyle, about whom there has been a recent flurry of interest, is right, those two matters may not be unrelated! Of course, for life (as we know it) to flourish, one needs a well-behaved star, and the next paper, on coronal mass ejections, should be studied to see how well the Sun fits the bill.

After a chapter on digital image reconstruction, essential for all those costly new facilities, we head out into deep space with a very useful study of those newly classified stars of type L and T, consolidating what we now know about their physical characteristics on the basis of the 450 so far catalogued. 'Small stars' also feature in the next review, which investigates the population of high-velocity white dwarfs in the Galaxy and what they tell us of the history of the Milky Way. Remaining on the stellar theme, a detailed look at photometric systems should help observers to maximize physical insight from broad- and intermediate-band systems.

Chapter 9 takes us through a valuable update on the structure of the Galaxy's interstellar medium, employing the full spectral range of observations, after which we turn back to stellar astronomy with, firstly, a user-orientated review of the application of stellar-evolution models — which have continued to grow in sophistication — to the interpretation of colour-magnitude diagrams, and secondly, an up-to-date account of AGB evolution (mixing is still a problem). Still on the stellar track, just when the (extragalactic) 'thundering herd' had thought that stars were 'done and dusted', we find that abundance analyses may not be quite as reliable as we thought, especially for metal-poor stars; one can only hope that the authors of the following chapter on very metal-poor stars in the Galaxy had a preview of that work!

Onward and outward, we now come to a masterly review of galaxy classification, from the earliest times through to the latest developments, stressing that morphology should be the guiding principle. Mega-masers form the subject of the next chapter, which, with the ensuing review of molecular gas at high redshift, give hope to exciting prospects in cosmology from up-coming projects like *ALMA*. Continuing out in the long-wavelength régime, we are then treated to a study of the cosmic infrared background, much of which is generated by dusty high-redshift galaxies. These objects are clearly the location for vigorous star formation, and the consequences of such activity is revealed in the review of galactic winds which follows; obviously we must reckon on the possibility of intergalactic metal pollution. Deep X-ray surveys are the concern of the penultimate chapter, based on new results from *Chandra* and *XMM–Newton*. And finally we come back to the near-primordial ISM and IGM with a discussion of results on damped-Lyα systems.

Make sure your library takes a copy — or has on-line access. — DAVID STICKLAND.

Women in Space — Following Valentina, by D. J. Shayler & I. Moule (Springer, Heidelberg), 2005. Pp. 410, 24 × 17 cm. Price £22·50/$39·95/€32·95 (paperback; ISBN 1 852 33744 3).

The purpose of this book is to celebrate the achievements of all the women who have taken part, in a multitude of ways, in missions of space exploration, and to chronicle those missions. It begins by examining the histories of the numerous daring females who attempted flight in various forms — balloon, parachute, glider, biplane, supersonic jet, combat aircraft — before then focussing solely on space missions. To illustrate women's long fascination with celestial exploits it commences with a short history of women in astronomy, before going on to study in more detail the developments of aviation and the fortunes (or fates) of the somewhat persistent women who took part whenever they could. In the pioneering days most of the action seems to have been in France, or involving French women, whereas the history of space missions is divided — somewhat unequally — between the Soviet Union and the USA.

The chronicle from early aviation to the Girl Astronaut Programme is a splendid narrative packed with detail and fascinating snippets of daredevilry, the prize for which must surely go to American Myrtle Cagyle, who was taught by her brother to fly at the age of 12, and when somewhat younger had demonstrated her keenness by jumping off the roof of their house using a pillowcase as a 'parachute'. There were a surprisingly large number of early female aviators — probably well outnumbering women in astronomy. The odds against their participation were loaded, not only with practicalities such as physical strength but also with cultural prejudices, which the book discusses but perhaps not as thoroughly or contextually as it might. The story of Valentina Tereshkova and her epic space flight in 1963 as the first woman (and the tenth human) in Earth orbit is told with a disarming frankness, and includes material only recently unearthed from archives. From then on the competitive nature of the superpowers' race into space is undisguised, though prestige always took second place to scientific need, safety, and readiness.

The book examines the contemporary and subsequent developments of space exploration in fairly minute detail. While there is a focus on the parts played by women, not only as front-line crew-members but also in a wide range of ground-based tasks such as communicators, dieticians, or designers and fabricators of space-suits, the account is so comprehensive that it constitutes a fairly thorough history of modern space exploration. For each of the 40 or so women who have flown in space the book provides a biography, often with a detailed map of the route through the selection stages, and a list of the duties specific to each flight. For any student of the subject here is the reference book *par excellence*, with ample cross-references to relevant literature, reports, and similar documents.

However, it is also at the commencement of its accounts of space exploration (*i.e.*, women actually in space) that the book becomes less readable. Because of a desire to include every scrap of information, whether fully pertinent or not, the earlier narrative style is lost and we are bombarded, increasingly, with lists of details, acronyms (in one place as many as 13 in a row) and in-house terms which do not necessarily appear in the glossary, and are hard for someone unfamiliar with the topic to remember. One of the stated purposes of the book is to record which missions were flown by female crew-members, how much work they carried out on each mission, and their numerous responsibilities. That is indeed achieved, but at the risk of turning a popular-looking book about space flight into an encyclopaedia. While the product is clearly the result of a formidable amount of research, the information could have been organized into a more easily assimilated form. The flights are dealt with in chronological order, with the biographies of the individual women inserted at first mention and thus rather difficult to refer back to, while the duties of each female crew-member become tedious to read and somewhat repetitive. It would have been more practical to design a table of the duties, and to present the biographies in alphabetical order in an Annexe.

