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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2007 January 12th at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

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

The President. First of all, may I wish you all a happy New Year. I am sad to report that we have received notification of the death of Professor Donald Osterbrock of Lick Observatory, a very distinguished American astrophysicist, who died yesterday, January 11. He was awarded the Society's Gold Medal in 1997 and was made an Associate of the Society in 1976. We have also been notified of the death of Virpi Niemela, Emeritus Professor of La Plata University, Argentina, who died on December 18 last year. She was made an Associate of the Society on December 8, so unfortunately she was only an Associate for 10 days, but nevertheless we obviously honour her. I would like to ask you all to stand and remember those Associates of the Society. Thank you.

Moving on to our exciting programme, our first talk is by Professor Andrew Collier Cameron from St. Andrews. He is going to talk about 'New planets from *SuperWASP*'.

Professor A. Collier Cameron. The subject of my talk today is the discovery of the first two transiting exoplanets found by the *SuperWASP* instrument on La Palma. A large consortium of investigators from Belfast, St. Andrews, Keele, Leicester, the Open University, and Cambridge has been working intensively on the hardware, analysis software, and data archive since we set up our first prototype instrument on the back of a Meade telescope in the *WHT* car park during the summer of 2000. With invaluable assistance from ING, the IAC, and Sutherland, Don Pollacco's and Coel Hellier's teams at Belfast and Keele have delivered two superb wide-field survey instruments whose prime goal is to discover large numbers of extrasolar giant planets that transit their parent stars.

Transiting giant planets are of key astrophysical importance, because they are the only exoplanets for which we can determine both masses and radii. The planetary mass–radius relation is as important to our understanding of planetary structure and evolution as the stellar mass–radius relation is for stars, but only 14 of the 210 currently known exoplanets exhibit transits. Measurements of the transit light curve (combined with a stellar-mass estimate and Kepler's third law) yield the stellar and planetary radii and the impact parameter of the planet's trajectory across the face of the star. Radial-velocity follow-up then yields the reflex velocity of the parent star, and hence the mass of the planet. For its inaugural observing season in 2004 May to October, *SuperWASP* used an array of five 200-mm $f/1 \cdot 8$ Canon lenses backed by 2k × 2k CCDs to secure light curves of 1·1 million stars with $8 < V < 13$. The candidates found with such small-aperture, wide-field instruments can be followed up with high-precision radial-velocity instruments on modest-sized telescopes.

We searched 1·1 million light curves for periodic transit-like events. After applying a variety of tests to eliminate false positives and objects whose eclipse properties were clearly stellar rather than planetary, we eventually selected 25 objects with well-defined, periodic, transit-like events for radial-velocity studies. There were carried out with *SOPHIE*, the newly-commissioned and more powerful successor to the famous *ELODIE* radial-velocity instrument on the 1·93-m telescope at the Observatoire de Haute-Provence. Working in collaboration with the Geneva and Marseilles planet-search teams, and using their proven strategies for efficient elimination of astrophysical false positives, we discovered two *bona fide* planets in the sample and characterized their orbits in four nights of observation. The two planets, named WASP-1b and WASP-2b, provide a spectacular illustration of the complexity of the exoplanetary mass–radius relation. The discovery was announced at the Heidelberg conference on transiting exoplanets in 2006 September. With alacrity that would have done credit to gunslingers of the Old West, the Harvard team secured high-precision photometry of a transit of WASP-1 from their Arizona facility within hours of the announcement, and were able to report a refined radius measurement at the same conference the following morning.

The resulting refined radius estimates, published recently by Charbonneau and by Shporer's team in Israel, confirm that, although both planets have masses close to 0·9 times that of Jupiter, WASP-1b has a radius 1·44 times that of Jupiter, while WASP-2b is roughly Jupiter-sized. The complexity of the planetary mass–radius relation, and the number of physical parameters that influence it, underlines the importance of the WASP project's goal of finding many more such planets. Recent models by Burrows, Guillot, Fortney and others show that planetary radii depend not just on mass and age, but on the mass of any silicate or ice core, and on the metallicity (and hence the opacity) of the gaseous envelope. Stars with large cores tend to have smaller radii at a given total mass, whereas enhanced interior opacity serves to slow contraction.

Other factors such as stellar irradiation and the efficiency of heat transport from the dayside to the nightside of such a planet can also play an important rôle. We have been awarded director's discretionary time on both *HST* and *Spitzer* to refine the radii of both planets and to determine their dayside effective temperatures *via* secondary-eclipse observations. Both the La Palma and the Sutherland *SuperWASP* installations have been operating with eight cameras apiece since 2006 May. By continuing the radial-velocity follow-up with *SOPHIE* in the north and *CORALIE* in the south, the prospects are good for finding at least six new transiting planets per year in each hemisphere from now on.

The President. I have a question: I must say I was surprised by the big range in the radius of the transiting exo-planets; I had taken in the view that all planets of this mass had Jupiter's radius. You don't seem to be very surprised?

Professor Collier Cameron. Well, in a sense it's one way of keeping both camps happy as far as planet-formation models are concerned, because there are two very distinct camps: there's the gravitational-instability camp who claim that most

Jupiter-like planets have little or no core at all; on the other hand if a planet forms out beyond the ice boundary by gathering together several tens of Earth masses of silicates and ice, and then hoovers up the gas to make up the rest, it will finish up with a much smaller radius.

The President. So you'd say that Jupiter would have to be one of those, at the low end of the radius range?

Professor Collier Cameron. Well funnily enough, nobody really seems to know how big Jupiter's core is. There seems to be a consensus that Saturn has a reasonably substantial core; but our understanding of just how big Jupiter's core is — what's the latest word? Any planetary folk out there?

Professor B. W. Jones. It's still really uncertain.

Professor Collier Cameron. Still very uncertain — so you never know, we might learn something about Jupiter from these observations.

Mr. M. F. Osmaston. Does your equipment give you a handle on the orbital eccentricity, because if there is a relationship between the build-up of eccentricity and planetary constitution, it could be one of the parameters one needs?

Professor Collier Cameron. It could indeed. In fact, there's a nice paper by Peter Bodenheimer and others looking at orbital eccentricity and the possibility that HD 209458 B is tidally heated. Eccentricity can be driven, for instance, if there is another planet elsewhere in the system. The radial-velocity observations of these planets are generally not good enough to tie down very low eccentricities. But one very good reason for obtaining *Spitzer* measurements is that if you can get the timing of the secondary eclipse relative to the primary eclipse, then if there is a significant eccentricity, you are likely to see it in a displacement of the secondary eclipse.

Mr. Osmaston. But you haven't got any data on the two planets you've discovered?

Professor Collier Cameron. Not yet, no, but once we've made the *Spitzer* observations I hope to be able to say something about that.

The President. Thank you very much. Our next talk is on quite a controversial subject: 'The scientific case for renewed robotic and human exploration of the Moon', by Ian Crawford from Birkbeck College.

Dr. I. A. Crawford. Lunar science is undergoing something of a renaissance at present, driven in large measure by the declared US intention to return astronauts to the Moon by 2020. This, in turn, has entrained a lot of international activity in its wake, and a lunar 'Global Exploration Strategy' is currently being developed. The UK is fully engaged in this process, and just this week PPARC has announced the possibility of a UK-led robotic mission to the Moon, which could form part of its contribution to this global activity.

The primary scientific importance of the Moon arises from the fact that it has an extremely ancient surface, mostly older than 3 billion years, with some areas extending almost all the way back to the origin of the Moon 4·5 billion years ago. It therefore preserves a record of the early geological evolution of a terrestrial planet, which more complicated bodies such as Earth, Venus, and Mars have long lost, and a record of the inner Solar System environment (*e.g.*, cometary and meteorite fluxes, interplanetary dust density, and solar-wind flux and composition) from billions of years ago. Studies of the Moon are therefore important for improving our understanding of the early history of the Earth–Moon system, as well as the Solar System as a whole. While the Apollo and Luna missions retrieved 382 kg of lunar rock and soil samples from a total of nine landing sites, and provided valuable first-order information about the age and composition of the

Moon, they were only obtained from restricted areas of the near-side and are not sufficient to answer many outstanding questions of lunar evolution.

Therefore, as reviewed most recently by the US National Research Council Report on *The Scientific Context for the Exploration of the Moon* (National Academies Press, 2006), there is a strong scientific case for renewed human and robotic exploration of the Moon (see also I. A. Crawford, 'The Scientific Case for Renewed Human Exploration of the Moon', *Space Policy*, **20**, 91, 2004). Specific objectives include: sampling a representative range of lunar lithologies (*e.g.*, far-side highlands, South Pole–Aitken basin, polar regions); emplacement of a global geophysical (*e.g.*, seismic and heat-flow) network for a better understanding of the lunar interior; confirmation and characterization of polar volatiles; better calibration of the inner Solar System bombardment history; better understanding of impact- cratering processes; identification and sampling of palaeoregolith deposits (which may contain samples of ancient solar wind, Galactic cosmic rays, terrestrial meteorites, *etc.*); assessment of the value of the lunar surface as a platform for astronomical observations; and assessment of the value of the lunar surface as a platform for life-science investigations.

A subset of these objectives is of direct relevance to astrobiology, because they bear on the conditions within the inner Solar System under which life first evolved on Earth. Examples include (*i*) characterization of the impact rate in the Earth–Moon system between 4·5 and 3·5 Gyr ago, with implications for our understanding of the possible 'impact frustration' of the origin of life; (*ii*) possible preservation of ancient meteorites blasted off Earth in the first Gyr of Solar System history, which may preserve evidence of the surface environment from the time life arose, and which could also provide a record of the rate at which material was transferred between planets that is of relevance to models of panspermia; (*iii*) possible preservation of samples of the Earth's early atmosphere; and (*iv*) possible preservation of cometary volatiles and organics preserved in permanently shadowed polar craters and/or buried palaeoregoliths, which would help elucidate the importance of these sources in 'seeding' the terrestrial planets with pre-biotic materials. These specifically astrobiological implications of lunar exploration have been discussed in more detail elsewhere (I. A. Crawford, *Internat. J. Astrobiol.*, **5**, 191, 2006).

Several of the above lunar-science objectives, such as the emplacement of geophysical instruments, can certainly be achieved robotically, and this is the focus of the UK-led MoonLITE study recently announced by PPARC. This would deploy seismometers and heat-flow probes on surface penetrators released from an orbiting spacecraft to study the lunar interior. However, other objectives, such as the efficient collection of diverse rock samples, and the identification and sampling of palaeoregoliths, would benefit from the presence of human operations on the surface. The efficiency of human surface exploration was amply demonstrated by the later Apollo missions. For example, consider the *Apollo 17* mission to the Taurus–Littrow Valley on the south-east shore of Mare Serenitatis. The valley is approximately 8 km wide and the *Apollo 17* Lunar Module landed close to the centre. During their three days on the lunar surface, the two astronauts conducted three traverses with the lunar roving vehicle, each lasting just over seven hours. The total distance traversed was 35 km, and a total of 110 kg of rock and soil samples were collected and returned to Earth. In addition, the *Apollo 17* astronauts obtained a 3-m deep core sample of the regolith, measured the lunar heat flow by sinking thermocouples about 2 m below the surface, deployed eight explosive packages around the Taurus–Littrow valley as part of an active-seismic-profiling experiment, measured the local gravity field using a traverse gravimeter, measured the mechanical and electrical properties of the lunar regolith, and performed a number of additional surface experiments — all in just three days of field work. It is instructive to compare the speed, and relative thoroughness, of the *Apollo 17* exploration of the Taurus–Littrow valley with what could have been achieved using small-scale robotic rovers of the *Spirit* and *Opportunity* type. During its first three years of operation on Mars, *Spirit* has traversed a total distance of about 4 km, which may be compared with the 35 km covered in three days by the *Apollo 17* crew!

A specific example (albeit only one of many possible examples) of how future lunar science would benefit from the speed and efficiency of human explorers would be the study of the young basaltic lava flows in northern Oceanus Procellarum. This area consists of a patchwork of discrete lava flows with estimated individual ages ranging from about 3·5 to 1·2 Gyr. This is a far greater range of ages than any basalt samples collected by the Apollo missions (which occupy the narrow age range 3·8 to 3·1 Gyr). Thus, collecting samples from a number of these different lava flows and returning them to Earth for radiometric dating would greatly improve the calibration of the lunar cratering rate for the last three billion years (which is used, with assumptions, to date planetary surfaces throughout the Solar System). Moreover, geochemical studies of these samples would yield information on the evolution of the lunar mantle over this same time period. Finally, as the younger lava flows are superimposed on older ones, we may expect to find layers of palaeoregoliths sandwiched between flows dating from within this age range, which may contain a record of solar and Galactic phenomena from these ancient times. The archival value of such palaeoregoliths will be enhanced by the fact that both the under- and overlying basalt layers will lend themselves to radiometric dating, thereby precisely defining the age of the material and the geological record they contain. Taken together, this would be a very rich scientific harvest, but it probably will require the presence of human field geologists active on the surface.

Similar arguments could of course be made for the exploration of other parts of the lunar surface, such as the interior of the South Pole–Aitken basin on the farside. Similiarly, a human presence would greatly facilitate the deployment and maintenance of astronomical instruments on the Moon, and provide novel opportunities for research in the life sciences. For all these reasons it is extremely good news for planetary science, and science generally, that the renewed human exploration of the lunar surface is once again in prospect. Europe, and within Europe the UK, should aspire to play a full part in these exciting activities.

The President. Questions?

Dr. F. Diego. Ian, is there a possibility of having the exploration of the Moon as an international adventure, instead of the United States only? I am thinking of the example of the *ISS*. I see the danger of a single nation going ahead just because they can do it.

Dr. Crawford. Well, I agree with you completely; it will be preferable if it is more international. What I would say, in NASA's defence, is that I personally have been quite impressed by the extent over the last year to which they have tried to engage the international community and different space agencies around the world to attempt to internationalize this as much as possible. But we have a problem — I mean they can do this, as you say, because principally they can afford to do it. If Europe aspires to be a bigger player in these things, and if within Europe the UK aspires to be a bigger player, then it is up to us to put more resources into it. If Europe can put $16 billion into space exploration instead of $3 billion, then the partners are more equal, so the whole exercise would be more inter-national. So you can't really blame just the US for running away with it because they have the resources; if we want to join them on a more equal footing, then it's down to us to put the resources in.

Mr. J. Stone. Touching on that, Michael Griffin has said recently that he would be very happy to see NASA cooperate with others, and he did specifically mention the UK in this; together with a recent statement from our new space minister that he is not necessarily ruling out any longer the possibility of involvement in manned space exploration, it looks like we might have an exciting time ahead.

Dr. Crawford. Well, let's hope so.

A Fellow. To what extent does the fact that the Moon is a lot closer to Earth than Mars ameliorate the control-system problems which mean that *Spirit* and *Opportunity* have been crawling around at snail's pace? With the light-time delay to the Moon, you could almost get real-time control of a rover, which means they could go a lot further a lot faster.

Dr. Crawford. Oh yes, you could indeed ameliorate them to some extent, I don't disagree with that. You're still stuck, though, with the fact that rovers are attractive because they are cheap and they are cheap because they are small, and you can make them very clever; and by putting humans in the loop sort of tele-robotically from the Earth you can certainly make them more versatile than they are on Mars — I agree with that. But planets are not small, and if you wish to explore them properly, there comes a point where trying to explore planets with small rovers becomes limited by their very smallness; they can't just step over a metre-sized rock as an astronaut could — it becomes a huge issue to navigate around them. You can't drill very deep; the deepest Apollo drill core was three metres — even that is likely to be pushing future robotic technology. What we need to do on the Moon, and later on Mars, is to drill to hundreds of metres, or kilometres, as we do on Earth; and it gets to a scale that is just not amenable to robotic exploration — at least I just don't think it is realistic to expect robotic or automated systems to be able to cope. And then, of course, we have the fact that astronauts by definition have to come back for all sorts of reasons [laughter] and that means that the rock samples automatically come back with them. This was another lesson from Apollo: we have 382 kg of Moon rock returned to the Earth, just because the astronauts had to come back; it's the same on the Moon again, it will be the same on Mars, and scientifically that is hugely valuable — actually to have these materials returned rather than just relying on *in-situ* measurements. So, there is still, obviously, a place for enhanced robotics in the exploration of planetary surfaces — I don't disagree with that at all; but what I don't think it does is bring the long-term scientific benefits of actually having humans exploring planetary surfaces.

Professor J. L. Culhane. A complementary point to that — if I read your scale right, the Oceanus Procellarum frame is about 2000 km on the side, maybe a little less; how long would it take humans to do an Apollo-like job of exploration?

Dr. Crawford. The whole basin is a little bit larger than the UK, I suppose, in terms of scale. The astronauts in *Apollo 15*, *16*, and *17* had a roving vehicle, and they were limited by two things: they were limited by the life-support capability of their particular Apollo-era backpacks to seven hours and they were limited by the operational constraint that if the rover broke down, they would have to be able to walk back to the lunar module. Now, you can't explore very far on that basis, so in terms of a return to the Moon, what would be very valuable from the point of view of scientific fieldwork and exploration would be the development of a pressurized rover or something that would allow *c.* 100-km traverses. I don't think that is out of all bounds of plausibility. And then, of course, you don't have to trundle over the whole basin all at once, but just pick an area at the interfaces of several of those different lava flows with a wide range of ages — say from 1 to 3·5 Gyr — in this region. Having enough mobility to sample around an area like that, you'd sample three or four discrete lava flows to calibrate the cratering rate and look for palaeoregolithic samples.

The President. If I could just put a slightly sceptical point of view. You mentioned with approval the US moves towards a lunar programme, and I think that one of the things that makes me sceptical is that this was initiated by President Bush. It seems to me that he has then destroyed what was a pretty marvellous programme that NASA had, which could well have included this kind of exploration, and it seriously damaged other aspects of the programme for NASA. I think this is why some of us are not quite as enthusiastic as you about this programme.

Dr. Crawford. Well, I'm not here to defend President Bush. [Laughter.] I'm not sure if the alleged destruction of the US planetary programme isn't somewhat exaggerated.

The President. Well, they cut the *Origins* programme.

Dr. Crawford. I think you've got to be somewhat realistic in turns of space policy; the US has a big choice to make. The space shuttle does need replacing, it is clearly past its best-before date, the *ISS* — for better or for worse — is nearing completion, so they have a choice: what do they do after that, do they shut down their human space-flight programme entirely, or do they do something with it? Now, given that they have to do something with it, and given that the Moon is the most scientifically exciting near-term objective — because although Mars is exciting in the longer term, there are many other issues, ethical and otherwise, to be resolved — then a return to the Moon in that context seems to me a rational choice to make. And in terms of science, I am absolutely convinced, for the reasons I have already outlined, that scientific knowledge will greatly advance as a result of this exercise. So, as scientists, that's important to us. Now, in terms of the way the US funds its other space activities, this is really an argument for US policy to decide whether to ring-fence or support its robotic exploration programme.

The President. I'll let you have the last word on that; thank you very much. [Applause.]

The President. The next talk is by Dr. Peter Grindrod of UCL. He's going to talk about 'Does Titan erupt like Io? A new model for explosive volcanism on Titan'.

Dr P. Grindrod. I'd like to begin by answering my own question: I'm afraid we don't think that Titan erupts just like Io, but hopefully by the end of this talk, you might start to see a few similarities between the two. This talk is intended to be a follow-on from some work about Titan's methane which Dominic Fortes presented here on 2004 May 14 and 2006 February 10.

Most people are aware that Titan is the only moon that has a substantial atmosphere. Like the Earth's atmosphere it is mostly nitrogen, but it also contains some methane, which I'll talk more about in a moment. Also, because of the hydrocarbon haze, we cannot see down to the surface at visible wavelengths. So we use infrared and radio wavelengths (radar) to penetrate the haze, and by doing this the *Cassini* spacecraft has begun to reveal details of Titan's surface geology. It was known before *Cassini* arrived at Saturn that Titan's surface would be cold and icy, but what has now been revealed is that it is also a very geologically diverse and active place. We see evidence of numerous surface processes, probably on-going, such as dune formation, cryovolcanism, pluvial and fluvial activity, dried-up lake beds and — most recently discovered — lakes around the northern polar region. One issue which still has not been resolved entirely is the persistence of Titan's atmospheric methane, which is continuously destroyed by photochemical processes on timescales of just a few tens of millions of years. Either we are very fortunate to see the last remnants of a previously much higher methane level, or there is some process or processes actively buffering the methane loss. We think that explosive volcanism might be one, if not the principal, means of buffering the methane loss.

Titan is most likely made of a rocky core overlain by an icy mantle. With the postulated presence of ammonia around proto-Saturn, and allowing for the polymorphism of ice, the current models of Titan's internal structure predict a subsurface ocean composed of a mixture of ammonia and water. But these models fail to account for any reaction between the water and rock components during accretion and differentiation. Our work began by using experimental data to model the reactions that should take place during this hydration process to see how it would affect the internal structure. We found that the rocky core should become hydrated to the serpentine mineral antigorite, with a thin layer of brucite on top. Above this would be a shell of high-pressure ice phase VI, atop which is an ocean of aqueous ammonium sulphate (formed from the reaction of water with sulphates leached from the core) and a heterogeneous crust made of methane clathrate, ice phase Ih, and ammonium sulphate. Although this internal structure may not seem very different from the existing models, the slight change in chemistry has important consequences for the thermal evolution and for surface cryovolcanism.

As our preferred cryomagma is substantially warmer ($\sim$70 K) than just a mixture of water and ammonia, the first question we must address is whether we can actually cause melting in the crust to act as a source for any volcanism. Assuming likely geotherms for Titan in the range 1–20 K km^{-1}, we have determined that partial melting can occur in the crust at depths greater than about 10 km. The next problem we must address is bringing the melt to the surface, as cryomagmatic fluids have a negative density contrast and thus will propagate downwards without external forces. We considered both topographic pumping and diapiric stress as possible driving forces for melt migration, and found that melts can easily be driven from depths of between 2 and 10 km for likely scenarios on Titan. We think it is therefore entirely feasible for aqueous ammonium sulphate to be the predominant cryomagma on Titan.

So what happens when the cryomagma reaches the surface? Just as in terrestrial examples, it will quickly form a crystalline crust which will both insulate the flow and generate latent heat, allowing the flow to travel further. We applied a simple radiative-cooling model to a typical (50 $\times$ 10 km) cryolava flow on Titan to determine whether the effusion rates and emplacement times seem reasonable. In the case of an ammonia–water mixture, the lack of a latent-heat contribution (as cooling to solidification in this system tends to form a glass) means that the flow must be emplaced unrealistically quickly. An aqueous ammonium sulphate flow of the above size would be emplaced in a more conservative (and realistic) 12 days.

In addition to effusive flows, explosive volcanism becomes possible if the magma contains a dissolved volatile species. Current models of ammonia–water volcanism on Titan predict a purely effusive régime due to the exceedingly low solubility of methane in such mixtures. An ammonium-sulphate cryomagma is more amenable to explosive volcanism for a number of reasons. The warmer temperature allows slightly more methane to dissolve at depth, although probably not enough to cause large explosive eruptions on its own. Any magmatic dike intruding into a methane-rich crustal layer would cause the cold equivalent of a phreatomagmatic eruption on Earth. Finally, we propose that methane-clathrate xenoliths might be significant. Any piece of wall rock ripped out of place by an ascending magma would be composed mainly of methane clathrate, which will decompose at a given pressure (and therefore depth), immediately liberating methane gas into the magma and causing fragmentation. This fragmentation depth depends on the composition of the magma and the quantity of xenoliths incorporated, but is likely to be at about 1–2 km below the surface. Xenoliths are common in magmas on Earth, especially deep-sourced magmas, and it would be very surprising if their (cold) equivalent did not exist on Titan.

Geological evidence of explosive eruptions is often difficult to distinguish from effusive eruptions in orbital data. Normally we would expect to find explosion craters (calderas), fields of ash cones, and ash flows. Major explosive eruptions could result in the 'cold' equivalent of a *nuée ardente* with dense cryoclastic material flowing down the flanks of the volcano (carving channel-like features) and running out, perhaps for many tens of kilometres. Cryoclastic volcanism would also result in a large volume of fine-grained material which could then be redistributed by regional and possibly global winds. But is there any evidence for these types of features? The most convincing cryovolcano candidate seen to date, Ganesa Macula, has many of these features on its summit and flanks. Many of the north-polar lakes sit in circular features that resemble explosion craters on Earth; the presence of liquid methane in this region would indeed make the chances of a phreatomagmatic-type eruption more likely here. Finally, the commonplace dunes seen at equatorial latitudes on Titan could be the result of persistent explosive volcanism, which can also account for the atmospheric methane in the atmosphere. It seems to us that another working hypothesis, that of explosive volcanism, is required for Titan.

The President. Questions?

Professor Collier Cameron. In the context of this model, what would be the composition of the material that is forming the dunes?

Dr. Grindrod. This will be what the cryomagma is made of, basically, so the dunes are mainly going to be made up of ice. The methane clathrate will break down into ice and methane gas, so we expect icy compositions, which is pretty much what we've seen in the spectral return so far. We would also expect some ammonium sulphate to be produced too, but we're not sure what the actual spectrum of ammonium sulphate looks like at the wavelengths measured by *Cassini* — it's never been measured — but it would probably look very similar to ice.

Professor Culhane. Presumably it would be good if you could actually trap an explosion and see one. Can the model make any predictions about the rate of explosive events?

Dr. Grindrod. Chris McKay at NASA is actually going through this at the moment, independently of us. He's already taken the next step, so he's working on what happens from the surface into the atmosphere. In terms of seeing the eruption happening, I'm not sure if we could catch that from orbit, especially from radar. There are some persistent clouds which may be orographic, or there may be volcanic activity actually happening — we can't tell just yet.

The President. Thank you very much. [Applause.] Our final talk this evening is by Roger Davies from Oxford, who is going to talk about 'A new paradigm for early-type galaxies'.

Professor R. L. Davies. The *SAURON* survey comprises a study of the dynamics and formation history of the stars, and the kinematics, distribution, and physical conditions in the ionized gas, in 72 galaxies — 24 each from the E, S0, and Sa Hubble types. Interesting results include: (*i*) early-type galaxies display a much larger variety of kinematic structures and line-strength distributions than previously appreciated; (*ii*) the maps provide powerful constraints on the star-formation history; (*iii*) ionized gas is common (75% of the sample) and is found in both regular and irregular structures; (*iv*) secondary star formation is found in 50% of S0s and 25% of Es, predominately in the nuclear regions; (*v*) the mass-to-light ratio is roughly proportional to stellar-velocity dispersion, which accounts for the tilt of the Fundamental Plane; and (*vi*) the two-dimensional kinematic observations are able to constrain the dynamical models and robustly recover the distribution function of galaxies, which can be used to determine the orbital structure and mass of the black hole in galaxies.

One of the most important discoveries of the *SAURON* survey is that there is a clear separation of early-type galaxies into two groups: fast and slow rotators, defined by their specific angular momentum per unit mass (λ_R). The stellar orbital distribution of the roundish slow rotators is intrinsically different from that of the flattened fast rotators. There is a correlation between ellipticity and anisotropy — round galaxies are isotropic and flattened galaxies are increasingly anisotropic. This discovery challenges the 25-year-old view of the orbital distribution in early-type galaxies, and is illustrated by the improved definition of the isotropy parameter by Binney in 2005, and the associated treatment of the V/σ diagram.

The generality and interpretation of these exciting new results is crucially dependent on the systematics inherent in the galaxy selection for the *SAURON* survey. The *SAURON* survey selection is uniform by ellipticity, luminosity, morphology, and environment and so, although it contains representative early-type galaxies, the sample is far from complete in terms of the intrinsic galaxy population. The projected distributions of shapes and morphologies of E/S0 galaxies are far from uniform. In order to generate results which can be compared in a statistical way to galaxy-formation models it is necessary to observe a complete sample of early-type galaxies.

The dichotomy between early-type galaxies discovered by the *SAURON* survey is relevant to the latest galaxy-formation scenarios. The analysis of a large sample of galaxies in the nearby Universe observed by the SDSS shows that galaxies are distinct in terms of their colour, in the sense that they can be clearly separated into a 'blue cloud' and a 'red sequence'. This discovery, and the subsequent realization that the bimodality can be traced back in time at higher and higher redshift, is the precursor to a dramatic improvement in the detailed testing of galaxy-formation scenarios. Faber *et al.* concluded in 2005 that the gas-free, quiescent, red-sequence galaxies form by merging of gas-rich, star-forming, blue-cloud galaxies, followed by a rapid ejection of gas, due to the feedback of energy into the ISM generated by accretion onto the central supermassive black hole.

In a merger event between blue gas-rich galaxies the gas tends to settle onto a plane and form a disc, so the end result of the merger, after the gas has been expelled from the system, will be a red, purely stellar system dominated by rotation. Mergers can still happen between red gas-poor galaxies, in which case simulations show that the resulting red galaxy will show little or no rotation. The two classes of early-type galaxies observed by *SAURON* are likely to be the fossil record of the formation path followed by the galaxies when moving from the blue cloud to the red sequence. To constrain these models it is crucial to compare the predicted fractions of fast/slow rotators with the ones observed in the nearby Universe.

To obtain a complete and statistically unbiassed sample of the stellar kine-matics of nearby early-type galaxies, we are undertaking integral-field observations of all the 241 E/S0 galaxies observable with *SAURON*. We have been allocated long-term status at the *William Herschel Telescope* through both the NL and UK TACs for this project. The scientific goals of the project are: (*i*) *True distribution of specific angular momentum*: some evidence for a dichotomy was observed many years ago. However, it was only with the advent of integral-field spectroscopy that the full richness of the kinematics of early-type galaxies could be observed and a quantitative kinematical classification could be constructed. The data from a complete sample will be used to perform a kinematical classification of early-type galaxies at $z = 0$. The characterization of the classification will increase our understanding of galaxy evolution and will serve as one of the anchor points for the models of hierarchical structure formation. (*ii*) *Distribution of intrinsic shapes*: the observation of the stellar kinematical misalignment of a complete and statistically selected sample of early-type galaxies will allow a derivation of the distribution of intrinsic shapes. This provides a record of the galaxy-formation process (*e.g.*, a formation involving gas produces disc-like objects, while collisionless mergers generate triaxial galaxies). (*iii*) *The* $(V/\sigma,\varepsilon)$ *diagram*: this diagram has been used for the past thirty years to try to understand the orbital distribution in early-type galaxies, a fossil record of the formation process, as the galaxy shape depends critically on the galaxy-formation mechanism. The *SAURON* survey allowed the diagram to be put on a rigorous basis and interpreted with Binney's revised formalism. The proposed complete survey will allow the proper statistical distribution of the anisotropy in nearby galaxies to be obtained. (*iv*) *A definitive view on nearby early-type galaxies*: the original aims of the *SAURON* survey focussed on the orbit structure, frequency of cores, central discs, emission lines, distribution of metals, young stellar populations, and links between kinematics, chemistry, and dynamical structure of nearby galaxies. The survey has delivered a tantalizing view of these properties, which nevertheless lacks the required statistical robustness to be useful for a definitive test of theoretical models of galaxy formation and evolution. With a full sample of nearby galaxies with which to investigate the above properties, it will be possible to provide a stringent constraint on two key but poorly understood processes of galaxy formation, namely the star formation and the feedback from the central AGN.

Professor B. E. J. Pagel. In your last transparency, you showed something about the dark-matter fraction. I wonder if you would care to enlarge on that?

Professor Davies. This is in the paper that came out last year (M. Cappellari *et al.*, Paper IV of the *SAURON* series). One thing we can do is make an estimate of the mass-to-light ratio based on the stellar-population models, and a mass-to-light ratio based on the dynamical models. When you plot these one against the other you find that they track each other, up to an *I*-band mass-to-light ratio of about three. But then what happens is that the dynamical masses start to peel off. The population mass–luminosity relation stays the same, but the dynamical masses rise. These are the non-rotators. So the idea is that the massive galaxies that tend to be the non-rotators have perhaps a larger dark-matter fraction than the others.

Professor D. Lynden-Bell. I'd just like to make a point about the radial anisotropy. One of the things that is really rather clear is that if you have a radial anisotropy, then a weak rotation will flatten you much more than if you have an isotropic distribution. The point is when the predominant stellar motions are inwards and outwards, a weak rotation will make you much flatter because you haven't got pressure resisting the flattening. I think that's one of the reasons why the original diagram didn't contain everything one thought it did.

Professor Davies. Yes, I agree. There are a number of changes to the methodology since the 1980s that arise from such considerations that contribute to our new view as well as the new measurements.

The President. Well I think we'll stop there. Thank you very much. [Applause.]

I'm afraid we don't have drinks after this meeting because of the exile from Burlington House, and I have had complaints about this [laughter]. So I've been discussing this with the Treasurer, and we have agreed that we will have drinks after probably a couple of meetings, including the next meeting. After the February meeting there will be drinks available here. The reason we haven't done that for all meetings is that it's much more expensive to lay on a drinks party here. Quentin has asked me to say that those who would like to go for drinks at a local pub should meet in the archway at the entrance to Burlington House.

The meeting will now close. The next meeting will be on Friday, February 9.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

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

M. Rowan-Robinson, *President*
E. R. Priest, *Vice-President*
in the Chair

The President. I have to announce the death of Professor Tor Hagfors of the Max Planck Institut für Aeronomie in Katlenburg-Lindau, Germany, who died on January 16th this year. He was made an Associate in 1995 March. I'd like you all to stand for a minute. Thank you.

I'd like to welcome Professor Sami Solanki of the Institute for Astronomy, Zurich, to his first meeting of the RAS since he gave the 2001 Harold Jeffreys Lecture; he was made an Associate of the Society in 2006.

So now we come to the programme; unfortunately one of our speakers, Professor Eva Grebel, is not able to make it because of the weather conditions, so our talks are reduced in number but hopefully not in enjoyment. The first speaker will be Professor David Gubbins of the University of Leeds, and his talk is entitled 'The influence of the lower mantle on the Earth's magnetic field'.

Professor D. Gubbins. Several observations suggest the geomagnetic field is affected by the lower mantle: the four main concentrations of flux lie close, on average, to regions of high seismic velocity; virtual geomagnetic poles circle the Pacific during polarity reversal, another indication of flux concentration on specific longitudes; and parts of the Pacific region have experienced low secular change for at least the past few thousand years. Lateral variations in heat flux across the core–mantle boundary can provide qualitative explanation of all these phenomena: high heat flux induces down-welling in the core that concentrates flux while low heat flux suppresses convection, notably in the Pacific. Heat flux may be inferred from lower-mantle shear-wave velocity by assuming the variations arise from variations in temperature in the lower-mantle boundary layer. These qualitative ideas may be quantified, with difficulty, using numerical dynamo simulations with outer-boundary heat flux prescribed to match a lower-mantle seismic-tomography model. Several studies have demonstrated a similarity between a time-average of the simulated field and the recent geomagnetic field, but the simulations vary too rapidly to make a direct comparison. We have found a nearly-stationary-dynamo solution locked to lower-mantle thermal anomalies; the magnetic field has four main flux concentrations lying within a few degrees of the corresponding four lobes of the present geomagnetic field. Moreover, two of the lobes are more stable than the other two in both the simulation and the historical record, suggesting a difference in size and strength of one or two of the lower-mantle seismic anomalies. We can compare the output of these simulations with palaeomagnetic data, notably from the volcanic islands of Hawaii and Reunion; so far, comparisons of the time-average and changes with time are encouraging. The locked régime requires weak advection at the top of the outer core, otherwise boundary effects cannot penetrate a significant depth into the core. We achieved this initially with a high thermal diffusivity (Roberts number) and low Rayleigh number with uniform heating, but a more plausible buoyancy profile for the Earth's core is for vigorous compositional convection driving the dynamo at depth with weak thermal stratification operating in the upper regions, as could arise if the light component becomes immiscible at the lower pressures of the upper core. Further simulations have shown locking under this buoyancy régime.

Professor E. R. Priest. So you are saying that the cold regions have strong heat flux and that they also have strong magnetic fields, and that is of course exactly what happens in the Sun as well. Sunspots and core spots are where the magnetic field is strong. However, in your case you seem to be saying that it's the heat flux which then influences the dynamo and so causes the magnetic field to accumulate there, whereas in the Sun it's the other way around, it's the magnetic field which is dominating and changing the heat structure, probably. But I didn't quite understand the point you were making at the end about the Roberts number, so in your simulations you want the Roberts number to be a lot higher than normal. Are you suggesting that rather than using molecular values we should be using turbulent values, and that because the convection may be weaker close to the mantle that would produce the right values somehow?

Professor Gubbins. Exactly. The boundary conditions organize the pre-existing convection. If convection with homogeneous boundary conditions has the same sort of length scale and structure as what you're putting on from the mantle through the boundary condition, then you get a resonance. But you also have to allow the effect of the boundary to get into the main body of the core, and that means either high thermal diffusivity, or a Roberts number ten times the turbulent value. This is unsatisfactory but necessary for locking. We are observationally driven, and I find the observations absolutely compelling.

Professor R. Hide. I think this is quite an important point that you are talking about, because it seems to be rather crucial to look at the dynamics. Now we know what happens in two situations where you have a lateral compressed temperature gradient on Venus. Venus is covered with cloud, and cloud implies vertical motions, and as there is no evidence that Venus is losing its atmosphere, there must be descending motions somewhere, but it must be descending in very small regions which are not properly resolved, or weren't until recently. So you have this asymmetry if you heat and cool a fluid on a horizontal surface: the descending motion will be concentrated in a narrow region and the ascending motion will be distributed over a much broader region. Exactly the same is true of the oceans, where most of the descending motion is in very narrow regions indeed, and the same must be true of the Earth's core. And rotation doesn't alter this — it is rather general behaviour. Now coming to your problem, you're putting the distribution of heat transfer caused by the mantle at the top of the core, but of course the way you're dealing with it, you're using length scales. I think the solution might not lie so much in this Roberts number but in looking more closely at what's going on in the boundary layer itself, because there is that asymmetry and you can't get away from it — it is rather fundamental.

Professor Gubbins. You may be right. A result last Wednesday for low Ekman numbers showed two clumps of convection rolls beneath the cold regions, each clump made up of lots and lots of little rolls.

Professor Hide. Is the ascending motion in the core on a much bigger scale?

Professor Gubbins. Presumably, but we haven't looked yet ... this was Wednesday! But this is potentially very exciting. My hope is that when we can reach tiny Ekman numbers we are going to see a picture something like that.

Professor I. Roxburgh. I was going to ask you about the Ekman number. All these were at a fixed Ekman number? You haven't made a change at least to see in what direction it would improve?

Professor Gubbins. I would guess the number of runs we've done is over a thousand. And yes, we have. There's nothing startling in that: the requirement for locking is that the solutions be large scale. These are not easy to find. I'm presenting these because they actually do what we wanted. We can go as small as 3×10^{-6}, and that we can do conveniently and with enough runs to explore the physical behaviour properly.

The President. You're looking for your key under the lamppost?

Professor Gubbins. That's exactly right.

Professor D. Lynden-Bell. You mentioned, but I don't think you gave us any results about, the westward drift. Would you like to say a few words about this?

Professor Gubbins. We see flux patches on the equator above down-wellings. Unlike the main lobes, these are mobile and often occur in a four-fold cloverleaf pattern. They often detach and travel quickly westward, never eastward. Whether they are related to the observed westward drift in the Earth's core is difficult to say because these calculations do not represent the shorter time scales well. A high Ekman number means a false 'day', so the diffusion time may be 100 thousand years but only a few hundred 'days'! We have only just begun looking at the time variations in these solutions. It has been an ambition of mine since PhD days to find a simple, steady solution that I could perturb in order to understand time-dependent geodynamo behaviour, and now I think I have one.

The President. Thank you very much. [Applause.] Before I hand over to the Vice-President, as we have a lot of time, with your permission, I thought I would take this opportunity to tell you about some of the things the Society has been doing on your behalf — an informal five-minute report on our 'political' activities.

As you know, we are working very hard to make the RAS a more professional organization and one that has a voice in the Government, so let me just say a few words to keep you up-to-date about what's been going on.

There are three areas where we are currently pushing on, as it were; one is the new Research Council — you will have heard that the Treasury decided that it would be a good thing to merge PPARC and CCLRC. We have been very active in trying to define what the parameters of this should be, what the new Council should be like, and we believe we are being listened to. In other words, a number of the things that we have said are essential are actually being incorporated in the new Council, such as having a strong Science Committee, defining the strategy of the Council, maintaining a clearer view, and so on. That is one of the areas where we have been doing quite a bit of lobbying, both in the Office of Science and Innovation (OSI), which runs science in the country, and also with the Chief Executive of the Research Council.

A second area that we're trying to be active in, a more difficult one, is the current Comprehensive Spending Review (CSR). You'll be aware that all government departments are preparing their plans to submit to the Treasury, including OSI, which submits on behalf of the research councils within DTI. Our feeling in the RAS is that PPARC and astronomy have not done that well in the last CSR — obviously we're pleased there has been more money for science, but we feel that astronomy itself could be in quite a critical state, and so as part of that campaign I have been to the Treasury and I have also been to OSI to put the case for maintaining funding in astronomy, but it is hard to tell whether this is having any impression at all. The people I have spoken to are very polite and also they say it is quite appropriate that the RAS should be asking these questions, so I'm hoping that in some subliminal way this message will get through and we won't be savaged, but who knows. As part of this activity you will receive a message soon about suggesting that you might think about trying to get your MP to put written questions that relate to astronomy funding, space, anything that is relevant to your institution, your region, especially your constituency. This is a new departure for the RAS to try and persuade its Fellows to lobby MPs, something that we know the AAS does very actively in the US. So, when you receive this suggestion, with a list of possible questions, do think about ways that you could contact your MP and put things to them — especially if they have constituency angles — as this will also help the Government be aware that we exist and that you care about what happens.

The third area in which we are active is space, because there is a big debate going on about whether there should be more activity in space, and there are two points to make here. One is that we have consulted you about the importance of human space exploration. Council put together a very carefully worded statement about the importance of space science and what the rôle of human space flight might be within that, and hopefully you've all received an e-mail inviting you to vote on this. There is a certain fraction of members for whom we may have the wrong e-mail, so you might want to check if the RAS has your correct e-mail. If you have received this e-mail, please vote, because we intend to send the results on to the Parliamentary Select Committee which is debating space, and we want not only the result to be clear, we also want the turn-out to be good. And secondly, again on space, I mentioned the Parliamentary Select Committee and we submitted written evidence to them and we've now been called to give oral evidence, so I will be appearing before the Committee on February 21 along with Len Culhane on behalf of the UK Space Academic Network and Martin Rees on behalf of the

Royal Society. So, basically, we have an opportunity to push our case for space science there.

In conclusion, I thought I would let you know what is happening and I'm looking for your support in carrying on this kind of lobbying and trying to have an impact on the Government. I will now hand over to the Vice-President.