The (mainly US) women who have flown in space repeatedly insisted that they were selected for their ability and not their gender. Several stressed that they preferred not to be known as "women in space", and to that extent this book somewhat betrays that wish. In seeking nevertheless to celebrate women's achievements in space flight, it could have done so more subtly — and made a more readable product — by discussing (for example) changing attitudes to women in traditionally male rôles as the culture caught up with developments. While the biographies are textual CVs and mostly avoid mentioning their pedigrees, in one case the woman's father was a Vice-Admiral and she is described as having "an impressive family background" — an unfortunate and uncalled-for remark that reveals a flash of chauvinism which is surprising, given the book's objectives. (No mention is made of the mere but highly influential research-scientist mothers that some of those women probably had.) The remark is the more unwanted in that the book has a strong mission to encourage aspiring young women to pursue careers in science and technology *notwithstanding* — and possibly in spite of — family backgrounds, be they illustrious or commonplace.

Although the book is well written (marred only by a slightly irritating mis-use of the comma) it could have been better edited. Occasionally a long description is followed by a paragraph summarizing, or rather repeating, much of what has just been said, giving the impression that the authors tossed in both of their own research write-ups for good measure, or that a section author was not fully *au fait* with the content of adjacent material. There are also a large number of typing errors: mis-words (that would not have been detected by a 'spell-checker'), wrong spelling, and wrong pagination in the index; in places the frequency of mistakes averaged nearly one per page.

This book is not written by astronomers — one Frenchwoman is mentioned as achieving prominence in the "UAI", which is then dutifully spelled out in French as indeed it appears on her web page — nor does it maintain much of a focus on astronomy. Nevertheless, the topic is sufficiently close to our field that many astronomers, particularly those with a desire to set the facts straight in their own minds at least, will find this a most helpful and surely unique addition to their libraries, and extremely good value for money. — ELIZABETH GRIFFIN.

Cosmic Abundances as Records of Stellar Evolution and Nucleosynthesis in Honor of David L. Lambert (ASP Conference Series, Vol. 336), edited by T. G. Barnes III & F. N. Bash (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 378, 23·5 × 15·5 cm. Price $77 (about £44) (hardbound; ISBN 1 583 81201 6).

David Lambert is the world leader in the determination of stellar abundances and in their interpretation from the point of view of stellar evolution and nuclear astrophysics. In 1987 he was awarded the Dannie Heinemann Prize of the American Institute of Physics and the AAS for "establishing a new standard of precision for the quantitative analysis of stellar spectra", and with over 300 publications and a similar number of collaborators world-wide he is one of the most cited authors in astrophysics. The book under review records the proceedings of a conference in honour of his 65th birthday organized by students and colleagues at his home institute — the University of Texas at Austin — in 2004.

The contributors come from a wide range of astrophysical specialties, reflecting the breadth and significance of their hero's own work; not only stellar and solar spectroscopists appear among them but also leading figures in stellar-evolution modelling (such as Arnett, Charbonnel, Lattanzio, Maeder, and J. C. Wheeler), in stellar atmospheric modelling (such as U. G. Jørgensen), and in nuclear astrophysics (such as Käppeler).

The proceedings open with an entertaining biographical account by Nick Grevesse, and conclude with some thoughtful remarks by Lambert himself in which he surveys the prospects for new observational and modelling techniques. There is a new review and table of solar abundances by Asplund, Grevesse & Sauval, much discussion on effects of rotation and other candidates for 'extra-mixing' on stellar evolution and yields, and extensive coverage of abundances in most kinds of stellar object that Lambert has studied, which is nearly every kind of stellar object one can think of: metal-poor stars, M-dwarfs, hot stars, giants, horizontal-branch stars, AGB stars, interacting and non-interacting binaries, R CrB and extreme-helium stars, the Sgr dwarf spheroidal galaxy, supernovae, *etc.*, *etc.*, as well as discussions of light-element abundances, including the primordial lithium problem and molecular and nuclear data.

The book is thus a very useful survey of current trends in stellar abundances and their astrophysical significance, as well as being a worthy tribute to David Lambert himself. — BERNARD PAGEL.

Uncertain Science ... Uncertain World, by H. N. Pollack (Cambridge University Press), 2005. Pp. 243, 23 × 15 cm. Price £19·99 (paperback; ISBN 0 521 61910 6).

Presenting a discussion of how we interpret an uncertain world and why science is founded upon and driven by this uncertainty would be a noble task at any time. Today, more than ever, it would be better viewed as an essential social service and a duty that governments ought increasingly to take upon themselves rather than sticking with their more familiar flirtations with statistics designed either singly or multiply to cajole, confuse, frighten, or occasionally comfort and inform their citizens. Pollack has, laudably, taken the task upon himself in this book. Based upon his personal research interests, the book is woven around the theme of climate change, which is used to illustrate practical examples and the limitations of uncertainty in data analysis as well as forward and backward modelling. Uncertainty is shown to be an unavoidable feature of our interpretation and understanding of the world rather than merely a sign of our ignorance or reluctance to make firm decisions. Occasional personal anecdotes and swipes at multi-national and international vested interests might produce a smile or a wince, but either way they punctuate the text with welcome bursts of human interest.