The Vice-President. Well, as you can see, Michael Rowan-Robinson is proving to be an extremely effective President of the RAS. He's spending huge amounts of energy and time on moving the Society in new directions and if any of you do have any comments or suggestions, please feel free to send them to me or Michael, or any other Councillor. We're also trying to be much more responsive to the membership, so please take that as an open invitation. In addition, of course, Michael is, and has been for many years, the UK's leader in infrared astronomy, so we look forward with great anticipation to his Presidential Lecture. The title is, 'Terahertz surveys: the opening of the far-infrared and sub-millimetre window'.

The President. [It is expected that a summary of this lecture will appear in a future issue of *Astronomy & Geophysics.*] [Applause.]

The Vice-President. There's time for a few questions or comments.

Rev. G. Barber. If, at first light, Population III stars are to be to be expected, say, somewhere between redshift 50 to 100, should they also be observable in the terahertz band?

The President. Well, I suppose redshift 100 would take Lyman alpha to 10 microns, so not quite. However, I think it would be difficult because we're talking about very massive stars, a few hundred solar-mass stars, which are pretty hard to detect individually. Most of the arguments about them come from background fluctuations, so I think the prospects are not great. I think with *NGST*, we certainly will be able to detect redshift-10 galaxies, redshifted into the mid-infrared. I think the potential of the terahertz band still remains. Yes, it could in principle reach really high redshifts but we would need vast, cool telescopes to beat the confusion limit.

Rev. Barber. I'm just trying to work out what the current magnitude might be expected to be at such a distance.

The President. Let me know what the answer is. [Laughter.] I'm not sure what it would be off the top of my head.

Dr. D. Ward-Thompson. What in your opinion is the most important discovery in the last 40 years in this waveband?

The President. Gosh, that's a difficult question! I think it is the discovery — for both young stellar objects and also ultra-luminous galaxies — that when stars form, essentially all the energy is coming out initially in the far infrared, so you have to have this band with which to study star formation. Eventually, stars break out of the clouds, and you then get optical and UV radiation and the rest of astronomy can take part, but initially it's terahertz.

Dr. C. Trayner. You mentioned bolometers. What other detectors are there in the terahertz range?

The President. These days there are also heterodyne instruments working on molecular-line astronomy and there are also Josephson junction devices. But in fact, for the observations I've talked about with *ISO*, *Spitzer*, *Akari*, and so on, they're still using bolometer arrays, so at the moment we're still dependent on descendents of the bolometer. But it's quite difficult to build a heterodyne instrument to go into space, as ESA is currently finding with *HIFI* on *Herschel*, but it is happening. So, on *Herschel* there will be a heterodyne instrument operating.

Professor P. G. Murdin. I didn't know the anecdote that you told about your part in the choice of Mauna Kea for the *JCMT*, but I remember it must have been a year after that that you had an observing run on La Palma, in which I think 10 cm of rain fell; there were rocks and boulders which bowled into the roads, making it impossible to get up and down to the telescope even in jeeps, the generators were shorted out, and your observing run was a bit curtailed. I think now that must have been La Palma getting its own back. [Laughter.]

The President. Just for the record, we did go up to the summit every night. We weaved our way through the streams and the boulders just in case it cleared. [Laughter.]

Professor Murdin. What I remember most about that incident was you giving me a terrific reprimand for not getting the telescopes up and running the following night. [Laughter.]

The President. It's the secret of my success. [Laughter.]

The Vice-President. I was also interested by your reminding us of the brevity of the Penzias & Wilson paper. Wouldn't it be wonderful if all the papers we have to read were so short. It would make life so easy for us. But I'd like to thank Michael very warmly for the great sweep of astronomy that he's shown us is available in this range, and taking us back so interestingly through the history of the development of the subject. Thank you very much indeed. [Applause.]

The Vice-President. I have two announcements. The first is that the next meeting is on Friday, March 9th, and more importantly, perhaps, that there is a wine reception now. You're all welcome, just through the doors there, at a cost of £1 per head.

IMPROVING SUNSPOT RECORDS: THE OBSERVATIONS BY M. HELL REVISITED

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Sunspot observations performed by the Jesuits in China during the first half of the 18th Century and compiled by Maximiliano Hell in the book *Observationes Astronomicae* (1768) have been revisited in order to get more reliable data for future reconstructions of solar activity. The most important result is the incorporation of two years, 1731 and 1746, to the list of years with no sunspot observations. Moreover, we have found a new sunspot record (for 1751 May 25).

Introduction

The study of solar activity is very important for both climatologists, owing to the enormous influence of the Sun over our planet's climate, and solar physicists. For this reason we need an accurate reconstruction of solar activity in order to understand the behaviour of the Sun during past centuries[1]. Although several techniques have been used to assess solar variability through time, most are based on the study of sunspot characteristics, such as the number or area. Thus, since the first telescopic sunspot observations in the early 17th Century, the study of scientific documents including sunspot records has been of critical importance in the reconstruction of past solar activity[2].

Historically, the International Sunspot Numbers (Wolf or Zurich sunspot numbers, R_Z) have been used as the main time series to characterize solar activity since the early 18th Century. In this regard, Hoyt & Schatten[3] elaborated a comprehensive reconstruction of solar activity based on sunspot groups, the Group Sunspot Number (R_G), which contains 80% more raw information and provides a more homogeneous series than Wolf numbers[3–5].

Records of solar activity through the 18th Century are sparse, especially during the second quarter of the century. Thus the reliability of the reconstruction of solar activity is based on the accuracy of a limited number of data. For this reason, special care is needed in dealing with fragmentary observations of sunspots. In this regard, note that the number of days per year used in the reconstruction by Hoyt & Schatten[3] is small for this period.

One of the sources used by Wolf[6] and by Hoyt & Schatten is the compilation by Maximiliano Hell[7] of the astronomical observations of the Jesuits in China during the first half of 18th Century (actually from 1717 to 1752). The book by Hell is divided into two parts; in the second part are included the observations carried out by August Hallerstein. Wolf and Hoyt & Schatten ascribed all the observations and the book to Hallerstein, although this is not correct. The aim of this paper is to revise the sunspot observations included in the compilation by Hell in order to enhance the reconstruction of solar activity during 18th Century.

The sunspot observations compiled by Hell

The sunspot records in the compilation by Hell[7] are based exclusively on solar eclipse observations. Note that the solar (and lunar) eclipse observations were very important at that time for the accurate determination of geographical coordinates. Moreover, a normal task undertaken by astronomers of the 18th Century was to time the moments of apparent contact between the Moon and the various sunspots during the progress of the eclipse. Table I shows all the solar eclipse observations recorded by Hell; it includes the date of the observation, the record from the Hoyt & Schatten reconstruction, and the revised record.

In several cases, Hoyt & Schatten list the number of groups as being zero when no sunspot information is available in the original records. In the revised data this has been corrected, eliminating the spurious observations. Note the importance of this information in the special case of a small annual number of records, as in the years included in Hell's compilation. The days with no sunspot information influence noticeably the monthly and annual averages of the sunspot numbers. It is crucially important to emphasize that the absence of evidence for sunspots should not be construed as evidence for the absence of sunspots.

For the year 1723, Hoyt & Schatten list the records for June and November. The record included as the observation of Hallerstein is the only record for June.

TABLE I

Dates of the eclipse observations compiled by Hell. The numbers of groups included in the reconstruction of Hoyt & Schatten (HS) are presented, as are the revised values of this work.

Date	*HS*	*This work*
1718 Sept. 24	0	No sunspot information
1719 Feb. 19	-	No sunspot information (Carolo Slaviczek)
1720 Aug. 04	-	No sunspot information
1721 Jul. 24	-	No sunspot information
1723 June 03	0	No sunspot information (Slaviczek, Nanchang, Kiang-Si, and anonymous, Sinoae, Conchinchina)
1730 Jul. 15	7	3 (Kegler and Pereira)
1731 Dec. 29	0	No sunspot information
1735 Oct. 16	1	1 (Jac. Phil. Simonelli, Sin-Fun-Hien, Kiang-Si)
1742 June 03	1	1
1746 Mar. 22	0	No sunspot information
1751 May 25	-	1 (Antonio Gogaisl, Pekin, and anonymous)

However, in the book by Hell, there is no information about sunspots in the report of the solar eclipse of 1723 June 3. Therefore no sunspots were recorded during that month in 1723. In this regard, Hoyt & Schatten obtain $R_G = 4 \cdot 5$ for the year 1723, but with the inclusion of the new information this should be $R_G = 9 \cdot 0$.

Hoyt & Schatten include only one record in the years 1731 and 1746 from the compilation by Hell. However, as we indicate in Table I, no sunspots were recorded on those dates. For this reason, both years must be included in the list of years identified by Hoyt & Schatten as having no sunspot records. The complete list includes 1636, 1637, 1641, 1731, 1744, 1745, 1746, and 1747. Note that, currently, there are no sunspot observations recorded for the period 1744–1747.

For the solar eclipse of 1730 July 15, Hoyt & Schatten list seven groups. This observation was carried out by the Jesuits I. Kegler (or Koegler) and A. Pereira[8–9]. In their description of the contacts of the Moon with the sunspots, the observers describe seven spots on the solar disc, taken as seven groups by Hoyt & Schatten. Unfortunately, there is no drawing of that solar eclipse and the actual positions of the seven sunspots are gleaned from verbal descriptions. However, a close inspection of the observation shows that these seven spots can easily be arranged into three groups. The first group consists of just the largest spot observed near the solar limb, the second group consists of four spots in the solar northwest, whereas the third consist of two spots in the southwest. Thus, the number of groups should be reduced from seven to three, as quoted in the revised record. This reduction might partly explain the anomalously high number of groups in the year 1730, when the observations were very scarce. In fact, with this new information, and using $k = 1 \cdot 255$, as specified by Hoyt & Schatten, R_G is 45 for the day 1730 July 15, 30 for the month 1730 July, and 67·2 for the year 1730, whereas the corresponding values of Hoyt & Schatten are 106, 61, and 69·7, respectively.

Wolf and Hoyt & Schatten omitted a sunspot observation on 1751 May 25 recorded in the book by Hell. With this new observation, and again using $k = 1 \cdot 255$, R_G is 15 for the day 1751 May 25, 47·5 for the month 1751 May, and 33·5 for the year 1751, whereas the corresponding values of Hoyt & Schatten for the month and year are 49·0 and 33·7, respectively.

Conclusions

The re-analysis of documents containing early solar observations is a valuable tool in improving the reliability of the data used in the reconstruction of solar activity during past centuries, especially when a very small number of daily observations is available. In this work, the observations made by Jesuits in China in the first half of 18th Century have been reviewed. The records used in the Hoyt & Schatten[3] reconstruction have been revised for their use in future reconstructions (Table II). New sources of information about sunspot observations could provide useful additional quantitative values in future attempts to derive more reliable sunspot numbers[10–12].

TABLE II

A comparison between Group Sunspot Number available from Hoyt & Schatten (HS) and from this work (n.a. means not available).

Date	*Daily values*		*Monthly values*		*Annual values*	
	HS	*This work*	*HS*	*This work*	*HS*	*This work*
1723 June 03	0	n.a.	0	n.a.	4·5	9·0
1730 Jul. 15	106	45	61	30	69·7	67·2
1731 Dec. 29	0	n.a.	0	n.a.	0	n.a.
1746 Mar. 22	0	n.a.	0	n.a.	0	n.a.
1751 May 25	n.a.	15	49·0	47·5	33·7	33·5

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References

(1) I. G. Usoskin & G. A. Kovaltsov, *Solar Phys.*, **224**, 37, 2004.
(2) J. M. Vaquero, astro-ph/0702068 [*Adv. Space Res.*, in press].
(3) D. V. Hoyt & K. H. Schatten, *Solar Phys.,* **179**, 189, 1998.
(4) V. Letfus, *Solar Phys.,* **184**, 201, 1999.
(5) D. H. Hathaway, R. M. Wilson & E. J. Reichmann, *Solar Phys.,* **211**, 357, 2002.
(6) R. Wolf, *Mitth. über die Sonnenflecken*, **1**, 114, 1857.
(7) M. Hell, *Observationes Astronomicae ab Anno 1717 ad Annum 1752 Pekini Sinarum Factae, Vindobonae*, 1768.
(8) I. Kegler & A. Pereira, *Phil. Trans. XXXVII*, **420**, 179, 1733.
(9) I. Kegler & A. Pereira, *Memorias de Mathematica e Phisica da Academia R. das Sciencias de Lisboa*, **2**, 36, 1799.
(10) J. M. Vaquero, *Solar Phys.*, **219**, 379, 2004.
(11) J. M. Vaquero, R. M. Trigo & M. C. Gallego, *Astron. Nachr.*, **326**, 112, 2005.
(12) J. M. Vaquero *et al.*, *Solar Phys.*, **240**, 165, 2007.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 195: HD 50730 B, HD 213014 B, AND HR 8082 B (BD $-5°$ 1867, BD $+16°$ 4746 B, AND BD $+26°$ 4074)

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Orbits are presented for three stars, all of which were initially observed simply because they were seen as faint visual companions to stars that were 'genuinely' on the writer's observing programme. Of the three, only in the case of HD 213014, where the angular separation is about 9″, is the faint star physically connected with the bright one; the other two are more distant (>1′) from their 'primaries', possess independent BD designations, and are merely optical companions. HD 50730 B is thought to be a late-type giant; it is single-lined, and has a low-eccentricity orbit with a period of 270 days. HD 213014 B is a double-lined F-type main-sequence system in a short-period (28-day) orbit which nevertheless has the high eccentricity of 0·73. It is a bit disconcerting that the rotational velocities of the two stars appear to be near to, but distinctly less than, the expected pseudo-synchronous ones. HR 8082 B is a triple-lined main-sequence object consisting of one late-F star which has a quasi-constant radial velocity plus a pair of G-type companions which circulate in a 206-day orbit of low eccentricity. The system was discovered to be a close visual binary by Couteau in 1970, and has since exhibited a slow change in position angle; the F star is identified with the visual primary.

Introduction

The discovery of the spectroscopic duplicity of the three objects discussed here arose as a result of the observer's curiosity to see whether faint stars that were visible in the fields of view, quite close to certain of his programme stars, might be physical companions.

HD 50730 is a composite-spectrum binary system which was the subject of systematic radial-velocity observations for 20 years before its orbit, which has a period of some four years, was described in a recent paper[1] (no. 181) in this series. At the time that he was writing that paper, the author found that he had once observed BD $-5°$ 1867, a considerably fainter star about 2′·7 distant from HD 50730, so (thinking to tie up a loose end) he took another measurement in order to confirm the first — only to find a discordance! He felt a responsibility to pursue the matter, although the star concerned is at a troublesome declination ($-6°$) where observing conditions at the Cambridge telescope are bad and where, moreover, it is a bit reprehensible to try to measure radial velocities at all because the coudé beam is vignetted by elements of the tube structure — the telescope was not designed for declinations below $-5°$.

HD 213014 was on a list of radial-velocity standards that was posted at the Haute-Provence (hereinafter OHP) *Coravel*; all observers were encouraged to observe a star selected from the list about once every two hours. This particular standard, a $7^{m}\cdot 6$ late-F star, was an obvious double star having a separation of about $9''$ and a Δm of about two magnitudes and looked very likely to be a physical binary, so after dutifully observing the primary the writer naturally took an interest in the companion too. As far as he is aware, he was the only *Coravel* user who did so. At first only a single, very variable, velocity was measured for it, but at the beginning of the second season a weak secondary dip was recognized in the traces, and a high-eccentricity orbit with a period of only about 28 days was soon determined.

HR 8082, a sixth-magnitude late-type giant, was placed on the observing programme in 1982 for occasional monitoring of its radial velocity owing to a (quite well founded) suspicion that it might be a composite-spectrum binary. The 'companion' star, three magnitudes fainter and rather more than $1'$ away, was first measured in 1989; a second observation in 1997 proved discordant and the interest in the object was greatly increased when it soon proved to give three dips in radial-velocity traces. The strongest component remains fixed in velocity, while the other two dips, which are unequal, move backwards and forwards on either side of it in a period that turned out to be about 206 days. It was only when this paper was being written that it came to the author's attention that the system had already been recognized as a close visual binary[2], thus neatly complementing the spectroscopic discovery described here.

HD 50730 B (BD −5° 1867)

The magnitudes given by *Tycho* (transformed from the corresponding quantities in the *Tycho* photometric system — see ref. 3, **1**, p. 57) are $V = 9^{m}\cdot 07$, $(B-V) = 1^{m}\cdot 70 \pm 0^{m}\cdot 08$. *Simbad* lists slightly (but not significantly) different values, and records the spectral type as K2; that is the type to be found in the SAO star catalogue[4], in which it is said (but with no reference given) to have come from the Leander McCormick Observatory of the University of Virginia. When the source of the classification eluded the writer, an appeal through Dr. W. C. Saslaw to the University of Virginia elicited from Dr. R. Patterson the information that about 9000 Leander McCormick classifications, made by Vyssotsky, were incorporated into the SAO catalogue, but that the one of immediate interest was not to be found in print and was presumably supplied privately.

The only paper retrieved by *Simbad* on the object is the present writer's own Paper 181[1]. The 'primary' star (HD 50730 itself) was fully described in that paper, and it is needless to repeat the information on it here. As noted above, the variability of radial velocity of the star of present interest only came to light when the work on the primary was being written up. Since then, the object has been observed tolerably regularly during the rather short intervals of season and sidereal time when it is accessible to the Cambridge telescope, and a total of 31 measurements — listed in Table I together with the single OHP one — has been made with that instrument. By themselves, they define an orbit with a period of $270\cdot 8 \pm 0\cdot 6$ days; they cover about three cycles of it. The single OHP measure was taken nearly 11 cycles before the start of the Cambridge observations. It has been subjected — as have the OHP measurements of the other stars treated here — to the usual adjustment of $+0\cdot 8$ km s^{-1} in an effort to place them on the

TABLE I

Radial-velocity observations of HD 50730 B

All the observations were made with the Cambridge Coravel, apart from the first one, which came from the OHP Coravel.

Date (UT)		MJD	Velocity km s^{-1}	Phase	(O−C) km s^{-1}
1996 Dec.	15·15	50432·15	+109·0	$\overline{10}$·185	−0·3
2004 Nov.	20·14	53329·14	96·1	0·901	0·0
2005 Jan.	2·04	53372·04	102·8	1·060	+0·4
	5·03	375·03	103·1	·071	+0·1
	13·01	383·01	105·1	·101	+0·3
	22·00	392·00	106·7	·134	0·0
Feb.	12·90	413·90	110·8	·215	+0·3
Mar.	17·84	446·84	112·9	·337	+0·2
Nov.	4·19	678·19	109·7	2·193	+0·1
	13·16	687·16	110·1	·226	−0·8
	19·14	693·14	111·6	·248	+0·1
	25·12	699·12	111·9	·270	−0·1
	30·12	704·12	112·4	·289	+0·1
Dec.	15·11	719·11	112·7	·344	0·0
	28·04	732·04	112·1	·392	−0·2
2006 Jan.	28·97	53763·97	109·8	2·510	+0·6
Feb.	8·91	774·91	107·6	·550	0·0
	25·87	791·87	104·7	·613	−0·2
Mar.	4·87	798·87	104·4	·639	+0·7
Apr.	3·83	828·83	98·8	·750	+0·1
Oct.	27·19	54035·19	109·5	3·513	+0·4
Nov.	1·20	040·20	107·8	·532	−0·6
	17·19	056·19	105·4	·591	−0·5
Dec.	6·10	075·10	102·0	·661	−0·6
2007 Jan.	6·01	54106·01	97·8	3·775	0·0
	14·99	114·99	97·3	·809	+0·4
	22·98	122·98	96·3	·838	0·0
Feb.	1·95	132·95	96·1	·875	+0·1
	14·95	145·95	96·1	·923	−0·3
	26·88	157·88	97·4	·967	−0·3
Mar.	3·88	162·88	98·7	·986	+0·2
	21·84	180·84	+101·5	4·052	−0·4

original Cambridge zero-point. Since it has an internally estimated standard error of 0·46 km s^{-1}, whereas the errors of the Cambridge measures, estimated externally from the residuals from the orbit derived from them, are somewhat smaller (0·40 km s^{-1}), it has conservatively been attributed half-weight. Incorporated into the solution of the orbit, it then refines the period to 270·33 ± 0·26 days. It may still be prudent not to take the standard error of the refined period, depending as it does upon a single observation of different provenance from the rest, too literally, but the refined period certainly ought to be more accurate than the one based on the Cambridge data alone: by actual trial, an alteration as improbably large as 1 km s^{-1} in the single velocity from OHP is found to change the derived period by about half a day only. The final orbital elements are set out in the informal table below, and the corresponding velocity curve is shown together with the underlying data in Fig. 1.

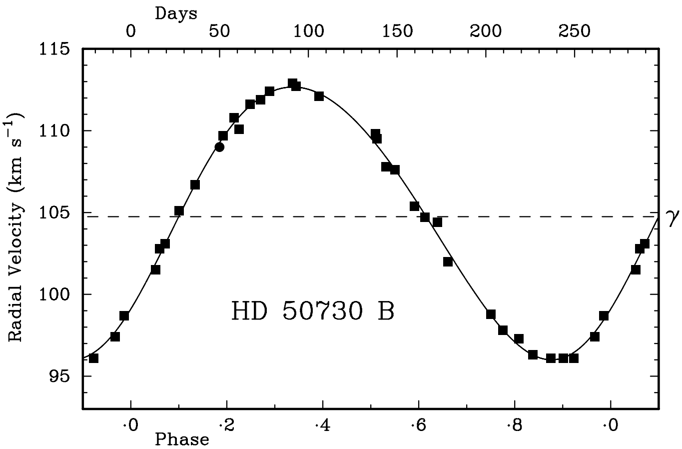

FIG. 1

The observed radial velocities of HD 50730 B plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The filled squares denote measurements made with the Cambridge *Coravel*; the single filled circle represents an OHP observation.

P = 270·33 ± 0·26 days	$(T)_2$ = MJD 53626 ± 6
γ = +104·75 ± 0·07 km s^{-1}	$a_1 \sin i$ = 30·9 ± 0·4 Gm
K = 8·33 ± 0·10 km s^{-1}	$f(m)$ = 0·0161 ± 0·0006 $M_\odot$
e = 0·079 ± 0·014	
ω = 231 ± 8 degrees	R.m.s. residual (wt. 1) = 0·34 km s^{-1}

The eccentricity is seen to be more than five times its standard deviation; the fact of its being significantly non-zero is corroborated by Bassett's[5] second statistical test which compares the sums of squares of the residuals from orbits computed in one case 'free' and in the other with zero eccentricity forced upon it. In the present instance the sum of squares is more than doubled by the imposition of zero eccentricity, showing that the two degrees of freedom represented by e and ω cost more in terms of the sum of squares than all the other 26 degrees together.

The most noteworthy feature of the elements is clearly the high γ-velocity. HD 50730 B is only the second object found in this series of papers to possess a $|\gamma|$ higher than 100 km s^{-1}. It is of interest that when the first one, HD 170737, was shown[6] in Paper 35 to have a γ-velocity of −141 km s^{-1}, that was the highest $|\gamma|$ that had ever been found for a Galactic binary system. In the quarter-century since then, ever so many binary systems have been discovered to have $|\gamma|$ values far higher than that, some of them[7] above 300 km s^{-1}.

From the agreeably large depth of the dips in radial-velocity traces and the extremely red colour index, we might with some confidence suppose the star's spectral type to be something like K5/M0 III.

HD 213014 B

HD 213014 is the only 'real' double star among the three systems treated in this paper; it has much the smallest separation and *looks* like a true binary system at sight — one would not expect to see two stars so close together, just by chance, at magnitude levels at which there are on average only a few stars per square degree. It was listed by F. G.W. Struve in his *Catalogus Novus*[8] of 1827 as Σ 2908, but a note against it says "*Besseli, mihi non inventa*"; it would have saved the present writer some time if Struve had noted the relevant bibliographical reference[9] to Bessel, who gave estimates of the stars' magnitudes as 7 and 8 and of their separation as 10″. Struve[10] later estimated the magnitudes of the pair as 7 and 8·7, and described the colour of the primary very precisely as *albasub-flava*, *i.e.*, slightly yellowish white. He was the first to make filar-micrometer measurements of the separation and position angle: in 1828 he[10] found the secondary to be 8″·86 distant in p.a. 116°·3. After more than 160 years, *Hipparcos*[11] obtained 9″·08 and 113°·7, so any change has been marginal at most. The system appears in Burnham's catalogue[12] of double stars as BDS 11737, and in Aitken's[13] as ADS 15967.

The magnitudes of stars as close together as those constituting HD 213014 are difficult to measure by the conventional method of aperture photometry, but presented little difficulty to *Hipparcos*, or more specifically to *Tycho,* whose V_T and B_T measurements, transformed according to the recipe given in the *Hipparcos* introductory volume[14], yield V and $(B-V)$ as $7^m\cdot59$ and $0^m\cdot98$ for the primary and $9^m\cdot52$ and $0^m\cdot41$ for the secondary. Halliday[15] classified the brighter star as G9 III from a slit spectrogram taken at 33 Å mm^{-1} with the DDO 74-inch reflector; he also made an assessment of line strengths by his experimental 'oscil-loscopic microphotometer' method, which returned a 'luminosity number' (directly comparable with MK luminosity class) of 3·4, *i.e.,* barely brighter than class III–IV. The secondary component was classed as F3 V by Stephenson[16], who used a 1-prism spectrograph giving 150 Å mm^{-1} at Hγ on the Lick 36-inch refractor. The *Hipparcos* parallax[17] is 0″·00227 ± 0″·00146, which when inverted yields a very shakily estimated distance of, formally, 440^{+800}_{-170} pc. Really, all it tells us is that the distance is more than about 250 pc and could be a great deal larger; correspondingly, the projected separation of the components in linear terms must be well over 2000 AU, and the distance modulus must be at least about seven magnitudes and could be much greater.

Simbad records a substantial literature (33 items) on the primary star. The great majority of the papers refers to its radial velocity, which actually is of little pertinence in itself: the reason for its popularity is that the star has for half a century been an IAU radial-velocity standard[18]. Most of the papers concerned, therefore, are not setting out to make new determinations of its velocity, whose IAU-adopted value[18] of $-39\cdot7$ km s^{-1} was reaffirmed[19] in 1983, but are simply trying to ensure that their procedures reproduce that known value as nearly as can be expected. Some of them, but by no means all, tabulate their actual results; we refrain from giving references to all the papers here, because even where they do give velocities of HD 213014 they mostly represent only internal house-keeping, and in any case the constant velocity of the primary star is of no more than oblique interest in the present context. In the course of examining the literature, the writer noticed the V magnitude of HD 213014 given as $8^m\cdot49$ in one tabulation[20], and the spectral type given as G8 V in two[21,22] and as dG8 in a third[23]. In none of those instances did it appear that fresh and reliable information was being imparted, but rather that errors of transcription must have been made. Eggen[24], however, offered a new idea — that HD 213014 is a member of his 'Wolf 630 group'; membership would define its distance modulus as $6^m\cdot33$, corresponding to a parallax of 0″·00542. No papers at all are listed by *Simbad* for HD 213014 B, not even the one[16] which reports its classification of F3 V from a slit spectrogram.

Although there seems little purpose in listing the *published* velocities of the HD 213014 primary, there is some point in presenting the ones that have been made by the writer in the same manner as those of the secondary, to permit a comparison of the mean velocities of the two components. The first photoelectric measurement of the radial velocity of HD 213014 A was actually made 40 years ago in the course of a demonstration programme[25] on stars in the +15° Selected Areas; the two results obtained in that work, though consonant with the more recent ones, are omitted from Table II, which lists the 61 *Coravel* velocities, of which 37 were made at OHP and 24 at Cambridge. The mean of the OHP data (which have received the usual adjustment of +0·8 km s^{-1}) is −39·02 km s^{-1}; the r.m.s deviation of an individual measurement is 0·17 km s^{-1}, and the standard deviation of the mean is only 0·03 km s^{-1}. The corresponding figures for the Cambridge velocities are −38·83, 0·22, and 0·045 km s^{-1}. The formally very accurately determined discrepancy, 0·18 ± 0·055 km s^{-1}, between the two mean values exactly confirms the conclusion of the recent Paper 190[26], in its discussion of four stars having colour indices extremely similar to that of HD 213014 A, that Cambridge velocities of objects of that colour warrant a correction of −0·2 km s^{-1}.

TABLE II

Coravel radial-velocity measurements of HD 213014 A

(Observed at OHP in 1994–1998, at Cambridge in 2000–2004)

UT Date	*Velocity km s^{-1}*	*UT Date*	*Velocity km s^{-1}*	*UT Date*	*Velocity km s^{-1}*
1994 Aug. 5·06	−38·9	1997 Sept. 9·98	−39·1	2000 Sept. 3·05	−38·7
6·05	−39·1	10·96	−39·3	21·02	−39·1
8·03	−39·1	14·96	−39·1	25·04	−39·1
		Dec. 21·79	−39·3	27·02	−38·9
1996 Dec. 14·79	−39·3	23·75	−39·1	30·95	−38·8
17·79	−39·3			Oct. 5·98	−38·8
25·81	−39·1	1998 July 8·10	−38·8	8·96	−38·8
		9·11	−38·8	19·93	−38·6
1997 Jan. 24·75	−38·9	10·06	−38·8	23·98	−38·9
25·77	−39·0	13·04	−38·9	30·90	−38·3
26·75	−39·0	23·04	−38·8	Nov. 1·88	−38·7
July 18·13	−39·2	24·05	−39·1	3·92	−38·8
19·04	−39·0	25·08	−38·7	13·86	−38·7
20·09	−39·2	26·10	−38·9	Dec. 9·83	−39·2
21·06	−38·9	28·01	−38·9		
22·10	−39·0	29·07	−38·9	2001 Oct. 9·04	−39·2
23·07	−39·2	31·02	−38·8	9·97	−38·9
24·01	−38·9				
25·09	−39·1	2000 July 19·09	−39·1	2002 Aug. 13·06	−38·6
26·06	−39·0	20·12	−39·0	15·08	−38·5
27·07	−39·2	Aug. 2·07	−38·5		
28·03	−38·9	9·08	−38·8	2004 Sept. 3·86	−38·8
Sept. 9·00	−38·9	29·06	−39·1		

HD 213014 B has been something of a challenge to observe, owing to its comparative faintness and its early type, which ensures that it gives only weak dips in radial-velocity traces; of course its duplicity means that such a dip as would otherwise exist is further weakened by being split between the two components.

Near the nodes of the orbit, the velocities of the two stars differ by upwards of 100 km s^{-1}, the maximum separation being about 144 km s^{-1}. The limited scanning range of the OHP *Coravel* meant that it was often necessary to observe the two components in separate integrations; although at Cambridge the range of the scan can be selected at will, separate integrations were often employed there, too, simply to reduce the integration times when the velocity separation was large. In such cases the times attributed to the two measurements have normally been averaged. On some occasions when time was short or the photon-counting rate was discouragingly low, the primary only was measured. Fig. 2 illustrates a Cambridge trace obtained when the two dips were only modestly separated and were observed together; it represents about as good a trace as can be obtained with that instrument on HD 213014 B.

Difficulty of another character arises from the high eccentricity and the rapidity with which the radial velocity can change (rates up to 6 km s^{-1} per hour). It is far from easy to obtain measurements, as would be desirable, at closely and regularly spaced intervals during the phases of rapid variation of velocity. At first

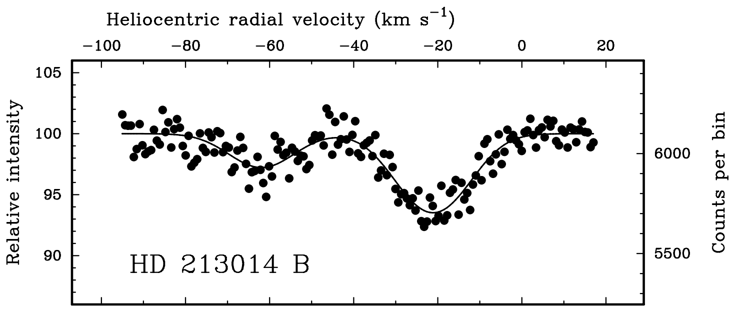

FIG. 2

Radial-velocity trace of HD 213014 B, obtained with the Cambridge *Coravel* on 2002 August 13.

sight the orbital period of 27·852 days, differing as it does by ·148 from the integral number of 28 days, appears wholly innocuous — in the course of seven cycles all phases will have been presented within two hours of the meridian at any particular Universal Time. Expressed in *sidereal* days, however, the period is only ·072 short of the integer, so in seven cycles (which is quite as many as one gets in an observing season) only half of the complete range of intra-day phases is accessible anywhere near the meridian. Moreover, 13 cycles occupy 362·07 days, so almost the same phases recur year after year, with the change from one year to the next being no greater than that from one month to the next.

The available radial velocities are listed in Table III. The Cambridge ones have received an adjustment of $-0 \cdot 8$ km s^{-1} to bring them as nearly as possible into systematic homogeneity with the OHP ones. One rather 'wild' measurement of the secondary has been rejected. The residuals from the two sources are comparable, so the data have been weighted equally. Velocities of the secondary have been weighted $^1/_4$; they would probably have needed to be attributed even less weight were it not for the fact that so many of the observations were made of the components individually, allowing the secondary to be accorded longer

Table III

Radial-velocity observations of HD 213014 B

The sources of the observations are as follows:
1996–1998 — Haute-Provence Coravel; 2000–2006 — Cambridge Coravel

Date (UT)	*HMJD*	*Velocity*		*Phase*	*(O – C)*	
		Prim. *km s*$^{-1}$	*Sec.* *km s*$^{-1}$		*Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$
1996 Dec. 17·79	50434·79	+3·0	—	0·870	+1·1	—
1997 Jan. 24·76	50472·76	−61·0	—	2·233	+0·7	—
25·78	473·78	−54·4	—	·270	+2·8	—
26·76	474·76	−53·0	—	·305	+0·3	—
July 18·13	647·13	−36·7		8·494	—	—
20·10	649·10	−35·2		·564	—	—
21·07	650·07	−27·1	−49·5	·599	+0·1	+1·9
22·11	651·11	−24·8	−58·0	·636	−0·8	−2·8
23·06	652·06	−21·4	−60·4	·671	−0·4	−1·6
24·00	653·00	−18·0	—	·704	−0·2	—
25·07	654·07	−13·6	−67·8	·743	+0·4	−0·8
26·10	655·10	−8·8	—	·780	+1·2	—
27·11	656·11	−5·7	−77·0	·816	−0·1	−0·1
28·01	657·01	−1·8	−81·8	·848	−0·5	+0·2
Sept 9·02	700·02	−42·0		10·393	—	—
9·99	700·99	−38·7		·427	—	—
10·97	701·97	−39·3		·463	—	—
14·98	705·98	−31·5		·607	—	—
Dec. 20·79	802·79	−92·1	+26·2	14·082	−1·1	+2·3
22·75	804·75	−74·1	+2·1	·153	+0·1	−1·8
23·76	805·76	−69·2	−4·2	·189	−1·3	−0·8
1998 July 9·10	51003·10	−56·1	−15·8	21·274	+0·5	+0·9
10·05	004·05	−53·6	−20·9	·309	−0·7	+0·2
23·04	017·04	−10·8	−70·6	·775	−0·3	+0·5
24·06	018·06	−7·0	−74·7	·812	−0·8	+1·5
25·09	019·09	−2·4	−80·5	·849	−1·2	+1·5
26·07	020·07	+3·6	−88·4	·884	−0·5	−0·1
27·00	021·00	+10·3	—	·917	+0·8	—
28·02	022·02	+12·8	−99·9	·954	+0·1	−1·4
28·948	022·948	−17·3	—	·987	−1·0	—
29·08	023·08	−32·8		·992	—	—
29·09	023·09	−33·6		·992	—	—
29·92	023·92	−101·4	+34·9	22·022	+0·3	−1·6
30·12	024·12	−104·4	—	·029	0·0	—
31·03	025·03	−97·9	—	·062	−0·3	—
2000 Jan. 9·78	51552·78	−85·4	—	41·010	+0·7	—
July 19·09	744·09	+3·8	—	47·879	+0·5	—
Aug. 2·08	758·08	−44·2	−31·0	48·381	+1·5	−1·5
Nov. 13·85	861·85	−83·6	+17·3	52·107	+0·5	+1·7
Dec. 9·81	887·81	−104·0	+38·8	53·039	0·0	−0·3
2001 Aug. 16·92	52137·92	−98·2	+33·7	62·019	+1·5	−0·4
Oct. 9·02	191·02	+11·0	—	63·926	+0·2	—
9·96	191·96	+11·2	−97·2	·960	−0·7	+0·4
2002 Aug. 12·957	52498·957	−5·6	−75·7	74·982	+0·5	+0·6
13·051	499·051	−12·6	−68·3	·986	+0·2	+0·1
13·135	499·135	−20·8	−62·8	·989	−0·7	−3·0
15·07	501·07	−98·8	+34·7	75·058	−0·1	+1·7
2003 Aug. 17·13	52868·13	−61·1	−10·4	88·237	0·0	+1·0

TABLE III (*concluded*)

Date (UT)		*HMJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O–C) Prim. km s⁻¹*	*(O–C) Sec. km s⁻¹*
2004 July	9·99	53195·99	−83·4	—	100·009	−1·0	—
	10·01	196·01	—	+16·8	·009	—	+1·0
	10·10	196·10	−91·5	—	·013	−1·1	—
Aug.	7·08	224·08	−96·8	—	101·017	+0·8	—
Sept.	3·88	251·88	−95·9	+26·5	102·015	−1·1	−1·8
2005 June	9·06	53530·06	−67·9	−7·0	112·003	−0·9	−2·5
Oct.	25·946	668·946	−22·5	−52·0*	116·990	+1·0	+3·8
2006 July	3·04	53919·04	+8·4	—	125·969	+0·2	—

*Rejected observation

integration times than the primary. Times in Table III are given to a third decimal of a day at phases where the rapidity of velocity change warrants it. The orbit is plotted in Fig. 3 and has the following elements:

P = 27·85180 ± 0·00010 days	$(T)_{42}$ = MJD 51580·345 ± 0·008
γ = −38·31 ± 0·13 km s^{-1}	$a_1 \sin i$ = 15·42 ± 0·06 Gm
K_1 = 58·78 ± 0·18 km s^{-1}	$a_2 \sin i$ = 18·19 ± 0·13 Gm
K_2 = 69·3 ± 0·5 km s^{-1}	$f(m_1)$ = 0·1886 ± 0·0020 $M_\odot$
q = 1·180 ± 0·009 ($= m_1/m_2$)	$f(m_2)$ = 0·310 ± 0·006 $M_\odot$
e = 0·7288 ± 0·0012	$m_1 \sin^3 i$ = 1·057 ± 0·018 $M_\odot$
ω = 100·2 ± 0·3 degrees	$m_2 \sin^3 i$ = 0·896 ± 0·010 $M_\odot$

R.m.s. residual (unit weight) = 0·8 km s^{-1}

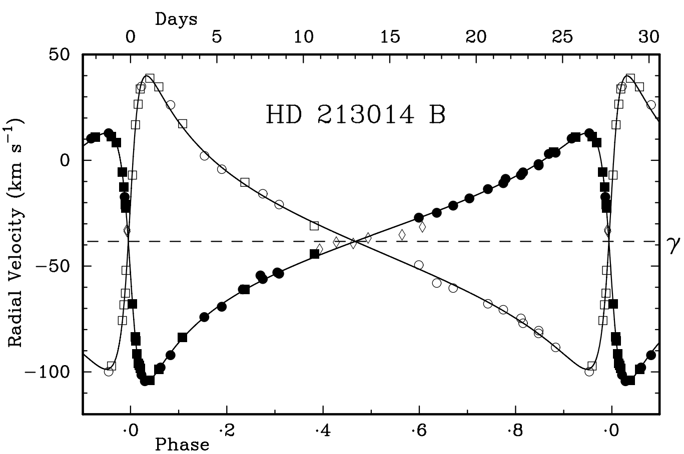

FIG. 3

The observed radial velocities of HD 213014 B plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Filled and open squares represent Cambridge observations of the primary and secondary components, respectively; circles indicate OHP measurements analogously. Open diamonds indicate velocities that were reduced as if the traces were single-lined and were not taken into account in the solution of the orbit.

The 'true' period, measured in the rest-frame of the system and calculated by means of the expression $P_{\text{true}} \sim P\,(1 - \gamma/c)$, is 27·85535 ± 0·00010 days. Its relevance is that it is the one that should be used in the calculation of $a \sin i$ and $f(m)$. Since, for nearby Galactic stars, the quantity γ/c is typically of the order of 10^{-4}, it often happens that when the orbital period of a binary system is determined to that degree of precision the true period is distinguishable from the observed one. In the present case the difference is about 37 standard deviations, its unusually high statistical significance arising from the high precision of the period (owing to its shortness and especially to the high orbital eccentricity) and to the substantially non-zero γ-velocity. Of course, statistical significance is one thing and practical significance is another: the velocity amplitude K with which the period is combined in the computation of the 'derived' quantities $a \sin i$ and $f(m)$ is not determined here to anything like 1 part in 10^4.

The difference between the γ-velocity of the spectroscopic binary and the quasi-constant velocity of the visual primary is 0·71 ± 0·13 km s^{-1}, if the OHP measures are taken as fixing the zero-points for both stars as implicitly they *are* here. Such a velocity difference is smaller than the Earth's velocity in its orbit round the Sun by a factor of 40, and would therefore represent the relative velocity in an orbit with a radius 40-squared times the size — radius 1600 AU — in a system containing one solar mass. In the HD 213014 system, with a probable total mass of something like 4 $M_\odot$, the separation would have to be as much as 6400 AU, much larger than the probable projected separation, to reduce the relative velocity to such a value in a circular orbit. On the other hand there is no question of the velocity difference being too small: of course the stars' separation seen in projection is only a minimum value, the relative-velocity vector is not likely to be directed accurately along the line of sight, and the measured velocity discrepancy probably under-states the actual difference in the radial velocities of the centres of mass of the stars because the observed values for the main-sequence pair are probably affected by a 'convective blue-shift'. The latter effect has been quantified[27] at as much as 1 km s^{-1} for Procyon, whose F5 spectral type puts it quite close to the integrated type[16] (F3) of HD 213014 B.