Admirable as the aims of the book are, I have some difficulty, nay uncertainty, in envisaging for which readership it was intended and, much more importantly, to which readership it will actually reach and make a difference. Practising scientists and teachers would do well to peruse the book, if not as a refresher for themselves, to provide pointers to ways of explaining and clarifying the concepts to non-professional audiences. The book's content is pitched at a level where any scientifically-literate reader would mostly be able to respond with a knowing nod, but would rarely, I suspect, have any moments of true enlightenment. However, there are times when I felt the discussion could have gone further in an attempt to clarify the argument or to dispel misconceptions or quell misgivings, which must be lurking in some readers' minds. For instance, when stating that the probability of global warming being real (as opposed to a data-analysis artefact) is >99%, or in other words a 'virtual certainty', we are dismissively told that any denial of its reality could only come from the "climatological equivalents of the Flat Earth Society". This seems a little incongruous since even Pollack would presumably admit that the probability of the Earth being flat is identically zero as opposed to <1% (or 'extremely unlikely' in his terminology). So in that case what is wrong with reversing his argument to suggest that global warming should be assessed as simply 'true', not fudged around with a little safety net of uncertainty? And yet we know from the rest of the text that that is simply not possible — discuss!

A large group which would stand to gain a great deal (if one is allowed such Orwellian flights of fancy) from discussions and insight into the meaning, relevance, and consequences of uncertainty in the world comprises those poor souls who are cajoled, confused, or frightened by uncertainty (or risk as it is often perceived) in the guise of statistics or probabilities in their everyday lives — in other words virtually all of us. The trouble is that while this book might well help us appreciate the theoretical concepts and practical statistics of life's inherent uncertainties, it does nothing to help resolve, or even come to terms with, some very real conflicts or apparent contradictions we encounter daily. I had hoped it would delve much more (and so be more at home on the self-help bookshelves than those for popular science) into the ways in which we deal, often unconcernedly, with a great deal of uncertainty in our lives. In which case, it would then be fascinating to try and understand why we are so often pulled up short when some choice is to be made based upon only a knowledge of the raw probabilities of the possible outcomes, be it trivial or of global consequence. One only has to contemplate the confusion and conflicts generated by probabilities surrounding the national lottery, road-traffic accidents, crime, smoking, nuclear power, MMR jabs, GM organisms, and so on to appreciate what a productive area that could be. Maybe Pollack will take up that challenge in a sequel. — Dave Pike.

Who Built the Moon?, by C. Knight & A. Butler (Watkins Publishing, Winchester), 2005. Pp. 262, 24 × 16 cm. Price £16·99 (hardbound; ISBN 1 842 93132 6).

The authors of this book pose an intriguing question, but their suggested answer is far from convincing. Now, I would be the first to admit that the 2006 'official' lunar-origin hypothesis is far from being 100% water-tight. The suggestion that the Earth was struck a glancing blow by a single Mars-sized asteroid, conveniently just after core differentiation, and that just over one percent of the mantle was detached to form our impressive satellite, has many niggling problems. But where would science be if there were no problems to solve?

You cannot accuse Knight & Butler of being minimalists (or planetary scientists). Occam's Razor is ignored. When confronted with a list of facts such as today the Moon is 400 times smaller than the Sun, and 400 times closer, and that the product of the lunar circumference and its inverse spin period is 400 km day^{-1}, Knight and Butler draw but one 'obvious' conclusion. They are convinced that these dimensions and movements are solely designed to alert us to the fact that the Moon is not a natural body. They suggest that it was manufactured by skilled engineers of the future whose construction brief forced them to travel back in time to the dawn of the Solar System. Few details are given as to the actual mode of construction but the purpose was to stabilize the

Earth's spin speed and precession axis, so that life could develop on our planet. The Moon's message is not only that "no Moon means no humans" but also that an alien future exists.

Much is made of the megalithic yard, the Sumerian double kush, the secrets of the genome, lunar mascons, the Möbius Principle, and the message of *2001: A Space Odyssey*.

I found the whole book deeply unconvincing. I am not changing my lecture notes. Only read *Who Built The Moon?* if you want to be amazed by how blatantly some can disregard scientific methodology. — DAVID W. HUGHES.

Annual Review of Earth and Planetary Sciences, Vol. 33, 2005, edited by R. Jeanloz, A. L. Albee & K. C. Burke (Annual Reviews Inc., Palo Alto, CA), 2005. Pp. 710, 23·5 × 15·5 cm. Price: institutions $200 (about £110), individual $88 (about £48); add $5 for orders outside the USA (hardbound: ISBN 0 824 32033 6).

Each new *Annual Review of Earth and Planetary Sciences* is a box of intellectual chocolates, all offered by recognized experts in their fields. The 2005 edition is no exception, as eclectic as ever: where else would one find feathered dinosaurs nesting with Martian crust, or a discussion of Antarctica's great sub-glacial lakes bound together with thoughts on the interior of the giant planets?

What to pick out? The choice is wide. Most enthusiasms are catered for — a score of papers, each one of which would be an excellent basis to set before an advanced undergraduate as the subject of a term paper.

Geophysics dominates the volume. The range is very broad. Planetologists have good essays on Mars (Nimmo and Tanaka), the interiors of the giant planets (Guillot), and the origin of the Earth–Moon system (Jacobsen). Perhaps in part more in hope than review, there is a discussion of planetary seismology (Lognonné): good information from the Moon, wind on Mars, and the promise one future day of sounding out Europa's ocean.