To create a model for the system we start from the differences between the components as regards (*a*) radial-velocity signatures and (*b*) masses. As nearly as can be determined, the mean ratio of the dip areas given by the respective components in radial-velocity traces is just 2:1, equivalent to $0^m\cdot75$ when expressed in terms of stellar magnitudes. In dealing with F-type binaries previously[28], there was adopted an empirical estimate that the actual difference in V magnitudes is 1·15 times the magnitude-equivalent of the dip ratio; the reason for the difference is that stars of later types possess spectra that cross-correlate better with the (Arcturus-based) masks in the *Coravel*s. Thus we estimate $\Delta V \sim 0^m\cdot85$. Another criterion comes from the mass ratio (q in the orbital elements above). Assessment of the trend of variation of log(mass) with colour index in Andersen's diagram[29] that plots masses of eclipsing binaries leads to the conclusion that in the F types (and probably G also), log m has a gradient of −0·013 per sub-type. For HD 213014 B, log $q \sim 0\cdot07$, so we deduce a difference of about 5 sub-types between the components; agreeably, that accords well with the magnitude difference already estimated. It remains, therefore, to adopt types that will jointly give a $(B-V)$ colour index close to the observed value of $0^m\cdot41$ and a type near to the actual classification[16] of F3 V.

If we select model types of F3 V and F8 V, with absolute V magnitudes of 3·2 and 4·0 and colour indices of $0^m\cdot37$ and $0^m\cdot52$, respectively, we obtain a colour index for the combination that agrees exactly with the observed one, but the spectral type will obviously be slightly later than F3. We can compromise by selecting an (F2 + F7) combination, which will be about $0^m \cdot 2$ brighter and $0^m \cdot 03$ bluer than (F3 + F8). The absolute magnitude of the revised combination is $2^m \cdot 58$, which by comparison with the *Tycho*-derived apparent V magnitude of $9^m \cdot 52$ leads, without allowance for possible interstellar absorption, to a distance modulus of $6^m \cdot 94$. That corresponds to a distance of 244 pc and to a parallax of $0'' \cdot 0041$, representing a discrepancy of $1 \cdot 25\sigma$ from the *Hipparcos* value — a bit uncomfortable but not unacceptable. It implies an absolute magnitude of about $+0^m \cdot 7$ for the G9 III visual primary, a result that cannot be faulted. Although the distance modulus found here is a lot ($1^m \cdot 28$) smaller than is implied by the measured parallax, it would need to be $0^m \cdot 6$ smaller still to validate Eggen's proposal[24] to make HD 213014 a member of the 'Wolf 630 group'. It is difficult to find a means of modifying the parameters enough to accommodate such a small modulus, which would also leave the *Hipparcos* parallax adrift by more than $2 \cdot 2\sigma$. Postulating significant interstellar absorption would be counter-productive, because the concomitant reddening would require the stars to be bluer, and therefore further up the main sequence and consequently brighter, to produce the correct observed colour.

The masses of the stars in the (F2 + F7) model should be about 1·45 and $1 \cdot 25\ M_\odot$; a comparison with the values of $m_{1,2} \sin^3 i$ given by the orbital elements above suggests that $\sin^3 i \sim 0 \cdot 73$, or $\sin i \sim 0 \cdot 90$.

A referee, to whom the writer is grateful for help in getting this paper into shape, has expressed some disquiet at the lack of references to support the values adopted for such quantities as the masses mentioned in the immediately preceding paragraph, and by implication for the colour indices, stellar radii, *etc.*, utilized in other places. There is no intention on the part of the author to conceal the fact that he is indebted to the standard tabulations such as those in refs. 30–33 and to other relevant works such as that by Andersen[29]. The truth is, however, that the major contribution to the uncertainties in most of the values so adopted lies not in the tables but in the arguments with which they are being entered, *i.e.*, in, for example, the spectral types or colour indices. Thus the quantities adopted should be regarded as nothing more than the author's effort at 'intelligent guesses', and it would seem inappropriate to dignify them with references and thereby possibly give the impression that mere guesswork, albeit made in good faith, could receive any sort of authoritative support from those publications.

Rotational velocities are not determined very accurately from such weak dips as are given in radial-velocity traces by the components of HD 213014 B, but the mean values found from a fair number of observations should be reliable. The means given by the OHP traces for $v \sin i$ are $10 \cdot 7 \pm 0 \cdot 6$ km s^{-1} for the primary and $12 \cdot 1 \pm 1 \cdot 2$ for the secondary; the corresponding Cambridge means are $11 \cdot 4 \pm 0 \cdot 6$ and $11 \cdot 7 \pm 0 \cdot 8$ km s^{-1}, respectively. Allowing, possibly, for a little bit of prejudice, in not wishing to admit that the slightly smaller secondary has a slightly larger rotational velocity than the primary, we might take the projected rotational velocities of both stars as 11 km s^{-1}. The actual equatorial velocities of the stars are slightly larger, say 12 km s^{-1} to the accuracy that is warranted, if the value of $\sin i$ estimated in reference to the orbital inclination in a previous paragraph is applicable to the axial inclinations too.

It would be nice to think that the rotations are 'captured' into pseudo-synchronism[34] with the orbital revolution; although[35] captured rotations are not usual among main-sequence binaries with periods longer than 10 days or so, the high eccentricity of the HD 213014 B orbit brings the stars close enough together at periastron for tidal effects to be important. The minimum separation is $(1-e)$, *i.e.*, 0·27, times the mean, and since periods go as the three-halves power of distances the periastron separation corresponds to the radius of a *circular* orbit with a period of only about 0·14 times the actual period, about 3·9 days. The pseudo-synchronous period at the eccentricity of HD 213014 B is[34] only about 1/8 of the orbital period, *i.e.*, 3·5 days, from which we deduce that the equatorial rotational velocities ought to be about $14 \cdot 4(R/R_\odot)$ km s^{-1} — about 21 and 18 km s^{-1} if we adopt values of about 1·45 and 1·25 $R_\odot$ for the stellar radii. Those are decidedly larger than the observed values, and no plausible way is apparent of fudging the numbers or bending the arguments to coerce agreement: the conclusion seems inescapable that the stars are not rotating as fast as they 'ought' to be. It may be relevant to remark at this point upon the orbit's high eccentricity, which is extraordinary for a system with such a short period; no similar combination of elements is to be found among the objects treated previously in this series of papers. It is tempting to see the discrepancy in terms of a non-equilibrium situation in which the orbital eccentricity is being driven up on a rather short time-scale by some mechanism; the rotational velocities would be pseudo-synchronized at an eccentricity of about 0·64. The visual primary, whose projected distance from the binary is more than 2000 AU, seems too far away to alter the eccentricity by the Kozai[36] mechanism, but it is perhaps possible that there is another component, not easily detectable, in a hierarchy intermediate between the primary and the 28-day binary, that could cause the eccentricity to change on an appropriate time-scale.

HR 8082 B

The observer's initial interest was in HR 8082, a star that he had first observed almost 50 years ago in the course of the first of the Cambridge programmes of photoelectric narrow-band spectrophotometry[37], on the strength of the λ 4200-Å CN band in late-type stars. The star was at that time listed with spectral type sgK1, which was no doubt copied from the *Radial Velocity Catalogue*[38]; the K1 there must have come from the paper on spectroscopic absolute magnitudes by Young & Harper[39], and the 'subgiant' prefix probably represents the *Catalogue* editor's own interpetation of those authors' results, which were $M_V = +2 \cdot 4$ (Young) and $+1 \cdot 7$ (Harper). The Cambridge narrow-band CN[37] and Mg *b*-line[40] measurements indicated a giant rather than a subgiant luminosity. As a corollary to the narrow-band work, Redman (the Cambridge Director at the time, and mentor and collaborator of the writer) arranged for his staff member Argue to visit KPNO and make *UBV* measurements[41] of many of the same stars which had up till that time lacked accurate photometry, with the result in the case of HR 8082 that $V = 6^{m} \cdot 11$, $(B-V) = 1^{m} \cdot 05$, $(U-B) = 0^{m} \cdot 62$. In conjunction with the *Hipparcos* parallax[42] of $0'' \cdot 00937 \pm 0'' \cdot 00072$, corresponding to a distance modulus of $5 \cdot 14 \pm 0 \cdot 15$ magnitudes, the V magnitude yields a luminosity estimate of $M_V = +0^{m} \cdot 97 \pm 0^{m} \cdot 15$, so the star is certainly of luminosity class III. A classification of K0 II–III was made from an objective-prism spectrum taken at a dispersion of 110 Å mm^{-1} at Hγ with the *Curtis Schmidt*[43] by Yoss[44]. The only radial velocities⋆ published for HR 8082 are three by Harper[45] obtained photographically with the Victoria 72-inch reflector and a prismatic spectrograph giving a reciprocal dispersion of about 30 Å mm^{-1} at Hγ. The mean

⋆*Note added in proof.* A mean of $-8 \cdot 47 \pm 0 \cdot 19$ km s^{-1} ($n=2$) is given in *A&A*, **427**, 313, 2004.

velocity and its 'probable error' as given by Harper demonstrate the velocity that he attributed to the date 1930 October 21 to have been misprinted — it should be −7·9 and not −17·9 km s^{-1}. The mean of −6·1 is the value which, after an empirical correction of +0·2 km s^{-1}, is printed in the *Radial Velocity Catalogue*[38].

When the writer first took a particular interest in composite spectra some 25 years ago, he noticed the unusual relationship between the colour indices of HR 8082: the $(U-B)$ index was seen to be far too blue in relation to the $(B-V)$ one, or for that matter for a star of the type that HR 8082 was supposed to be. The implication was that the light intensity in the ultraviolet was probably held up by a hitherto unrecognized companion star of earlier type, so the object was placed on the radial-velocity observing programme and monitored systematically. In the event, no change has been seen in the velocity, but that does not necessarily invalidate either the impression that there is a discrepancy in the colours or that a hot companion star may be responsible for it. Such of the writer's radial velocities as were made with the *Coravel*s at OHP and ESO were included in, or may have completely constituted, the mean of −8·40 ± 0·09 km s^{-1} listed in a table to which attention was recently drawn by Famaey *et al.*[46]. The full set of observed velocities of HR 8082 is presented in Table IV.

TABLE IV

Radial-velocity measurements of HR 8082

UT Date	*Velocity km s^{-1}*	*UT Date*	*Velocity km s^{-1}*	*UT Date*	*Velocity km s^{-1}*
1982 June 29·03*	−7·6	1988 Nov. 6·91§	−7·2	1999 Dec. 29·76¶	−7·0
July 20·06*	−7·7	1989 Oct. 31·84§	−7·6	2000 Sept. 4·96¶	−7·4
Aug. 31·94*	−7·7	1990 Dec. 27·71*	−8·9	2001 Dec. 7·74¶	−6·9
Sept. 23·94*	−8·6	1991 Dec. 21·72§	−7·4	2002 Sept. 3·01¶	−7·1
1983 June 15·04*	−7·2	1992 Aug. 14·96§	−7·5	2003 Aug. 20·06¶	−7·1
July 5·07*	−9·2	1993 July 11·07§	−7·4	Sept. 14·00¶	−7·3
Oct. 24·26†	−6·9	1994 Aug. 5·04§	−8·0	2004 Sept. 5·02¶	−7·3
Dec. 4·74*	−8·2	1996 Dec. 25·78§	−7·3	2005 Aug. 17·06¶	−7·0
1985 Oct. 12·87*	−8·3	1997 Sept. 10·92§	−7·8	2006 July 19·10¶	−7·1
1987 Nov. 8·99‡	−8·0	1998 July 12·07§	−7·3		

Sources: *Original Cambridge spectrometer † ESO *Coravel*
‡ DAO § OHP *Coravel* ¶ Cambridge *Coravel*

Each of the three main instruments used (the original Cambridge spectrometer, and the OHP and Cambridge *Coravel*s) has given nine velocities. The means for the separate sources are −8·16 ± 0·22, −7·50 ± 0·09, and −7·13 ± 0·06 km s^{-1}, respectively. The last value is expected to be subject to a negative correction of about −0·2 km s^{-1}, which has repeatedly been found (*cf.* ref. 26 and the section on HD 213014 above) to be applicable to Cambridge *Coravel* velocities of stars with $(B-V)$ colour indices similar to that of HR 8082. After the application of such a correction, the difference between the mean values from the two *Coravel*s is negligible. There remains a small and statistically quite significant discrepancy between the 'original Cambridge' and the subsequent measurements, but it would be rash indeed to conclude that any real variation of velocity has been demonstrated. With the 'original Cambridge' data suitably down-weighted, the overall mean value for the radial velocity of HR 8082 may be taken as −7·5 km s^{-1}.

On the occasion of the very first measurement of HR 8082, in 1982, the observer noted the existence of a visual companion of about $9^{1}/_{2}^{m}$ about 100″ following the principal star. Later in that same summer of 1982 he tried to measure its radial velocity with the original spectrometer but was unable to get a result, although he was able to tell from the trace that the trouble was that the star was too blue for measurement with that instrument. At the same time he noted that the companion possessed its own, much fainter, companion at an estimated position 10″ north-following its primary. He subsequently found that he was not the first to have noticed the companion's companion — the pair was listed by Struve as no. 2756 in his *Catalogus Novus*[8] of 1827! That catalogue, however, does not give any actual measurements, although in the case of interest Struve, who estimated the magnitudes of the pair as 8 and 10, did go so far as to assert (in Latin, as usual) that the primary was the preceding component, and by placing it in his class III he indicated that the angular separation was between 8 and 16 seconds of arc. However, when he subsequently came to present, in his enormous work *Mensurae Micrometricae*[10], micrometer measurements of the pairs in his *Catalogus*, Struve rejected the pair on the grounds that the secondary was fainter than 9^m — which by a large margin it certainly *is*. The omission was repaired, after a half-century, by Stone[47], who observed it in 1879 with the 11-inch Cincinnati refractor; he considered the magnitudes to be $8^1/_2$ and 11, and the separation to be 11″·52 in position angle 47°·4. Burnham, in his 1906 catalogue[12] in which the pair is identified as BDS 10733 and as "Σ 2756 *rej.*", gave a second measure, made by himself in 1905 with the Yerkes 40-inch refractor, of 11″·53 in 51°·5; he estimated the magnitudes as 9·0 and 11·1. He also noted the existence of "a 6^m star in the field, 285°·6, 79″·24" — which is, of course, HR 8082 itself. The *Index Catalogue*[48] notes that there are *three* measurements of the pair between the limiting dates of 1879 and 1905, which are the dates of the two of which we are aware. Burnham[12], however, distinctly asserts that his was only the second measurement, and Dr. B. Mason, the keeper of the *Washington Double-Star Catalogue*, has kindly informed me that there is no record of any additional measures in that comprehensive work.

Surprisingly, the star that is here called HR 8082 B (though more officially it is BD +26° 4074) features in the *General Catalogue*[49], although it is much fainter than the majority of the entries. It is listed there as being of magnitude 10·1 and spectral type G0. The magnitude was derived from Argelander's estimate of $9^m·4$ in the *BD*[50], with the considerable correction proposed by Pickering[51], while it is implicit in the *Introduction*[49] to the *Catalogue* that the spectral type was furnished, nominally, by Harlow Shapley, Director of the Harvard College Observatory, after "consultation" with Miss Cannon. *Simbad* notes the spectral type as F8, possibly having copied it from the *Index Catalogue*[48], which indicates that the classification was made at the Leander McCormick Observatory. A trawl through the *Publications* of that Observatory has shown that it is to Vyssotsky[52] that we owe the F8 type. The *Index Catalogue* gives the magnitudes of the pair as 10·2 and 12·3. *Tycho* has provided more objective photometry of $V = 9^m·18$, $(B-V) = 0^m·59$ for the primary component.

In 1970 Couteau[2], using the Nice 50-cm refractor, discovered BD +26° 4074 to be itself a close visual binary, with a separation of 0″·33 in position angle 345°·1. It has been measured a number of times since, and has shown a slow decrease in position angle; the current value is probably near 320°. It is perhaps surprising that Burnham[12] did not discover the duplicity of the star when he was bisecting its image in order to measure the Struve companion in 1905, unless the separation was a good deal less then than it has been in recent decades.

Having failed to obtain a result on the star with his prototype radial-velocity spectrometer, the writer tried again, with the OHP *Coravel*, in 1989. With that

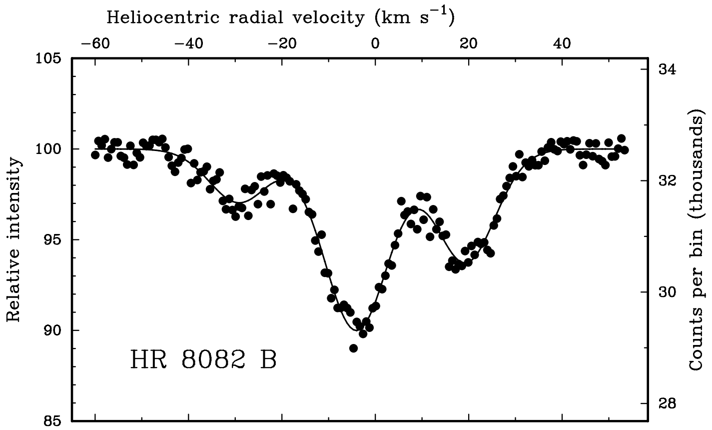

FIG. 4

Cambridge radial-velocity trace of HR 8082 B, obtained on 2004 September 18 and illustrating the triple lines at their maximum mutual separation.

instrument the object gave a trace that appeared to show a weak and asymmetrical dip, which did not arouse undue excitement. Further efforts in 1997 September were discordant with the earlier value. In December of that year, however, fresh radial-velocity traces of the object were found to exhibit *three* dips, so the star was regarded with entirely new interest. The dips are weak and are always more or less blended together, and since the star is none too bright for telescopes of the sort to which the observer has had access in recent years it has not been an easy object to observe. Fig. 4 illustrates a particularly well-integrated Cambridge trace taken when the dips were about as widely separated as they ever become. The central and strongest one is fixed in velocity, and the other two, which are of noticeably different depths, are often more or less merged with it; at the node, however, they become as distinct as they are shown in Fig. 4 or (rather less so, at the opposite node) as its mirror image in which the two outer dips have exchanged places. Altogether 13 traces have been obtained with the OHP *Coravel* and 35 at Cambridge. It has proved possible to derive meaningful trios of velocities from even heavily blended Cambridge traces by means of a triple-lined reduction program written (like the other *Coravel* reduction programs) by Dr. R. E. M. Griffin. Four of the OHP traces and one of the Cambridge ones have been reduced only as single-lined and cannot be used in the solution of the orbit, and in two other OHP cases no velocity has been given for the weak third component.

The velocities are all set out in Table V; those from OHP have received the usual adjustment of $+0 \cdot 8$ km s^{-1} and those from Cambridge have been adjusted by $-0 \cdot 6$ km s^{-1}. In the five instances in which only single-lined reductions are available, the resulting velocity has been printed between the columns for the primary and secondary of the short-period sub-system, although of course they actually represent also the large contribution of the 'fixed' component.

The orbit is solved as a straightforward double-lined one, with the measures of the two variable-velocity components weighted in the ratio 1 to $0 \cdot 5$ to favour the one that gives the stronger dip; multiplicatively, the OHP velocities have been half-weighted to bring the weighted variances of the two sources, as well as the two components, into approximate equality. The resulting orbit is shown in Fig. 5 and its elements are presented below.

P = 206·38 ± 0·08 days	$(T)_{23}$ = MJD 52428·7 ± 1·9
γ = −3·44 ± 0·14 km s^{-1}	$a_1 \sin i$ = 59·1 ± 0·6 Gm
K_1 = 21·02 ± 0·20 km s^{-1}	$a_2 \sin i$ = 67·5 ± 0·8 Gm
K_2 = 24·03 ± 0·28 km s^{-1}	$f(m_1)$ = 0·193 ± 0·006 $M_\odot$
q = 1·143 ± 0·017 ($=m_1/m_2$)	$f(m_2)$ = 0·289 ± 0·010 $M_\odot$
e = 0·140 ± 0·009	$m_1 \sin^3 i$ = 1·015 ± 0·029 $M_\odot$
ω = 321 ± 4 degrees	$m_2 \sin^3 i$ = 0·888 ± 0·022 $M_\odot$

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

The radial velocities of the third (stationary) component, although included in Table V and featuring in Fig. 5, are of course ignored in the solution; reassuringly they exhibit no relationship to the phase in the 206-day orbit, neither do they show any perceptible progressive variation with time, but it is evident from Fig. 5 that they lie very close to, though marginally below, the γ-velocity of the binary. In fact their mean (OHP half-weighted as for the other components) is −3·97 ± 0·10 km s^{-1}, differing by just −0·53 ± 0·17 km s^{-1} from the γ-velocity of the 206-day pair.

We can attempt to construct a model of the system, starting from the observed equivalent widths of the three dips, the mass ratio of the 206-day pair, and the colour index and spectral type. We can surely take it for granted that we are dealing with a system of three F/G main-sequence stars, since giants with such a colour or spectral type are rare and it would be almost inconceivable to find three of them together in one system. The logarithm of the mass ratio of the binary pair is 0·058 ± 0·006, so from its empirically determined gradient (noted in the section

TABLE V

Radial-velocity observations of HR 8082 B

The sources of the observations are as follows:
1989–1998 — Haute-Provence Coravel; 1999–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Radial Velocities* 'Fixed' km s^{-1}	Prim. km s^{-1}	Sec. km s^{-1}	*Phase*	*(O − C)* Prim. km s^{-1}	Sec. km s^{-1}
1989 Oct. 31·85	47830·85	−4·9	−23·4	—	0·722	−3·3	—
1997 Sept. 10·93	50701·93		−7·6		14·633	—	—
14·94	705·94		−8·8		·653	—	—
Dec. 21·83	803·83	−3·8	+18·6	−29·7	15·127	0·0	−1·0
26·77	808·77	−3·6	+18·2	—	·151	+1·1	—
1998 July 8·09	51002·09	−2·8	+19·5	−26·4	16·088	−0·3	+3·7
10·03	004·03	−3·2	+20·0	−31·3	·097	+0·3	−1·4
12·08	006·08	−3·4	+19·5	−30·7	·107	0·0	−1·0
15·03	009·03	−3·8	+18·7	−30·7	·121	−0·2	−1·7
22·99	016·99	−3·5	+16·4	−26·7	·160	0·0	−0·5
27·96	021·96	−4·0	+11·9	−22·5	·184	−2·4	+1·2
30·94	024·94		−1·8		·198	—	—
Aug. 5·00	030·00		−3·2		·223	—	—
1999 Dec. 7·79	51519·79	−3·1	−22·0	+16·6	18·596	−0·3	−0·8
19·78	531·78	−4·1	−22·5	+18·8	·654	−0·4	+0·9
28·78	540·78	−4·9	−24·2	+20·1	·698	−3·0	+3·3

TABLE V (*concluded*)

Date (UT)	*MJD*	*Radial Velocities* 'Fixed' km s⁻¹	*Prim.* km s⁻¹	*Sec.* km s⁻¹	*Phase*	*(O–C)* *Prim.* km s⁻¹	*Sec.* km s⁻¹
2000 Jan. 9·74	51552·74	−3·9	−17·4	+13·5	18·756	+0·5	+0·4
June 7·07	702·07	−4·4	−17·8	+10·3	19·479	−1·7	−0·7
July 20·07	745·07	−3·3	−19·1	+18·0	·688	+2·4	+0·8
Sept. 3·00	790·00		−3·2		·905	—	—
17·00	804·00	−4·4	+11·9	−23·2	·973	+0·1	−2·4
23·88	810·88	−4·0	+16·0	−23·9	20·007	+0·1	+1·6
Oct. 5·95	822·95	−3·9	+19·9	−28·6	·065	+0·3	+1·2
19·90	836·90	−5·4	+18·2	−28·4	·133	−0·1	−0·1
Nov. 1·87	849·87	−5·2	+12·1	−23·3	·195	−1·1	−0·9
Dec. 30·75	908·75	−4·4	−16·0	+10·1	·481	+0·2	−1·0
2002 July 22·07	52477·07	−3·2	+9·7	−15·8	23·234	+0·8	+1·8
Sept. 28·99	545·99	−4·9	−23·0	+16·9	·568	−2·1	+0·4
Oct. 23·91	570·91	−3·6	−20·6	+18·4	·689	+0·9	+1·3
2003 July 15·05	52835·05	−3·6	+11·8	−21·5	24·969	+0·6	−1·3
21·02	841·02	−4·0	+14·3	−22·8	·998	−0·6	+1·6
28·07	848·07	−4·1	+18·4	−28·4	25·032	+0·3	−0·3
Aug. 3·06	854·06	−4·6	+20·0	−30·6	·061	+0·5	−0·9
15·06	866·06	−4·2	+19·4	−29·6	·119	+0·4	−0·5
20·05	871·05	−4·0	+17·9	−29·2	·143	+0·2	−1·6
31·02	882·02	−4·2	+12·1	−20·7	·197	−0·9	+1·6
2004 Sept. 1·02	53249·02	−3·6	+12·2	−19·2	26·975	+0·2	+1·9
5·01	253·01	−4·2	+13·6	−23·2	·994	−0·9	+0·7
18·94	266·94	−3·9	+19·5	−29·1	27·062	−0·1	+0·6
25·96	273·96	−4·8	+19·5	−30·9	·096	−0·3	−0·9
Nov. 4·87	313·87	−3·9	+2·2	−11·4	·289	−0·4	−1·1
2005 Aug. 7·02	53589·02	−3·9	−21·8	+16·6	28·622	+0·3	−1·3
17·08	599·08	−3·9	−20·5	+18·1	·671	+1·4	+0·5
Sept. 2·93	615·93	−3·1	−17·0	+12·5	·753	+1·2	−0·9
8·01	621·01	−4·1	−15·4	+9·6	·777	+0·7	−1·5
20·93	633·93	−3·7	−9·2	+4·8	·840	−0·3	+2·0
2006 July 14·07	53930·07	−3·4	+5·4	−11·0	30·275	+1·1	+1·3
Sept. 9·01	987·01	−3·5	−19·5	+13·9	·551	+0·7	−1·8

above on HD 213014 B) of −0·013 per sub-type we obtain an indication of a difference of four or five sub-types between the two stars in the short-period orbit. The mean equivalent widths of the three dips in Cambridge radial-velocity traces are 1·67, 1·10, and 0·57 km s⁻¹. By coincidence the largest value is exactly equal to the sum of the others. The corresponding star, which obviously must be the brightest, must also be brighter than both of the others combined, since its spectrum must be of earlier type and therefore not cross-correlate as well with the masks in the *Coravel*s, so it needs to be brighter to give a dip of the same strength. The constant-velocity star is thereby identified with the primary component of the close visual binary, and the 206-day system with the visual secondary.

In comparison with the equivalent width (EW) of the strongest dip, the other two are smaller by factors of 1·53 and 3·02, numbers whose logarithms are 0·186 and 0·480 and which therefore correspond to differences, expressed in terms of stellar magnitudes ($\Delta m = 2\cdot5\Delta\log(\mathrm{EW})$) of about $0^{\mathrm{m}}\cdot47$ and $1^{\mathrm{m}}\cdot20$, respectively. From the rule of thumb relating ΔV to equivalent-width differences, noted in the previous section, the V-magnitude differences could be expected to be about 1·15

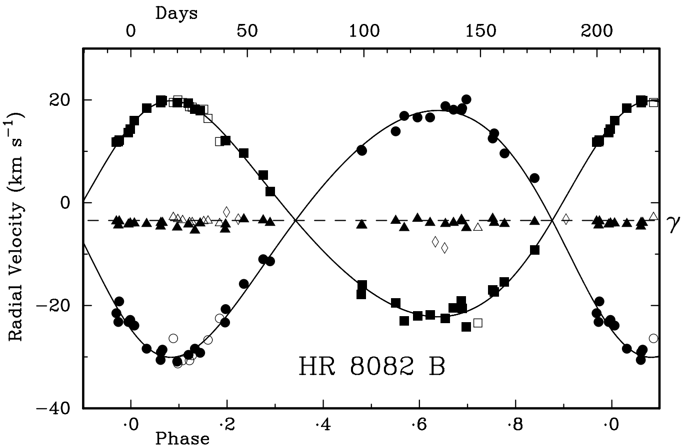

FIG. 5

The observed radial velocities of HR 8082 B plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. In a departure from the symbol-allocation conventions that are usual in this series of papers, Cambridge observations of all three components are denoted by filled symbols — squares and circles for the primary and secondary of the short-period sub-system, and triangles for the 'fixed' component — while the corresponding open symbols are used for OHP data. Open diamonds plot single-lined reductions.

times larger, $0^m \cdot 54$ and $1^m \cdot 38$, respectively, although the precision of the data and the correctness of the 'rule' by no means merit a second decimal place. What seems to be a plausible photometric model for the system is set out in the table below. To avoid unnecessary confusion and/or pedantry, the three stars are identified in the table simply as A (the singleton and visual primary), and B (the 206-day pair); the fact that the whole system is called in this paper HR 8082 B and is itself the primary of the wide visual binary Σ 2756 that, though first designated by Struve[8], was 'discovered' by Bessel[9], is for convenience glossed over. The photometric properties of the types G1 and G6·5 that are proposed in the table have been adopted by linear interpolation between G0 and G2, and between G5 and G8, respectively, in the tables in Landolt-Börnstein (ref. 31, pp. 15*ff*). The type G6·5 has been selected merely to facilitate such interpolation — there is no suggestion that a precision of 1/20, let alone 1/100, of a spectral class is warranted here.

Component	*Type*	M_V	$(B-V)$	$(U-B)$	M_B	M_U
A	F8 V	4·0	0·52	0·02	4·52	4·54
B	G1 V	4·55	0·60	0·09	5·15	5·24
	G6·5 V	5·3	0·71	0·25	6·01	6·26
B (sum)	~G3 V	4·11	0·63	0·14	4·74	4·88
A + B	~G0 V	3·30	0·57	0·07	3·87	3·94

The only results in the table that can be checked against observation are the combined $(B-V)$ colour index for the complete system, where our $0^m \cdot 57$ may be compared with the *Tycho*-derived value of $0^m \cdot 59$, and correspondingly the estimated combined apparent spectral type of G0, which agrees with Miss Cannon's classification[49]. The table also indicates an expected difference of just $0^m \cdot 11$ in the V magnitudes of the members of the close visual binary, and offers a number of other hostages to future comparison, in the form of the predicted $(U-B)$ colour index and the distance modulus derived from the total absolute magnitude of $3^{m} \cdot 30$ — the implied modulus is $5^{m} \cdot 88$, and the corresponding parallax would be $0'' \cdot 0067$.

I am indebted to the referee for pointing out that the minimum masses required by the orbit for the members of the 206-day binary, 1·01 and 0·89 $M_{\odot}$, are just about the masses that stars of their types are expected to possess. That equality leads to the conclusion that $\sin^3 i \sim 1$, and *a fortiori* $\sin i \sim 1$, and thus to the possibility of eclipses, although since the separation of the stars even at the more favourable conjunction is about 120 Gm and the sum of their radii is expected to be only about 1·3 Gm the likelihood of eclipses is small: the inclination would need to be within about arcsin(1·3/120), or about 0°·6, of 90° for eclipses to take place.

References

(1) R. F. Griffin, *The Observatory*, **125**, 81, 2005 (Paper 181).
(2) P. Couteau, *A&AS*, **5**, 167, 1972.
(3) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997.
(4) *SAO Star Catalog* (Smithsonian Institution, Washington, D.C.), 1966, vol. **3**.
(5) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(6) R. F. Griffin, *The Observatory*, **100**, 193, 1980 (Paper 35).
(7) *e.g.*, D. W. Latham *et al.*, *AJ*, **96**, 567, 1988.
(8) F. G. W. Struve, *Catalogus Novus Stellarum Duplicium et Multiplicium* (Typographia Academia, Dorpat), 1827, p. 73 (HD 213014); p. 69 (HR 8082 B).
(9) Bessel, *AN*, **4**, 301, 1825.
(10) F. G. W. Struve, *Stellarum Duplicium et Multiplicium Mensurae Micrometricae per Magnum Fraunhoferi Tubum Annis a 1824 ad 1837 in Specula Dorpatensis* (Typographia Academia, Petropolis), 1837, p. 163 (HD 213014); p. XXXIV (HR 8082 B).
(11) Ref. 3, **10**, p. DC424.
(12) S. W. Burnham, *General Catalogue of Double Stars Within 121° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1906, part II, p. 984.
(13) R. G. Aitken, *New General Catalogue of Double Stars Within 120° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1932, **2**, 1384.
(14) Ref. 3, **1**, 57.
(15) I. Halliday, *ApJ*, **122**, 222, 1955.
(16) C. B. Stephenson, *AJ*, **65**, 60, 1960.
(17) Ref. 3, **9**, 2228.
(18) J. A. Pearce, *Trans. IAU*, **9**, 441, 1957.
(19) A. H. Batten, *Bull. Inf. CDS*, no. 24, 3, 1983.
(20) [Announced by] S. Grenier *et al.*, *A&AS*, **137**, 451, 1999.
(21) M. Jura *et al.*, *ApJ*, **485**, 341, 1997.
(22) R. D. Jeffries, *MNRAS*, **304**, 821, 1999.
(23) J. R. Barnes *et al.*, *MNRAS*, **324**, 231, 2001.
(24) O. J. Eggen, *PASP*, **102**, 242, 1990.
(25) R. F. Griffin, *MNRAS*, **155**, 1, 1971.
(26) R. F. Griffin, *The Observatory*, **126**, 338, 2006 (Paper 190).
(27) D. Dravins, *A&A*, **228**, 203, 1990.
(28) R. F. Griffin & A. A. Suchkov, *ApJS*, **147**, 103, 2003.
(29) J. Andersen, *A&A Review*, **3**, 91, 1991.
(30) C. W. Allen, *Astrophysical Quantities* (Athlone, London), 1973.
(31) K. Schaifers & H. H. Voigt (eds.), *Landolt-Börnstein* (Springer-Verlag, Berlin), **2b**, 1982.
(32) K. R. Lang, *Astrophysical Data: Planets & Stars* (Springer-Verlag, New York), 1992.
(33) A. N. Cox (ed.), *Allen's Astrophysical Quantities*, 4th Edition (Springer-Verlag, New York), 2000.
(34) P. Hut, *A&A*, **99**, 126, 1981.
(35) A. Duquennoy, M. Mayor & J.-C. Mermilliod, in A. Duquennoy & M. Mayor (eds.), *Binaries as Tracers of Stellar Formation* (CUP, Cambridge), 1992, p. 52.
(36) Y. Kozai, *AJ*, **67**, 591, 1962.
(37) R. F. Griffin, *MNRAS*, **120**, 287, 1960.
(38) R. E. Wilson, *General Catalogue of Stellar Radial Velocities* (Carnegie Institution of Washington, Washington, D.C.), 1953.
(39) R. K. Young & W. E. Harper, *PDAO*, **3**, 1, 1924.

(40) T. J. Deeming, *MNRAS*, **121**, 52, 1960.
(41) A. N. Argue, *MNRAS*, **125**, 557, 1963.
(42) Ref. 3, **9**, 2092.
(43) R. F. Griffin, *The Observatory*, **122**, 329, 2002 (Paper 167; see footnote, pp. 330/1).
(44) K. M. Yoss, *ApJ*, **134**, 809, 1961.
(45) W. E. Harper, *PDAO*, **6**, 149, 1936.
(46) [Announced by] B. Famaey *et al.*, *A&A*, **430,** 165, 2005.
(47) O. Stone, *Publ. Cincinnati Obs.*, no. 5, 133, 1879.
(48) H. M. Jeffers, W. H. van den Bos & F. M. Greeby, *PLO*, **21**, Part II, 704, 1963.
(49) B. Boss, *General Catalogue of 33342 Stars for the Epoch 1950* (Carnegie Institution of Washington, Washington, D. C.), 1937, **5**, 198; **1**, 46.
(50) F. W. A. Argelander, *Astronomische Beobachtungen auf der Sternwarte zu Bonn* (Adolph Marcus, Bonn), **4**, 118, 1861.
(51) E. C. Pickering, *HA*, **72**, no. 6, 191, 1913.
(52) A. N. Vyssotsky & A. G. A. Balz, Jr., *Publ. Leander McCormick Obs.*, **13**, Part II, p. 81, 1958.

REVIEWS

Traveller's Guide to the Solar System, by G. Sparrow (HarperCollins, London), 2006. Pp. 224, 19·5 × 5·5 cm. Price £12·99 (paperback ; ISBN 0 007 23410 4).

For those of us familiar with serious, no-nonsense texts describing the wonders of our Solar System, this little book comes as something of a culture shock. This is not to say that the *Traveller's Guide* does not contain its fair quota of facts, but the fairly light-hearted way in which they are presented is a far cry from the science books I remember from my youth. A taste of what is to come is provided by the book's sub-title: "Long-haul Holidays and Mini-breaks in our Cosmic Neighbourhood". Since we are still some years away from mass space tourism and holidays beyond the asteroid belt are unlikely in the foreseeable future, Giles Sparrow has inevitably used artistic licence to write this book for a late-21st-Century readership. Hence, potential planetary tourists are told to peer at the Apollo landing sites through plexiglass screens and to visit the crash site of the *Beagle 2* lander on Mars.

In addition to the concise descriptions of the major planets, asteroids, and comets, the *Guide* includes two introductory chapters that summarize the basics of planetary motion, navigation, and space travel. There is also a chapter with some historical background and comments about living conditions in space. The *Guide* is easy to read and well produced, with colourful icons and numerous colour illustrations, including many computer reconstructions. The main text is also broken up by numerous side-boxes that illuminate various aspects of each topic.

Some of the phraseology I found rather cringe-making, *e.g.*, "traditional propellants were as weak as a kitten" and "an eccentric British scientist (is there any other kind?)". References to the pop group Pink Floyd and "recreating the famous space docking scene from *Diamonds are Forever* with a slowly spinning banana" made me wonder for which age group this *Guide* was intended. However, as a reasonably priced, popular-level, and up-to-date introduction to the Solar System, the book can be highly recommended. — PETER BOND.

The Electric Sky: A Challenge to the Myths of Modern Astronomy, by D. E. Scott (Mikamar Publishing, Portland, Oregon), 2006. Pp. 248, 23 × 15 cm. Price $25 (about £13) (paperback; ISBN 0 977 28511 1).

Last night there was a rather interesting programme on television strongly suggesting that our current concern over global warming caused by human activity is complete nonsense (*The Great Global Warming Swindle*, Channel 4, 2007 March 8, at 9 p.m.). This claim was backed by a number of apparently eminent scientists, some of whom were members of the Intergovernmental Panel on Climate Change (even if they are not now!). Their thesis was that CO_2 is a relatively minor greenhouse gas (water is more significant, for example) and that the trend in global warming, at least recently, doesn't correlate with increased industrial activity; rather they suggest that natural causes, such as solar activity, are responsible and that, anyway, the climate is *always* changing. More to the point of this review, they argued that 'climate change' is now such a major bandwagon that to secure research funding — and there promises to be a lot of it around — requires adherence to the prevailing dogma. In this respect, the programme had much in common with *The Electric Sky*.

The Observatory Magazine is no stranger to the notion that electrical forces play a much more significant rôle in the Universe than is conceded by the large majority of astronomers. Between 1946 and 1975, a pioneer of the subject, C. E. R. Bruce, published no less than ten communications on the subject in these pages, and between 1974 and 1990, Bruce's 'disciple', the late Eric Crew, continued the campaign with vigour. In 2000, Mr. Crew reviewed Körtvélyessy's *The Electric Universe* here (**120**, 338) and gave it a warm welcome. I'm not so sure how he would have reacted to Donald Scott's book.

At first I warmed to *The Electric Sky,* as Scott, in the early chapters, expounds some sound scientific principles, in particular the need to expose all theories to rigorous and continuous experimental testing. He then wades into the modern astrophysical establishment with a blistering attack on many of the current articles of faith of our science: string theory, multi-dimensional universes, the Big Bang, black holes, dark matter, dark energy, *etc.*, *etc.*, most of which have certainly not been subjected to experimental confirmation, and I found myself cheering him on. He then tells us where he believes the real answers are to be found: in the application of sound electrical theory from the likes of Maxwell, Alfvén, Birkeland, and even our own C. E. R. Bruce, to that well-established and, I think, widely accepted component of the cosmos, plasma.

However, the problem seems to me to arise when the entirely reasonable results from the laboratory are naïvely extended to the vast stage of astronomy — which, of course, renders them somewhat difficult to test in the objective way that Scott would presumably like. While electrical forces must have a part to play on the small scale, it is far from clear to me how charge separation can occur on the astronomical scale to make stars and galaxies behave like the discharge tubes and homopolar motors one would find in a school science laboratory.

It was from that point I began to cool towards *The Electric Sky*. According to Scott, the 'Electric Sun' is powered not by fusion reactions in its core but by energy from galactic electric currents (evidence for which seems to me to be entirely lacking); indeed the centre of the Sun is deemed not to be especially hot at all and what nuclear processing does take place is supposed to do so in *z*-pinch regions on the surface. I must confess that I always thought the B^2FH scheme of element production rather convincing.

Moving out from the surface of the Sun, Scott is right to question the glib use by astronomers of magnetic reconnection to deliver energy to various parts of the heliosphere but I can't really claim to have found any serious alternatives in the text; certainly no *quantitative* schemes are presented and one has to be content with hand-waving arguments and homely analogies aimed more, I feel, to keep the layman on board. The rôle of electricity throughout the Solar System is portrayed in much the same way, and by this time I was ready to agree with David Hughes' comment (on page 147) in relation to an electrical model of currents flowing in and out of comets as "… complete cobblers" and "Absolute balderdash"!

Out in the sidereal Universe, stars — as with the Sun — are caused to shine by the galactic currents, rather like a collection of light bulbs (hence throwing much of our stellar-structure and stellar-evolution theory into the bin), meaning that the H–R diagram evidently has nothing to do with a star's mass; so once again I was shocked to find my dearly-held notions rudely overturned. Binary stars form simply by getting over-charged-up with electricity — bang goes another one of my favourite paradigms. And then on to the grand scale of galaxies — which turn out to be nothing more than homopolar motors, such as you'd find in your electricity meter.

But at this late stage in the book I began to warm again because we are getting into areas of astrophysics way outside my traditional stomping ground — quasars, AGN, and the 'big picture'. The much (and perhaps unjustly) criticized work of Halton Arp is given an enthusiastic airing and the text concludes with attack on the 'thundering herd', the establishment that seems blinkered to unorthodox ideas and the funding agencies that seem prepared only to sponsor research that will confirm pre-conceived notions.

I can't honestly say that the hectoring style and the extreme extrapolation of everyday electrical science to the wider Universe has much appeal to me — or will be very convincing to the general astronomical readership — but a few telling punches are landed and the mainstream proponents of our subject should take note that not everyone is happy with the direction we are taking or the constraints placed upon free thinking. — DAVID STICKLAND.

Handbook of Space Astronomy and Astrophysics, 3rd Edition, by M. V. Zombeck (Cambridge University Press), 2006. Pp. 767, 23·5 × 15·5 cm. Price £50/$95 (hardbound; ISBN 0 521 78242 2).