On solid Earth the diet is excellent, a crust served up with cheese (Sleep's masterly analysis of lithospheric evolution, and Silverstone's comparison of Alpine tectonics with a slice of maturing, collapsing Brie). This is a volume that can stop a train — Kanamori's discussion of real-time seismology, for example, using P-wave detection to warn Japanese bullet trains to slow and stop before serious earthquake waves arrive. Sengör *et al.* provide a very valuable analysis of the tectonics behind what may be the next great European earthquake, near Istanbul, Europe's largest city. Volcanic seismology is also covered (McNutt).

Glaciologists and climatologists are well served. There are excellent discussions of sub-ice processes by both Clarke and Siegert. For the meteorologists, a team from the European Centre for Medium-Range Weather Forecasts (Palmer *et al.*) discuss model uncertainties in weather and climate prediction, Stevens reviews atmospheric moist convection, and Roe discusses orographic rainfall, a subject where sedimentology and meteorology have common interests.

For the palaeontologist there is plenty of choice: papers on feathered dinosaurs, reptile evolution, Precambrian animals, microbial nitrogen-cycle evolution, and the rise of oxygen in Earth's atmosphere.

What use is a volume like this? First, and most important, it is valuable for both specialist researchers and non-specialist readers. *Annual Review* editors have a track record of identifying subjects where good science is being done, and at any time a running average of the last, say, five years of *Annual Reviews* will provide snapshots of most of the 'hot' areas in Earth and planetary sciences. Secondly, they are superb fodder for the third- and fourth-year undergraduate essays: what better way to go about Geol 400 'Current Topics' than by setting a class of students to "Summarize and Discuss" one paper each from this volume? — MARY FOWLER.

Eclipses 2005–2017, by W. Held (Floris Books, Edinburgh), 2005. Pp. 191, 20 × 15 cm. Price £9·99 (paperback; ISBN 0 863 15478 6).

An eclipse of the Sun, whether it is annular or total, is a spectacular sight. Even a deep partial eclipse can capture the attention of the public. Such was the case of the annular eclipse of 2005 October 3. Although around 60% partial in the United Kingdom, the path of annularity ran diagonally across the Iberian Peninsula. Shortly before my departure for Madrid and a location close to the central line of the eclipse, this book arrived in timely fashion for review.

Eclipses 2005–2017 by Wolfgang Held is aimed at the eclipse watcher willing to travel to catch those precious few minutes of totality or annularity each year. The author has concentrated on providing maps of the eclipse paths as well as timing information, weather information, and sky diagrams for the eclipses themselves. The solar-eclipse maps are excellent. The book even comes with a pair of eclipse viewers! However, it does not restrict itself to eclipses of the Sun: lunar eclipses are included, although the information is somewhat less detailed. The section on unusual astronomical events is an interesting inclusion. These events include groupings of bright planets with and without the Moon (an area not well catered for in other publications), Saturn's passage through Praesepe in 2005/6, and the transit of Venus in 2012.

The explanatory material in the book is not particularly extensive and is accompanied by personal accounts of eclipses. Sadly, there are a small number of niggling errors in the text. These include the transposition of the distances for apogee and perigee for the Moon on page 24, and incorrect designations for constellations, *e.g.*, Cancerii. However, the book is probably worth its modest price tag for the maps alone and we can probably overlook the minor errors.

The weather treated the eclipse watchers well in Madrid and everyone saw a beautiful annular eclipse and the eclipse timings in the book were accurate. Sadly, observers back in the United Kingdom were not so lucky. They were clouded out for the most part. Perhaps they should take a look at Held's book and use it to plan their next eclipse expedition to sunnier climes. To paraphrase my PhD supervisor's checklist before going on an observing run, all you need are tickets, passport, money, and this book. — STEVE BELL.

Philip's Astronomy Dictionary: An Illustrated A–Z Guide to the Universe, New Edition, edited by J. Woodruff (Philip's, London), Pp. 256, 20 × 12·5 cm. Price £6·99 (paperback; ISBN 0 540 08689 4).

This new, pocket-sized edition of the well-known astronomy dictionary is a welcome addition to the library of any amateur astronomer. It comes fully illustrated in colour with more than 200 excellently produced diagrams and more than 1000 brief but well-informed articles. Undergraduates will also find the dictionary of use as a quick reference to an object or topic of interest.

As well as informed references on all kinds of topics, there are many brief biographies of the leading players who have shaped the history of astronomy. I was disappointed nevertheless not to find a reference to Cecilia Payne (later

Payne-Gaposchkin), who in 1925 discovered the overwhelming abundance of hydrogen in stars. Henry Norris Russell initially rejected Payne's conclusion but came to the same conclusion himself four years later. He should not, however, receive all the credit. Otto Struve (who *does* get a mention) once wrote that her thesis, *Stellar Atmospheres*, was the most brilliant astronomy thesis ever written. Another notable absentee is Harvard College astronomer Antonia Maury, who first uncovered the difference in spectral lines of dwarfs and giants. I was also a little disappointed to see no reference to the very important group of ζ Aurigae stars, the subject of my own thesis!

There are very few errors to be found in the book, although Guillermo Haro's first name has been misspelled on page 102. One criticism I would make of the book is the inconsistent use of units, instead of sticking to the IAU recommended SI units. On page 153, for example, we are told that the diameter of the Moon is 3476 km and has a density of $3 \cdot 34$ g/cm^3. On page 230 we learn that the temperature at the centre of the Sun is 15×10^6 K, with a pressure of 10^{11} bar. The pressure here should have been given in Pascals, the standard SI unit. Although it is true that most researchers use cgs units, many readers of this book will only have learned SI units.