I have to confess to having a soft-spot for this book. In its various incarnations it has been a constant companion for my entire scientific career, from the original Smithsonian Astronomical Observatory report through to the very well-thumbed and rather decrepit 2nd edition that currently resides on the book-shelf in my office. Nevertheless, in this review, I have attempted to set personal feelings aside and examine objectively whether or not this new version of the handbook is an improvement on earlier issues and whether or not it is worth buying.

The first CUP edition of the handbook received a fairly negative reception in its 1983 review in these pages (**103**, 261, 1983) and was compared rather unfavourably with Allen's *Astrophysical Quantities*. With hindsight, many of the criticisms levelled then were reasonable, with patchy coverage, a strong X-ray bias, and haphazard layout being among the most serious problems. It is likely that my affection for the contents arises from my own background in X-ray astronomy and the usefulness of book for those of us involved in building instrumentation. Perhaps this is where the earlier review slightly missed the point. The *Handbook of Space Astronomy and Astrophysics* is not an attempt to replicate 'Allen'.

It does include quite a large quantity of similar astrophysical data (some taken directly from *Astrophysical Quantities*) but also a lot more related to astronomical instrumentation, the physics of devices, and space physics, which can only otherwise be found by searching a number of reference sources.

Compared to the last edition, the new version of the handbook has almost double the number of pages and represents a considerable expansion of the content of each chapter. Examples of improvement include updated lists of Solar System natural satellites and high-redshift objects. There are also completely new sections such as the list of extra-solar planets, none of which had been detected when the last edition was produced. However, some sections remain irritatingly limited and some are quite outdated. The precision of the wavelengths quoted for UV spectral features has never matched the spectral resolution of the available instrumentation and still does not, so the information is not at all useful for line-identification purposes. The maps of interstellar column density are more than 20 years out of date and have been superseded by considerably more detailed information in the last few years, based particularly on ground-based sodium measurements and *Far Ultraviolet Spectroscopic Explorer* studies, a mission which is not mentioned at all. In general, the data on space instrumentation still betrays something of a US-centric bias. Yes, ESA's *XMM–Newton Observatory* is mentioned along with the NASA *Chandra* mission, in the X-ray chapter, but there is no mention of *ISO* in the infrared-astronomy section and only EUV sources detected by the *Extreme Ultraviolet Explorer* are listed, when the first EUV survey was carried out by the *ROSAT Wide Field Camera*.

So, the *Handbook of Space and Astronomy* carries some of its original flaws like a badge of honour. Coverage of topics within the various chapters remains patchy. Also, I think the book would benefit from a more thorough update of the original material, not just the addition of new items, which are the main changes in this 3rd edition. Nevertheless, there is a lot of material, particularly that pertaining to the basic tools of an instrument developer and observer in space astronomy, that, to my knowledge, is not usefully collected in any other single volume. It is certainly worth purchasing by anyone wanting a ready reference for basic instrumentation and observational techniques, although it is probably most useful for scientists working in high-energy astrophysics. I have no doubt that my copy of the 3rd edition will be as well-used as the previous one. — MARTIN BARSTOW.

Evolutionary Processes in Binary and Multiple Stars, by P. Eggleton (Cambridge University Press), 2006. Pp. 322, 25·5 × 18 cm. Price £65/$120 (hardbound; ISBN 0 521 85557 8).

It is a common illusion amongst astronomers who do not work in the field of stellar structure that stars are well understood and that all that is now needed is some tidying up around the edges, although perhaps it is recognized that evolution in binary star systems is less well understood. Anyone who reads this book will soon shed that illusion, even for single stars. There are many textbooks on stellar structure and evolution, some of which devote some space to binaries; there are several excellent textbooks on binary stars, most of which include a discussion of evolution. However, I know of no book quite like Peter Eggleton's monograph, which describes in great detail stellar evolution in the context of binary and to some extent multiple stars, but also gives a masterly and comprehensive one-chapter summary of single-star evolution, in a very concise style. Anyone who masters the contents will have a deep understanding of the processes involved and of the approximations that are necessary to make progress. But it will have been necessary to read every word with care, for the style throughout is very condensed. Despite that, the writing is clear and readable, even if some paragraphs need to be read more than once for the details of the argument to be appreciated.

This is not a book for the faint-hearted, and I think the author is optimistic when he suggests in his preface that it might be suitable for final-year undergraduates; to my mind, it is very much a graduate text. Nonetheless, the text is full of nice phrases that are accessible to everyone, although one of my colleagues might object to the phrase "magnetic fields, the last refuge of the charlatan"! Less controversially but imaginatively, Eggleton points up the dangers of using plane-parallel-atmosphere theory for giant stars, saying (p. 107) that "the atmosphere of a red supergiant may more closely resemble the flames from a log fire than the surface of an electric hot-plate".

A nice feature of the book, that will make it more accessible to non-experts, is that the detailed mathematical justifications have mostly been placed in the six appendices that take up the last 50 pages of the book; if the reader is happy to take the quoted results on trust, the appendices can be left alone. But anyone who wants or needs to know more can find the necessary details, albeit in a very condensed form. Experts will appreciate the fact that this is by no means just a review of the literature; indeed, the references to the literature are economical. Rather, it is a personal perspective on stellar evolution, with substantial original material, both in content and in presentation. As an example of the latter, section 3·3 contains the most detailed and helpful analysis I have seen of the response of stars to mass loss and mass gain. On content, Eggleton includes in section 4·5 his own semi-empirical dimensional model of dynamo activity, in the absence of a detailed theory in the literature. Similarly, he has his own take on empirical formulae for mass-loss rates (pp. 100 and 187).

In section 2·6, there is a nice list of eight fundamental unsolved (but interdependent) problems in the theory of single-star evolution, from a model for convection to a model for dynamo activity. A similar but longer list of eighteen problems relating to binary evolution concludes the book, which goes out of its way to look at evolution, warts and all — there is no attempt to sweep the difficulties under the carpet. It also faces squarely the very complicated set of possibilities that arise in binary- and multiple-star evolution, from various modes of mass transfer to a variety of dynamical encounters, and Eggleton has devised an abbreviated notation for describing these. This does allow a compact description of an evolutionary history (*e.g.*, *MMS;F1* → *MMD* → *MMS;F1*), but requires the reader to become thoroughly familiar with the notation before it is possible to read the description fluently. The abbreviations are clearly tabulated in Chapter 3, but I was still unable to interpret the 'State' column in Table 6·3. It may be that there has been an oversight, and the abbreviations used there have simply not been defined; certainly there are a few other minor errors of a proof-reading kind (*e.g.*, on p. 291 the reference to Fig. 3·7 should be to Fig. 4·5). I was also sorry to see that the 2001 edition had not been updated to include the resolution of the solar neutrino problem; the discussion on p. 54 makes no mention of the *SNO* results that clearly demonstrated the long-expected need for neutrino oscillations. And I would dispute the claim (p. 17) that the Sun "may well fill the orbit of Mars or even Jupiter before collapsing to a white dwarf"; that is only true if mass loss is neglected.

However, these are minor quibbles in what is a remarkable book. It is by no means an easy read, but it is authoritative and comprehensive and will be a fruitful source of ideas for those working in the field. It should also persuade our extragalactic colleagues that stellar evolution is still a topic with lots of interesting unsolved problems. — ROBERT CONNON SMITH.

The Hands-On Guide for Science Communicators — A Step-by-Step Approach to Public Outreach, by Lars Lindberg Christensen (Springer, New York), 2007. Pp. 270, 23·5 × 18 cm. Price €24·95/£19·50) (paperback; ISBN 0 387 26324 1).

Here is a milestone of its kind that can be recommended to anybody involved, to any degree, in scientific communication, particularly in astronomy. This guide offers practical advice on all aspects of it. It starts with generalities on science communication, as well as on the communication process and office, before moving to an extensive section related to the production sequence (chain, target groups, product types, written and visual communications, distribution, evaluation, and archiving). Another substantial part of the book deals with selected topics such as websites, video production, crisis communication, guidelines, press conferences, national and cultural differences (rarely tackled, but so important in areas such as Europe), commercial aspects, credibility issues (see below), and a couple of case studies. The guide is completed by a well-furnished glossary, an index, and lists of references and web links.

Discussing credibility issues is new, but has become necessary after a couple of infamous cases (remember the meteorite with life from Mars?). Credibility has a number of aspects. Some see it as resulting from the honesty of the source (or of the public-relations (PR) officer), while, for this reviewer, it occurs if the message conveyed has been accepted as believable by the receiver — which is no excuse for deliberate cheating or avoiding exposing possibly misleading information. In other words, it is not enough to be honest: one is largely responsible to tailor messages in such a way that they are correctly received.

Why do PR people make mistakes? For a number of reasons: they might be fresh and inexperienced, or incompetent, or poorly informed, or because they are not doing their homework properly, *i.e.*, checking and verifying their information sources. They might also think it is not important to use the right wording. PR people might also be submitted to pressures — personal, institutional, and temporal. But it is part of their job, as well as a sign of maturity and professionalism, to resist and overcome these. They might also be 'intoxicated' by their sources, typically by scientists who can tell stories the contextual importance of which is difficult to assess correctly. PR people cannot be competent in all the sub-disciplines covered by their institutions, but they should get the substance double-checked.

And it is not because things are done according to the book that they come out right every time. The usual guidelines can indeed be inadequate for a specific case, or in need of some upgrade — the basis of evolution and adaptation.

Now, why should one be extremely cautious when analyzing cases of hyping and avoid behaving as prosecutors? Simply because one is dealing with human material and that nothing is simple! Behind those entities called 'NASA', 'ESA', *etc.* (so often in the limelight), are human complexities, often with internal conflicting interests, from scientists to managers *via* PR officers, each one obeying their own dynamics and trying to deal with internal and external pressures. Years ago, in the infamous case of the discovery of Mars-based life, who was to be blamed for a hyping announcement made shortly before an approval vote for a NASA budget? The scientists? The PR people? NASA's management? The gullibility of the media? The US President's cabinet (who issued a supporting press release)? Or were they all benevolent accomplices? After the striking headlines, few readers noticed the rectifying statement (when published) in the inside pages of newspapers and magazines. But interested people noticed that the original information was wrong and they remember it. Some of our colleagues still make the mistake of believing that the taxpayers supporting most of our activities have limited intelligence or memory.

Why should hype and credibility be issues in astronomy communication and why should we worry at all about such things? After all, our disciplines have not the criticality of, for instance, life sciences that are often referred to ethical committees. Well, in the first instance, we all should be committed to truth and verified knowledge. Beyond this, wrong hype does affect the image of our community as a whole. Worse, it affects credibility in a way that has been so far unquantifiable. And at a time when we are fighting for money and positions more than ever, this is definitely something we should care about. Should we also have an ethical committee? Such a label probably sounds too strong, but perhaps a kind of working group might provide some guidelines on the matter, possibly also formulating recommendations and reference charters. At a recent space-related forum, a prominent scientific reporter said, in substance, that his job was primarily to help his newspaper make money. This reviewer hopes that we, in the scientific world, still rate the conveyance of correct information first and foremost.

From the Space Telescope European Coordinating Facility (ST-ECF) in Garching, Lars Lindberg Christensen, the author of the guide reviewed here, is managing European communication for the *Hubble Space Telescope* and the *James Webb Space Telescope*. He is also the press officer for the International Astronomical Union (IAU) based in Paris. He was the youngest recipient (in 2005) of the Tycho Brahe Medal for scientific communication awarded by the Bodil Pedersen Foundation. Among other things, he is also a founding member and the secretary of the IAU Working Group on 'Communicating Astronomy with the Public'. An example of his successful activities can be seen in the release of some 750 000 copies of the science documentary movie *Hubble — 15 Years of Discovery* that he produced and directed.

The book is very well presented, written in a good-humoured style, with an abundance of illustrations and practical details, together with examples from physics and astronomy. It is a fundamental contribution to the field of scientific communication towards the public and is *de facto* filling a gap. If it is openly aimed at public outreach, this book will also be useful to the ordinary scientist in their daily professional communications. Hence my advice: do get this jewel on your desk and read a few pages regularly! — A. HECK.

Mastering Your PhD, by P. Gosling & B. Noordam (Springer, Heidelberg), 2006. Pp. 166, 19 × 12·5 cm. Price £19·50/$39·95/€29·95 (paperback; ISBN 3 540 33387 8).

I am a third-year PhD student, and when I was asked to review this book I was curious as to how much of the advice within it I had unknowingly followed over the last three years, and how much advice it had to offer me now, particularly as I am writing my thesis.

The book is easy to read and is split into nineteen short chapters that aim to take the reader chronologically through their time as a PhD student. The format makes it easy to dip in and out of, reading the sections that are relevant at the time. It is aimed at those undertaking a science PhD, although actually the majority of examples refer to laboratory-based research. Most chapters have their main points highlighted by examples, describing a team of students restoring a piece of artwork, for instance, which tend to be useful.

In the preface the authors state that they wanted to write a book describing how to obtain the skills needed for research that are not specifically taught, from whom to introduce yourself to on your first day to preparation for thesis defence (the book is written using American terminology). The book does exactly that. There is guidance at every step: how to handle your first conference, writing papers, dealing with success and setbacks, and writing your thesis, with much emphasis placed on organizational skills, list making, and reviews of progress. While a lot of this advice is useful, most of it is common sense, and probably has, or should have, been provided to the student by their supervisor, or in training sessions provided by the research group or similar. However, those students who have largely absent or unapproachable supervisors may find the advice presented here useful. I would also recommend the book as a useful guide for prospective PhD students who are not sure of what a PhD actually entails, and what is expected of you as a student, in particular the advice that this is *your* PhD and you are expected to take charge at some point, not just blindly follow your supervisor's instructions.

I will use some of the suggestions from the book to help me with my thesis writing and viva preparation; however, the majority of the advice in the book has already been provided by my supervisor. — SARAH CASEWELL.

Philip's Complete Guide to Stargazing, by R. Scagell (Philip's, London), 2006. Pp. 320, 34 × 26 cm. Price £25 (hardbound; ISBN 0 540 08937 0).

There are a plethora of books out there whose purpose is to introduce the night sky to the novice stargazer. It can be difficult to choose one. If I was going to buy a book to introduce someone to the night sky, the question I would ask myself is, "does this book stand out from the rest?" One of the common problems with this type of 'coffee-table' book is the abundance of beautiful images, often taken with the most sophisticated equipment and processed with specialist software. The reader is left with an unrealistic sense of expectation when he looks through an amateur telescope for the first time. In many cases, there is an overwhelming sense of disappointment. The only object that really lives up to his expectations is the Moon.

A preliminary look through *Philip's Complete Guide to Stargazing* suggests that the book is a well-presented introduction to what the night sky can offer the would-be amateur astronomer. The book is split into several sections. The first 20 or so pages provide an introduction to astronomy and how astronomers observe the Universe. It covers the movement of sky and the objects we see in it. There is a limit to what can be covered in this number of pages but the introduction is a reasonable one.

The next section covers the sort of instrumentation the amateur might use, including the naked eye, binoculars, different types of telescopes, their mountings and ancillary equipment. There are useful comparisons between the different types of telescopes and advice on setting them up. The availability of modern technology, such as laptops and webcams, can make a real difference in entry-level astronomy and the author includes material on how to use these technologies with amateur-sized telescopes, giving some examples of the results one might obtain.

The sections on the Moon and the Solar System are well illustrated and include around 50 pages of useful information on how to observe the Sun, Moon, and planets. They include imaging from the latest space missions, and interesting subsections on solar and lunar eclipses, occultations, meteors, aurorae, and noctilucent clouds are provided. The shorter section on 'Stars and deep sky objects' illustrates the different stages of stellar evolution, including an overview of the different types of galaxies through to the various members of the stellar 'zoo'. Once again, the illustrations are good and there are hints and tips on observing and photographing/imaging these objects.

Monthly sky maps are also provided in pairs of months for both the northern and southern hemispheres. Also included are subsections focussing on constellations prominent in each pair of months. Rewarding objects for amateur observers are highlighted and constellation maps showing their location are provided. Star maps and photo-realistic maps are also given at the end of the 70-page section to give the observer a better idea of what he will really see.

The final 100 or so pages of the book are given over to an A–Z of astronomy. This may seem a little excessive, but to the novice this may well prove very useful. The entries have been cross-referenced well and cover a wide range of subject areas including historical astronomy, technology, and space missions as well as the basic set of general astronomical definitions you might expect.

For someone like me, who has been fortunate enough to use telescopes at some of the best observing sites in the world, it would be all too easy to take the night sky for granted. One must remember that for most people, the night sky is a few bright stars in the orange glow of streetlights. They may never see the full glory of a really dark night sky. Despite a few typographical errors here and there, this book provides a good introduction to astronomy and will hopefully spur people on to discover the delights of the night sky for themselves. Although the cover price of this book is £25, I found it was available for as little as £17, and at this price, it has to be a bargain. — STEVE BELL.

New Horizons in Astronomy: Frank N. Bash Symposium 2005 (ASP Conference Series, Vol. 352), edited by S. J. Kannappan *et al.* (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 304, 23·5 × 16 cm. Price $77 (about £40) (hardbound; ISBN 1 583 81220 2).

The Frank N. Bash Symposium 2005 was the second in what is intended to be a biennial series of conferences. The first, Bashfest 2003, was a conference to honour Frank N. Bash, the retiring Director of the McDonald Observatory, and was devoted entirely to the work and ideas of young researchers. This second conference was not only devoted entirely to the work of young astronomers, but was also organized by postdocs at the University of Texas, some of whom also edited this book.

The conference proceedings themselves are made up of two parts. The first consists of series of review articles, each of which is followed by a photograph of the author involved in a deep and meaningful discussion with another participant. The second is a series of essentially poster papers referred to as 'Research Highlights'. Since the conference was aimed at the work of young researchers rather than at a particular research area, the proceedings consist of a rather eclectic mix of papers. I haven't yet managed to think of a research area that isn't in some way included. The review papers themselves seem to be organized by distance from the Sun, starting with a discussion of small bodies in the outer Solar System, and ending with a paper on the Lyman-α forest. Other topics include star and planet formation, the local interstellar medium, accretion and jets in active galactic nuclei, and galaxy formation to name but a few.

Although one might imagine that not focussing on a particular research area somehow devalues these proceedings, I found having a book with reviews covering a wide range of research topics quite useful. I have already assigned the paper by Eric Ford on multiple-planet systems and planet formation to my students, and can imagine using some of the other papers in a similar way. Possibly the most negative thing about the book is that in reading it I came to the realization that my chances of ever being invited to a conference focussing on the work of young researchers is diminishing rapidly, and may actually have already gone. — KEN RICE.

Is Pluto a Planet? A Historical Journey through the Solar System, by D. A. Weintraub (Princeton University Press, Woodstock), 2007. Pp. 254, 24 × 16·5 cm. Price £17·95/$27·95 (hardbound; ISBN 0 691 12348 9).

Forget the science. Clearly the answer to the question, "what is a planet?", is all down to sociology, human nature, patriotism, and whether you think Pluto is in the gang, or is not. And David Weintraub (an astronomy professor at Vanderbilt University, Nashville, Tennessee) has convinced me that views as to the status of Pluto are strongly polarized on geographical lines. If you come from the USA, the land where Pluto was discovered, Pluto is *in*; it is one of the team. Let's face it, real kudos is derived by discovering a 'planet'. Discovering an adjectively compromised 'planet', be it minor, orphaned, free-floating, pulsar, belt-embedded, icy, or moon-like, just does not cut the mustard. But if the United States of America is not your home, the pressure is off. You 'out-staters' know a planet when you see one. The Ancient Greeks saw six. Later on, two more were discovered by Germans. And by 1846 September 23 you have your eight, just the right number for the walls of two rooms in a museum, and the story was over. Michael E. Brown (the leader of the team that discovered the trans-Neptunians Sedna and Quaoar) might say … "we think that letting future generations still have a shot at planet-finding is nice". Folk from outside the United States will simply say "tough"; there are no more in our Solar System to discover; the job has been done; it was all over bar the shouting in the middle of the 19th Century.

David Weintraub has written a fascinating, accessible, and eminently readable historical introduction to the development of the planetary ideal. We are taken back to the times when the Moon and Sun were planets orbiting Earth. We delve into the planetary status of bodies such as Ceres, and Pallas, and Vulcan. We admire the mathematical intricacies that led to the discovery of Neptune. We marvel at the dedication and patience of Clyde Tombaugh, and the financial beneficence of Percival Lowell. We are amazed as the mass of Pluto dwindles away between its discovery in 1930 and the discovery of its satellite in 1978. And then, horror of horrors, we see Pluto joined by a host of fellow Plutinos and finally overtaken by 2003 UB313. There is much talk of the usefulness of scientific classification and definition. But, after much discussion about upper and lower mass limits, sphericity and energy sources, resonances and companions, we end up realizing that the word 'planet' is only really useful if it is qualified by a host of adjectives such as terrestrial and gas-giant.

Weintraub is to be congratulated on providing such a revealing insight into the trials, tribulations, aspirations, arguments, and confusions behind astronomical endeavour. But, in answering his initial question, we should have guessed that there would be trouble after naming something after a Walt Disney doggy. — DAVID W. HUGHES.

Science, Society, and the Search for Life in the Universe, by B. Jakosky (University of Arizona Press, Tuscon), 2006. Pp. 160, 23 × 15·5 cm. Price $17·95 (about £9·50) (paperback; ISBN 0 816 52613 3).