My criticisms of this dictionary are indeed very minor and I can wholeheartedly recommend it to all those interested in a quick but reliable reference guide. — KEVIN MARSHALL.

From Blue Moons to Black Holes: A Basic Guide to Astronomy, Outer Space, and Space Exploration, by M. M. Knocke (Prometheus Books, Amherst, NY), 2005. Pp. 313, $27 \cdot 5 \times 21 \cdot 5$ cm. Price $19 (about £10) (paperback; ISBN 1 591 02288 6).

If you want a book which provides quick, concise answers to the most commonly asked questions in astronomy, and a well laid out, accessible, reference source on all things astronomical, then look no further. This is the one for you. Melton Knocke has used her experience in public education at several US observatories to home in on the questions that people most often ask about the Sun, planets, moons, and the wider Universe. She then seeks to provide simple, easy-to-understand answers to those questions that bother both the academic on the research frontier and the curious kid at the back of the classroom.

The book is divided into three sections. The first section, titled 'Questions and Answers', deals with the most-common questions people have regarding astronomy, outer space, and space exploration. Melton Knocke gives short, sharp answers to the questions (for example: "Q: Is Saturn the only planet with rings ? A: No"), and then goes on to explain her answer in a few clear and concise paragraphs (in this case the rôle of *Voyager 2* in understanding the rings around the other gas-giant planets). This is all done in a very engaging way, supplemented where necessary with photographs and diagrams to aid understanding.

The second section, 'Quick Facts', provides basic information about the Solar System, nearby stars, and constellations, in an easy-to-access format. It forms a sort of astronomical directory, allowing the reader to dip in and find facts quickly. All the facts are cross-referenced to recent research papers and there is a helpful list of websites to check the very latest information emerging from the space agencies and the research world. There is also a guide to forthcoming lunar and solar eclipses and meteor showers.

The third and final section provides 'A Brief History of Lunar and Planetary Exploration'. This does not deal with Earth-orbital space flights, but provides a comprehensive account of missions to the Moon, Mercury, Venus, Mars, and the outer planets, culminating in the remarkable twelve-year *Voyager* fly-by journey to Jupiter, Saturn, Uranus, and Neptune, which did so much to enhance knowledge of our Solar System. Pluto (if it is a planet, see Q&A in section 1) remains the only planet yet to receive a visit from us. Melton Knocke explains why an early visit is advisable in view of Pluto's outward drift from the Sun, having approached its closest point back in 1989.

This book is an excellent compendium of information, which will appeal to the interested student, the enthusiastic amateur, and the professional looking to improve communication with a wider audience. As a physics teacher, I will want to keep a copy ready to hand in the lab, both for myself and my students. It will provide a valuable source of information for topics on the Solar System (National Curriculum, Key Stages 3 & 4, 'The Earth & Beyond'). It would not be suitable as a course-work book, however, as it is too lengthy and does not cover the relationship between gravitational forces and the movement of the planets. — VIVIENNE FROST.

Visual Astronomy under Dark Skies: A New Approach to Observing Deep Space, by A. Cooke (Springer, Heidelberg), 2005. Pp. 214, 23·5 × 15·5 cm. Price £24·95/$39·95/€39·95 (paperback; ISBN 1 852 33901 2).

This book is a follow-up to the author's original *Visual Astronomy in the Suburbs*, and from the off, the title is a misnomer as in fact it does not deal with traditional visual astronomy but relates purely to the use of image intensifiers in place of an eyepiece. A better title might be 'Real-Time Observing of Deep-Space Objects'. The author complains that users of such devices are cold-shouldered by the amateur establishment and that the devices do not get the use/respect they deserve. This could perhaps be more to do with both their price (over $2000 US) and the fact that US-government paranoia will not let such devices be bought by anyone but US citizens resident in the US.

The book covers in some detail what needs to be taken to a dark-sky site and the perils of forgetting things. This of course applies to any form of observing but particularly when one is using so much electronic equipment that a generator may almost be required. The author also discusses the use of video cameras as a method of recording what is seen through the image intensifier. I am therefore confused a little as to the target readership as this could hardly be defined as 'visual' astronomy.

The main thrust of the book is to discuss what types of objects respond best to the use of the image intensifier, as well as what they look like. The author details the many sorts of deep-space object as well as providing images of them. Here the book falls down badly because, assuming the reproductions are faithful examples of what objects look like, you can see more with the naked eye through the size of telescope that was used to take the images. One can only suspect that, as is common in this Springer series, the images are poorly reproduced and do not give a true indication as to what the devices really show, otherwise there would be little point in getting one.

The author concludes the book with his opinion on the current state of hardware in amateur astronomy and possible future developments. This is a book with a very limited market, and whilst export restrictions for advanced technology remain in place it will continue to be so. — OWEN BRAZELL.

Astronomy Hacks: Tips and Tools for Observing the Night Sky by R. B. & B. F. Thompson (O'Reilly, Farnham), 2005. Pp. 388, 23 × 15 cm. Price £17·50 (paperback; ISBN 0 596 10060 4).

According to these authors, 'hacks' are ways of solving problems and getting things done. The book, aimed mainly (though not exclusively) at beginners, contains 65 hacks in 378 pages, so each one is either fairly detailed or a collection of small points. In theory each can be read alone but, even though there are cross-references, I do not believe a beginner would always pick up enough for this to be adequate. The addition of a glossary would improve the situation and would also remove some of the irritating repetition. Names are certainly given when equipment is reviewed but as the index does not reference this material it is not as useful as it might be. A lot of the pictures did not seem to me to be of much value — telescopes do not photograph well in the dark; but the diagrams and tables were often useful. The book also has many references to web sites.

Mostly the book is easy to read (although some small features did annoy, such as the use of 'Luna' for the Moon and frequent use of 'Dobsonian' as a telescope rather than a mount), but it is aimed at an American readership. This does not matter much generally (units are usually metric) but some of the advice given does not cross the Atlantic well. Disabling your car lights (Hack #43) may be acceptable on private ground but there is not much of that in this country and driving without fuses on public roads would not be ideal. Even more worrying is the advice in Hack #3 recommending a ·22 revolver or 12-gauge riot shotgun for safety at remote sites!

One thing this book seems to be extremely good at is generating debate. Lecturer not arrived yet? Open this book, read out a hack, and stand back. I am sure any beginners would learn a lot from the ensuing discussion, both about astronomy and their fellow astronomers! — RITA WHITING.

THESIS ABSTRACTS

BINARY STARS: POPULATIONS AND EVOLUTION

By James Fisher

This thesis is concerned with populations of binary stars and their evolution. Specifically we study a sample of spectroscopic binaries (SBs) in the local solar neighbourhood within a distance of 100 pc and to an absolute magnitude $M_V \leqslant 4$ and examine what the statistics of such a sample can tell us about the population of binary stars as a whole. To do this we use parallax data from the *Hipparcos* main catalogue correlated with SB data taken from the Batten catalogue (8th edition) supplemented with additional SB data, published and unpublished, of Roger F. Griffin. While a complete *volume-limited* sample is not reasonably possible with the data available, we believe that the sample presented and analysed here is probably the closest one can get to it at present. Previous studies are discussed and compared with our own.

We give various analyses of this sample, deriving distributions of period, mass-ratio (q), primary-mass, orbital semi-amplitude, semi-major axis, and eccentricity as well as its initial mass function (IMF). To determine the mass-ratio distribution of single-lined SBs we also use our own refined Monte-Carlo procedure. We find, by two different selection criteria, a clear peak at $q \sim 1$ with no unambiguous evidence for a second lower peak at $q = 0{\cdot}2$ to $0{\cdot}3$. These results are robust to all but the most extreme weightings of double to single-lined SBs. We also find that the q distribution cannot be reproduced by simulations that take stars at random from our own or others' suggested IMFs.

Aspects of how binary-star statistics relate to mechanisms of binary formation and, through binary evolution, to re-injection of material to the interstellar medium and hence to galactic evolution are also discussed. From the period distribution we also use a stellar-evolution code to point out a possible resolution of the shift in the observed white-dwarf-mass distribution from that predicted by single-star stellar-synthesis models. — *University of Sussex; accepted 2005 September.*

Satellites and Substructure in the Local Group

By Alan McConnachie

The internal structure and spatial distribution of a large number of Local Group galaxies are analysed. The majority of these systems are members of the M31 subgroup. Comparison of their properties with results for the Galactic subgroup is used to probe the representative nature of the populations and the effect of environment on various aspects of galactic evolution. This reveals, for the first time, important differences among the subgroups and individual systems, and highlights general trends in the properties of the Local Group population as a whole.

I begin by revisiting the fundamental problem of distance determination in the Local Group, and develop a procedure to optimize the tip of the red-giant branch as a distance indicator and explore the colour of the red-giant branch as a metallicity indicator. These techniques are applied to 17 Local Group galaxies, creating a large, homogeneous, distance-and-metallicity dataset. This enables distance accuracies of ~ 5 %, and the consistent analysis ensures that differential systematic uncertainties are minimized, simplifying comparative studies of these galaxies.

These techniques are then applied to the copious amount of substructure identified in M31, in particular quantifying the three-dimensional structure of the giant stellar stream to the south-west of M31. Star counts across and along the stream probe its stellar structure and provide tentative evidence for the location of a progenitor. The stream extends more than 100 kpc behind M31, before wrapping around the centre of the galaxy. A similarity in the colour of the stellar populations suggests that the stream then emerges as another prominent substructure to the north-east of M31. I also discover a second tidal stream in M31, emanating from the dwarf elliptical companion NGC 205. This feature is > 15 kpc in projection and I am able to identify its kinematic signature. These findings are a key component for numerical models that attempt to constrain the accretion history and mass distribution of M31.

I then re-derive membership of the M31 subgroup from first principles, and compare its spatial distribution to the Galactic subgroup. This latter system is more centrally concentrated than the M31 population, and comparison of the distributions relative to the galactic discs suggest that the Galactic population is somewhat incomplete at low latitudes. Several candidate streams of satellites are found around M31, in analogy to the Galactic system. Whether these streams represent a physical association or not cannot be addressed with the currently available data. In addition, I find that the M31 satellite system is displaced from its host towards the Galaxy, at > 3σ confidence. This warns against treating the M31 subgroup as complete/unbiassed/relaxed, and necessitates detailed modelling to determine the cause of this offset.

Finally, the structural properties of the M31 and Cetus dwarf spheroidal galaxies are derived, revealing the influence of tides and multiple-structural components in these systems. Comparison of the Galactic and M31 dwarf spheroidal satellites shows that, at all absolute magnitudes, the M31 population has significantly larger scale-radii than the Galactic population. In addition, both populations show the same correlation between galactocentric separation and central surface brightness: dwarfs further from their hosts are brighter. These findings suggest that environmental factors associated with the host galaxy influence the formation and subsequent structural evolution of dwarf spheroidal satellites. I outline possible physical mechanisms which could explain these results. — *University of Cambridge; accepted 2005 July*.

A full copy of this thesis can be requested from: alan@uvic.ca.

The Observed Nature of the Progenitors of Core-Collapse Supernovae

By Justyn R. Maund

Theoretical models of stellar and supernova (SN) evolution predict the nature of the progenitors of SNe prior to explosion. The observational identification of the progenitors is rare, and tests of these models are therefore difficult. The direct study of the progenitors in pre-explosion images is the main focus of this thesis. The correct identification of the progenitor locations is permitted by high-resolution, post-explosion, *Hubble Space Telescope* (*HST*) imaging of the SNe and the technique of differential astrometry.

Spectroscopy of SN 1993J, approximately ten years post-explosion, is presented. Superimposed on the SN spectrum are the narrow absorption-line features due to a hot-star component, spatially unresolved from the SN in high-resolution *HST* images. This hot-star companion explains the ultraviolet excess of the progenitor's spectral-energy distribution and the high rate of mass loss of the progenitor prior to explosion.

The progenitors of two type-IIP SNe, 2003gd and 2005cs, are identified in *HST* pre-explosion images. The progenitors are red supergiants, matching the canonical predictions of stellar-evolution theory for the progenitors of this particular type of SN. These progenitors are found to have arisen from particularly low-mass stars, around the minimum mass threshold for stars to end their lives as core-collapse supernovae (CCSNe) and at the boundary for the collapsing stellar core to be either oxygen–neon–magnesium or iron.

Mass limits are placed on the progenitors of seven CCSNe: 1999an, 1999br, 1999ev, 2000ds, 2000ew, 2001B, and 2004gt. High-resolution, post-explosion images are used to exclude nearby stars from being misidentified. The progenitor of the type-II SN 1999ev is significantly detected on pre-explosion images, implying an initial mass for the star of 15–18 $M_\odot$. Mass limits for the peculiarly faint SN 1999br show it to have arisen from a low-mass progenitor, rather than a massive star forming a black hole at its centre. The limits on the progenitors of the type-I SNe in the sample do not permit differentiation between massive, single, Wolf-Rayet-star progenitors and low-mass He and C+O stars in binaries.

Pre- and post-discovery observations are presented of two faint, low-velocity SNe: 2002kg and 2003gm. SN 2002kg is shown, on the basis of its post-discovery photometric and spectroscopic behaviour, to be a Luminous Blue Variable (LBV). This finding is consistent with other studies which have shown SN 2002kg to be consistent with the previously identified variable NGC 2403–V37. SN 2003gm appears similar to SN 1997bs, although there is no conclusive evidence that either is an LBV. The strong [N II] lines observed in SN 2002kg are suggested as a possible indicator of the LBV nature of some faint, low-velocity SNe.

The possibilities for improving the scope of research in this field are also discussed: through the expansion of archival imaging programmes of nearby galaxies, in anticipation of future SNe, and the use of ground-based imaging with adaptive optics to provide accurate progenitor locations once the *HST* reaches the end of its life. — *University of Cambridge; accepted 2005 September.*

OBITUARY

Willem Wamsteker (1942–2005)

It is with great sadness that we learned of the death on 2005 November 24 of Willem Wamsteker just a day after his 63rd birthday. Many members of the UK astronomical community, and especially those with an interest in ultraviolet astronomy, will have known Willem, who led the ESA support team for the *International Ultraviolet Explorer* (*IUE*) project at the Villafranca del Castillo tracking station from the early 1980s, through the termination of real-time operations in 1996, and on to the successful conclusion of the archive phase just a few years ago.

Willem studied astronomy first at Leiden, moving then to Arizona to work on planets with G. P. Kuiper before returning to the Netherlands to complete a thesis, also on planets, under H. C. van der Hulst. In 1975 he joined the European Southern Observatory in Chile as a Staff Astronomer, where he developed an expertise in infrared astronomy but also clearly gained experience of how to support astronomers at a major facility. This latter skill was to be invaluable in his long association with the *IUE* project: before the development of queue scheduling of observations, *IUE* was operated much like a traditional ground-based telescope, where the observer had to be present to plan and execute the observations. Willem always took a close interest in the programmes of these visiting astronomers and was a great encouragement, particularly to younger astronomers, in developing new ideas and ways of exploiting the capabilities of *IUE*. For the same reason, DS remembers many a drawn-out 'Three-Agency Meeting' (UK–NASA–ESA) at which Willem would fiercely defend some point or proposal he felt would be beneficial to the user community!

However, Willem's work on *IUE* did not finish with the end of the mission. He had always been a strong supporter of the creation of a uniform data archive for *IUE* and brought the project to an effective conclusion through the *IUE New Extracted Spectra System* (*INES*), the archive system set up in cooperation with the Spanish institute Laboratorio de Astrofisica Espacial y Fisica Fundamental (LAEFF).

Following that, Willem was appointed as Multi-Disciplinary Scientist at ESAC, where, perceiving the likely gap in the provision of UV facilities which now faces the astronomical community, he became a key proponent of the *World Space Observatory* (*WSO*), a project which was particularly close to his heart and in which he remained active to the end. An important element of the *WSO* project is the provision of access to space astronomy for a much wider constituency of countries than has hitherto been the case, including developing nations. This idea grew out of Willem's contribution to the ESA/UN series of workshops, promoting space in parts of the world not normally associated with such activities. This was much appreciated and earned him wide international respect. If realized, the *WSO* would be a fine memorial.

Willem's enthusiasm was infectious and it was invariably stimulating to work with him. He always made time for his colleagues and often became something of a father figure to younger generations of scientists. He will be sadly missed by his family and many colleagues around the world. — DAVE STICKLAND & MARTIN BARSTOW.

EX-EDITORIAL

DR. DAVID STICKLAND'S EDITORIAL LONGEVITY

By R. F. Griffin

I have managed to steal this space (although only after twisting the Managing Editor's arm about one radian beyond its normal range of movement) in order to draw attention to Dr. Stickland's unique service to this *Magazine*, and through it to the astronomical public. This year, Dr. Stickland becomes certainly the longest-serving Editor that the *Magazine* has ever had in its 129-year history. It is appropriate for me to draw attention to that fact, since the record has previously belonged to me, possibly tied with Thomas Lewis, who was in office a hundred years ago. I was Editor in 1963–85, which by an ordinary subtraction sum might be considered to be 22 years, but if careful research were to show that I was in post throughout 1963 and 1985 it could be 23. Lewis survived two periods of Editorship, 1885–87 and 1893–1912, which might be 2 + 19 years, or might at most be 3 + 20 and thus 23. Whichever way one cares to reckon, Dr. Stickland, who has been Editor since 1983, has embarked this year upon previously uncharted waters. I can only apologize for not having recognized that fact in time for this 'Ex-editorial' to appear in the February issue of the *Magazine* — it was only the arrival with the December issue of the *Annual Index* with its chronological listing of Editors that drew the matter to my attention.

The Editors of this *Magazine* act in a purely voluntary and thus altruistic capacity. They also have responsibilities (and, correspondingly, freedoms) beyond those undertaken and/or enjoyed by those in charge of most other journals, who are beholden to societies or commercial owners and sometimes have to do as they are told. The Editors of *The Observatory* actually *own* the *Magazine* and are answerable to none but themselves; they personally see not just to editorial matters but also financial ones. That can be no small responsibility. In particular, during Dr. Stickland's incumbency, the RAS might have pulled the rug out from under the *Magazine* by discontinuing the block subscription that it had long paid to enable it to distribute *The Observatory* to all Fellows as part of the benefits of Fellowship. The *Magazine* did not merely survive, but actually continued (and still continues) to carry reports of all RAS Astronomy & Geophysics meetings, thus coping with what ought to have appeared as a dramatic, possibly fatal, reverse with such studied nonchalance that it could somehow afford also to turn the other cheek!

Dr. Stickland not only corresponds with contributors, and marks up the material for the printer in the traditional green ink (normally a task for a 'sub-editor', but the *Magazine* has no such person and indeed no salaried employees at all), but he also sees to the commercial side of the *Magazine*'s business, to the extent of personally dealing with the billing and the payments. Additionally, as Review Editor he receives and posts out the books for review, as well as identifying (with the help of a panel of 'suggestors') and appointing appropriate reviewers for the more than 100 books received in an average year. Some time ago he nominally retired from his formal employment (or maybe it should be expressed as 'he was retired'), and comparatively recently he was obliged to relinquish a desk he retained there, which explains why the *Magazine*'s address changed to become identical with his own. What devotion! Long may he flourish!

Here and There

ALSO IN SPELLING

Order and Choas in Stellar and Planetary Systems. — *Physics Today*, 2005 April, p. 67.

WE'VE MOVED ON SINCE GALILEO

All four of its [Jupiter's] moons can now be seen. — *The Daily Telegraph*, June Night Sky.

FALSE DAWN

[Mercury] is the only planet where the Sun rises twice a day. — *Metro*, 2005 June 27, p. 16.

CONTINENTAL DRIFT

Variable-focus photographs of stars in Orion (by permission, David Malin, Anglo-European Observatory, … — *Journal RAS Canada*, **99**, p. 102, 2005.

Advice to Contributors

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

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

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(1) G. H. Darwin, *The Observatory*, **1**, 13, 1877.
(2) D. Mihalas, *Stellar Atmospheres (2nd Edn.)* (Freeman, San Francisco), 1978.
(3) R. Kudritzki *et al.*, in C. Leitherer *et al.* (eds.), *Massive Stars in Starbursts* (Cambridge University Press), 1991, p. 59.

Journals are identified with the system of terse abbreviations used (with minor modifications) in this *Magazine* for many years, and adopted in the other major journals by 1993 (see recent issues or, *e.g.*, *MNRAS*, **206**, 1, 1993; *ApJ*, **402**, i, 1993; *A&A*, **267**, A5, 1993; *A&A Abstracts*, §**001**).

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Figures may be submitted, separately, as standard Adobe PostScript files, but authors must ensure that they fit properly onto A4 paper.

The Editors welcome contributions to the *Here and There* column. Only published material is considered, and should normally be submitted in the form of a single legible photocopy of the original and a full reference to the publication, to facilitate verification and citation.

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CONTENTS

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