This book explores the relationship between science and society, focussing on astrobiology in particular and using that field as an example. In the process, Jakosky also explains what is known as the philosophy of science, which includes an investigation into why we do science and how discoveries can be classified into different types. His goal, as he states in his preface, is to explore the philosophical and societal issues in astrobiology. This emphasizes encouraging the specialists in science and the humanities to communicate more effectively, with the idea that both sides will benefit greatly from doing so.

He begins on a scientific note, by introducing astrobiology as a popular science and then discusses deeper questions such as determining what life is, and whether or not astrobiology is indeed even a science. Following on with this background fresh in the reader's mind he starts analyzing science from a philosophical and societal point of view, pointing out problems relating to the lack of understanding by the public of what happens in the scientific world, and the contradictions that exist between the public's interest and the lack of interactive communication from the scientific community. Naturally, the book is biassed towards the scientific element, as this is where the author's strengths lie. However, to complement this he admits his previous lack of knowledge regarding philosophy and society, which is greatly beneficial to the scientific reader who likewise will usually have a limited knowledge. My only criticism is that when discussing the core aspects of philosophy, he doesn't challenge them or discuss them subjectively.

I very much enjoyed this book; the majority of what the author said was insightful and certainly makes one realize that the scientific community as a whole (not just in the field of astrobiology) needs to make more effort to involve the public in major scientific endeavours and discoveries. The title could be seen as slightly misleading, as he only really considers the involvement and implications of astrobiology in society towards the end of the book. However, when he does, he raises some important issues, within which he illustrates that our preconceptions could often be wrong about what it would mean to society and religion if we found extraterrestrial life, and that scientists can be very ignorant when it comes to communicating to the public.

To finish there is an extensive list of references, which would prove extremely useful to anyone who is so convinced of the need to integrate society and science that they wish to pursue it further. — Claire Cousins.

Observational Astronomy, 2nd Edition, by D. S. Birney, G. Gonzalez & D. Oesper (Cambridge University Press), 2006. Pp. 312, 25·5 × 18 cm. Price £30/$50 (hardbound; ISBN 0 521 85370 2).

There are relatively few books available on observational techniques aimed at the undergraduate student of astronomy. This very same point was made in the preface to the first edition of this book when it appeared in 1991, under the sole authorship of D. Scott Birney, and the text soon became a staple resource. Unfortunately, parts of that text were already out of date within just a few years, when the development of CCDs and data-reduction techniques had largely replaced photographic methods in astronomy, and 'Birney' went out of print in the late 1990s. This meant that the lack of a comprehensive 'hands-on' textbook for this readership has persisted, with only one or two other titles occasionally helping to plug the gap. It is therefore timely that Birney's original text has been revised and updated, and suits once again its original purpose to provide a reference source for undergraduates and beginning graduate students in need of some background on observational methods.

The preface to this second edition notes that it is the fruit of the Master's thesis project of the third author, David Oesper. To bring the book up to date, all discussion of photographic techniques in photometry, astrometry, and spectroscopy has been excised, along with discussion of classical astronomical instruments, in favour of CCD techniques. The organization of the revised material works well, working from early chapters on celestial coordinates and time, with the chapter on charts and catalogues being updated to include references to important databases such as *Hipparcos* and *Tycho*, recent sky atlases derived from them, and the Sloan Digital Sky Survey. As is fitting for today's e-aware student, the availability of electronic catalogues and databases, such as provided by the CDS in Strasbourg, is highlighted, and there is discussion of planetarium-type software for visualizing the sky or generating customized charts.

A new chapter on quantifying light and that on optical telescopes provide good, solid food for the beginning undergraduate, introducing the magnitude scale and its relation to flux, and filter systems. The different types of telescope mount, and their various attendant focal stations, are introduced, along with formulae for image scale and brightness, discussion of aberrations, and eyepieces. The text never dragged, though I was familiar with much of it, and a student will find the underlying principles are clearly explained.

The following chapter considers the effects of the atmosphere, bringing together very well the range of problems faced by Earth-bound observers and how to deal with them. There are issues which students at various levels of application need to understand, from a description of seeing and basic refraction corrections, and extinction and its colour dependence, well illustrated here, to more advanced considerations such as differential refraction across a field and its effect on astrometric measurements, and quantifying the effect of scintillation in stellar photometry.

An entirely new chapter on detectors of light begins with a description of the eye, with photography now playing its part in a historical narrative that leads on to photoelectric detectors and image intensifiers, culminating, of course, with the charge-coupled device. The nuts and bolts of handling CCD data — dark and flat-field corrections, noise, gain, *etc.* — are then introduced, before two relatively concise chapters which condense well the essential 'how-to' of astronomical photometry and astrometry. Along with some quite practical advice on obtaining high-quality data, a student will find particularly helpful the real-world cases of differential photometry, making use of the authors' own data, in two up-to-date projects which each present their own particular challenges: the detection of the transit of the extrasolar planet TrES-1b, and observation of a stellar occultation by the asteroid 522 Helga. Another example demonstrates astrometric measurement, showing how the proper motion of Luyten's star can be detected using a 24-inch telescope, equipped with a CCD, in just one week!

The chapters on spectrographs and astronomical spectroscopy lay some essential foundations, including a stimulating, up-to-date application of spectroscopy in the measurement of radial velocity (extrasolar planets again), and a description of line-profile and abundance analysis in stars. The main types of variable star, and matters related to their observation (ephemerides and O–C diagrams, the perils of aliasing), are then served up briefly, before the final chapter on observing the Sun.

Every chapter includes a list of further reading, and most of the more detailed texts to which I would direct a student have been included, so the book will serve well as a first port of call on the more advanced topics. Further, one appendix (of three) lists internet and software resources; but although this is probably the part of the book that is at risk of dating most quickly, I was disappointed that most chapters referenced little internet material. For example, the chapter on the Sun now includes a brief mention of the highly-productive *SOHO* mission, but no link is made to its richly-resourced web-site (sohowww.nascom.nasa.gov) — which I encourage students to access to compare near-real-time spacecraft observations with their own (filtered telescopic) observations of the Sun, or to keep an eye on current solar activity.

There is not much else to criticize here: the illustration in Fig. 12·7 of the light path in an echelle spectrograph is confusing, I think; and I came across only one error which I recognized, in Chapter 2 — when a sidereal-time calculation yields a result greater than 24h, the calendar date should *not* be advanced by one day (p. 27), an error inherited from the first edition.

I will recommend this book highly to my students. Although the stated target readership is "upper level" undergraduates and higher, there is certainly much material suitable also for a (UK) first-year undergraduate. At least one other recent text[1] certainly caters for the beginning undergraduate observer, while another[2] is an established reference source on instrumentation and multi-wavelength astronomy for higher-level courses; but I don't think any other book on optical observational astronomy has quite the focus, and yet range, as this — fundamental concepts, practical relevance, examples of application, and material which will remain useful to the more advanced student, all in a concisely packaged volume. Birney's back. — STEVE FOSSEY.

References

(1) *Observing the Universe*, by A. J. Norton (ed.) (Cambridge University Press), 2004.
(2) *Astrophysical Techniques*, 4th edition, by C. R. Kitchin (IOP Publishing, Bristol), 2003.

The New Cosmic Onion: Quarks and the Nature of the Universe, by Frank Close (CRC Press, London), 2007. Pp. 219, 23·5 × 15 cm. Price £22·99/$39·95 (paperback; ISBN 1 584 88798 2).

Anyone who has read the earlier edition (1983) of this book will happily find many familiar items in this latest incarnation, such as the endearing pictorial representations of the quarks, which have induced some of my physics students to read the book. This new edition naturally includes the developments in particle physics that have taken place in the years since 1983, and there have been plenty! To name just a few, we have discovered the top quark, measured CP violation, and discovered that neutrinos have mass. This rush of new information has had a considerable impact on our understanding of the Universe.

The author's incentive for updating his book is stated in the foreword: to explain to the public why scientists are spending so much money to build the *Large Hadron Collider*. He is extremely successful in fulfilling this aim. However, some of the new material never quite attains the flow of the original, making some of the descriptions in this edition seem slightly scattered.

The early chapters discuss the beginnings of particle physics in a largely historical order. In describing the 'particle zoo'-era of the mid-20th Century, the author does try to mitigate the confusion produced by the ostensible inconsistencies of the particle-physics models of the time, by providing some of the cohesive explanations that now are available to us. Along the road we are treated to some charming explanations and delightful quotations.

Having set the stage in the early chapters, the remainder of the book presents coherent explanations of most of the major branches of contemporary particle physics. One omission is the area of hadronic-structure functions, which has progressed dramatically in the past decade both in terms of theory and experiment. The final chapters deal with some of the latest cosmological measurements, and illustrate the growing interdependency between the fields of cosmology and particle physics.

The book's illustrations are very helpful, although the Feynman diagrams don't consistently follow the same left-to-right time-ordering convention, so be careful not to get confused! The more-technical mathematical details are typically included in 'Tables': asides set in boxes within the text. Although I heartily approve of having the details available, in some cases the Tables continued for three pages, with an interrupted half-sentence on either side, making it a bit of a search to finish the sentence. I particularly liked the clear explanation of the eight-fold way (page 70), which is a useful visual aid in categorising the hadrons.

The 'Cosmic Onion' is the author's metaphor for the layers of sub-structure within our Universe. It's a nice image, but the frequent attempts to relate each layer back to human life become somewhat repetitive and anthropocentric for my taste.

This book is recommended for anyone who is curious about our evolving Universe: lay-people, students, and professional scientists, but especially those with a penchant for understanding complicated realities from the inside-out. The mathematical level is accessible for those with A-levels. This book highlights the large overlap between cosmology, astronomy, and particle physics, whilst mainly detailing the particle-physics aspects. Hence, it would be of interest to those in the former two fields who perhaps are not up to date on the areas of overlap. — LAURA KORMOS.

Annual Review of Earth and Planetary Sciences, Vol. 34, 2006, edited by R. Jeanloz *et al.* (Annual Reviews, Palo Alto), 2006. Pp. 723, 24 × 19·5 cm. Price $205 (institutions, about £110), $85 (individuals, about £45) (hardbound; ISBN 0 824 32034 4).

When this weighty volume thudded onto my desk for review recently, my heart sank and I asked myself how I had got suckered into doing it. However, on opening the volume, my fears were replaced with pleasure at the delightful and attractive new format, which made reading and digesting the book a rewarding and enjoyable exercise.

The new, modern format includes extensive use of colour in the figures, margin boxes giving summary points, definitions and, joy of joys, explanations of acronyms. Boxes at the end of many articles give future directions and chapter summaries, and key references are flagged with comments explaining their significance. This remarkable improvement in presentation style must surely hugely increase the utility and appeal of the book. Bravo Annual Reviews. Now how about an equally attractive cover as well, next year?

The very positive impression given by the new look was quickly reinforced by the topical and intriguing collection of chapters that comprises the present volume. A good number of hot, current topics are covered, so the book is well up to date regarding the most important avenues of enquiry today. Some particularly tough nuts, which perennially seem to defy complete understanding, are the subjects of full reviews. A nice triplet of related papers discusses mass extinctions, the cause(s) of which are still not well understood (with the exception of the one currently on-going), their inverse, the faunal explosion that marked the beginning of the Phanerozoic, and the temporal resolution of the stratigraphic record. The latter is of critical importance to determining just how sudden extinctions really were.

Topics representing earthquakes and volcanoes include the remote triggering of eruptions by earthquakes — a vexed subject which, in recent years, has been more notable for the steady growth in evidence that it occurs, rather than major breakthroughs in our understanding of why this unexpected phenomenon does occur. A related subject reviewed is the catastrophic degassing of lakes and possibly the oceans, a phenomenon that has caused disastrous loss of life in recent years and is attracting increasing interest because of the possible effect of methane hydrate release on global warming. Never entirely abandoned from the scientific agenda (the stakes are too high), earthquake prediction is represented by a chapter reviewing evidence for crustal-deformation-rate changes precursory to large earthquakes.

For the planetary scientist there are useful reviews on binary minor planets, cratering of planetary surfaces by bolide impact, asteroid dynamics, and cosmic dust sampling. One chapter asks "What is a planet?" This is, of course, a good question and something that Pluto and we would very much like to know the answer to just now. For the meteorologist, chapters on weather prediction, the Earth's water cycle, and circulation in the atmosphere complete the bill of fare.

In summary, this year's *Review* is not only a lesson to us all in clarity of presentation and good use of modern printing techniques, but almost any Earth or planetary scientist will find a good number of interesting and digestible review chapters on subjects that he or she really ought to know about. I enthusiastically recommend the book. — GILLIAN R. FOULGER.

Searching for Water in the Universe, by T. Encrenaz (Springer, Heidelberg), 2007. Pp. 193, 24 × 17 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 0 387 34174 9).

If Earth were not wet, we would not be here. And when it comes to water, our planet is fortunately 'just right'. Earth is massive enough, and warm enough, both to hold on to most of its initial inventory of H_2O (even though it makes up less than 0·03% of the total mass), and to have surface H_2O in its three physical phases. Because of this we not only benefit from a warming greenhouse effect, but also the surface of our planet has been teeming with life for nearly 75% of its 4 600 000 000-year existence.

Water has many important properties. It is an extremely effective solvent, and its absorption of CO_2 has helped stabilize the surface temperature of our planet. The low viscosity has enabled it to soak efficiently into soils, and support plant life. The high specific and latent heat and the low thermal conductivity have controlled the climate due to water's thermal inertia. And finally, the fact that the density of water maximizes at 4°C has stopped oceans freezing from the bottom up, and has safeguarded life. H_2O is a vital ingredient in the chemical composition of a host of those astronomical bodies with temperatures below about 3000 K (the thermal threshold beyond which H_2O is doomed).

Professor Encrenaz, from the Observatoire de Paris-Meudon, is a very well-known Solar System research astronomer and the author of many excellent books on Solar System topics. This, her latest book, is first class, well written, beautifully illustrated, and accurately aimed at a young university/advanced amateur-astronomy readership. It starts by reviewing the ways in which water can be detected spectroscopically in the infrared, millimetric, and radio bands. It then investigates the rôle of water — snow flakes in planet formation, the source of terrestrial water, and the evidence for water in distant galaxies, in the photospheres of late-type stars, beneath the surface of Mars, in the upper atmosphere of Venus, in the cometary dirty snowballs, locked up in asteroids, meteorites, and the satellites of the outer planets, and in habitable exoplanets.

I highly recommend this thorough introduction to a vital component of the Universe and life. — DAVID W. HUGHES.

Organizations and Strategies in Astronomy, Volume 7, edited by A. Heck (Springer, Dordrecht), 2006. Pp. 594, 24·5 × 16·5 cm. Price £107·50 (hardbound; ISBN 1 402 05300 9).

The previous six volumes of Heck's *Organizations and Strategies in Astronomy* have ranged wide — selection for tenure, cosmic evolution and a Creator, Antarctica, astrobiology, and many other topics. Vol. 7 is no exception, and at least as good a dog's high tea as the others. It looks at three national communities (UK, Greece, and Ukraine); five European-focussed international organizations; methods of assigning time at three observatories; the histories of US, UK, and European journals; five data bases; the actions and fate of astronomers in the Third Reich; two aspects of the psychology of science; and ten endeavours in what is variously called education and public outreach or communicating astronomy with the public. It ends with a bibliography from 1980 to 2006 January of papers in what the editor calls "socio-dynamics of astronomy" but I would call astronomy-orientated scientometrics.

Three confessions: (*i*) the authors with the most papers in this bibliography are André Heck himself (55), Helmut Abt (51), and yours truly (14, though several in low-prestige journals, like *BeamLine*, have been missed; and this is undoubtedly true for other authors as well); (*ii*) the editor rejected a chapter I submitted to an earlier volume in the series (leaving me wanting to quote an editor of *The Journal of the Canadian Medical Association*, "We thought that removing the research left a well-balanced paper."); and (*iii*) the IAU Working Group on Communicating Astronomy with the Public was founded and upgraded to a Commission under the Division of Union-Wide Activities on my watch as President of the Division. And if you think that sentence has too many entities of which you have never heard, wait 'til you read the book!

The chapter I would guess most likely to interest readers of *The Observatory* is the one by Paul Murdin (he has two others) on British Astronomy from Stonehenge until just before the *Faulkes Telescopes* were sold down the river (or, rather, across the Atlantic). Which items, if any, you want to object to will depend on the direction and intensity of your loyalties. I was distressed to find that, in Cambridge, calculations of "the development of structure in the Universe" are done only at DAMTP and study of star formation only at MRAO. Now what was that place RGO used to be next to when RGO was next to anything?

The chapter I learned most from was Hilmar Duerbeck's discussion of German astronomy under the Third Reich. The physicists of late have given a good deal of attention to what their German colleagues did during the second great war (what did Heisenberg know about bomb-making, and when did he know it?), but this would seem to be the most complete treatment of the astronomical community since a 1946 paper by Gerard Kuiper. There are too many individuals discussed for mentioning any one to be other than invidious (but Prof. X refereed one of my early papers, and, by George, he was a Nazi!). But there is much collective information as well — that Freemasons were forbidden party membership; that the Astronomische Gesellschaft was one of many pre-armistice organizations forbidden to meet without specific permission of the post-war occupying powers; that some astronomers were put to work as code breakers in addition to the more obvious tasks in navigation (as also in the US and UK); and that, while many were expelled from their jobs by the Nazis between 1933 and 1944, and by the Allies in 1945–46, the number of astronomers actually killed seems to have been small (compared, say, to the death rate at Pulkovo Observatory under Stalin).

Do not read the brief account by Michael F. Gorman of the discovery of quasars if detailed errors bother you at least as much as defects that are merely basic and fundamental. Oh, and I suspect that we are even weirder than Gregory Feist, author of the 'Psychology of physical science' chapter thinks we are, though he does note that we tend to be rather Aspergerish. — VIRGINIA TRIMBLE.

Kommandosache 'Sonnengott' — Geschichte der deutschen Sonnenforschung im Dritten Reich und unter alliierter Besatzung, by M. P. Seiler (Acta Historica Astronomiae, Vol. 31, Verlag Harri Deutsch, Frankfurt am Main), 2007. Pp. 246, 14·5 × 21 cm. Price €22·80 (about £15) (paperback; ISBN 3 817 11797 3).

Here is a fascinating historical work that we can recommend right away to people mastering German and interested in the history of solar astronomy under National Socialism in Germany and especially during World War II. Between 1939 and 1945, under the code name *Sonnengott* (Sun God), the air force of the Third Reich, the *Luftwaffe*, heavily invested in solar research, as well as in establishing a chain of solar observatories, since at that time, the study of solar activity was presumed to allow reliable daily predictions for determining the best frequency bands for long-distance military radio communications.

During the six years of the conflict, German solar research grew (quoting the author) "from a provincial backwater to the forefront of this science", thanks basically to the joint effort of two men: Hans Plendl (1900–1991) and Karl-Otto Kiepenheuer (1910–1975). Just before the hostilities, the former was an experienced researcher who had become a key figure by designing precision-bombing aids for the *Luftwaffe*. He subsequently became Göring's plenipotentiary for high-frequency investigations, before falling in disgrace. The latter, Kiepenheuer, was a young charismatic and eloquent astrophysicist, the son of a publisher who had seen his books burned when the National Socialist Party took power in 1933.

The contents of the present book by Seiler, nuanced and without concessions, are supported by detailed evidence, including interviews with some of the survivors, as well as original documents and mail exchanges between the actors of the time. Numerous illustrations are offered and a dozen pages of bibliography enable interested readers to go deeper into the matter. One of the author's conclusions is that, if some moral aspects of the rôle played by Plendl and Kiepenheuer in the *Luftwaffe*'s war effort can be debated, it is a fact that those scientists had a lasting influence on German solar physics in the second half of the 20th Century and on the collaborations it maintained with the scientific community in Europe and in the United States — an influence that continues at the beginning of this 21st Century.

The book exposes the mutual support of the scientists during the conflict, for instance, in securing positions away from the front line, but also in obtaining substantial subsidies for investigations of a definite intrinsic interest, although of a reduced utility for the *Luftwaffe* — something that did not remain without consequences when, towards the end of World War II, the Nazi authorities realized that the money spent on establishing solar observatories here and there in Europe was totally out of proportion to the actual contribution of those to the war effort.

Besides the development of stations in Germany (Schauinsland, Wendelstein, Zugspitze), work was also carried out in conquered territories, for instance, at the observatories of Belgrade (Yugoslavia), Kanzelhöhe (Austria), Meudon (France), Pic du Midi (France), Simeis (Crimea-USSR), and Syracuse (Sicily). The rôle played by some foreign characters — such as the French Bernard Lyot (1897–1952), the Dutch-born American Gerard P. Kuiper (1905–1973), the Swiss Max Waldmeier (1912–2000), and other less-known ones — is also presented without prejudice. The last part of the book is devoted to the relationships with the allied missions (essentially American and French ones) at the end of World War II, as well as to subsequent developments. No historical work on 20th-Century solar astronomy should ignore this book. — A. Heck.

Highlights of Astronomy, Volume 13, edited by O. Engvold (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 1085, 23·5 × 15·5 cm. Price \$95 (about £53) (hardbound; ISBN 1 583 81189 3).

The IAU General Assembly held in Sydney in 2003 included not only Commission and Division business, intra-Commission scientific meetings, and major symposia, but also 21 Joint Discussions (JD) and four Special Scientific Sessions (SSS). This edition of *Highlights* includes all bar five of the full proceedings from those meetings; four of the JDs published proceedings elsewhere and at greater length than could be accommodated in *Highlights* (as it is, it runs to 1085 pages). JD5 alone did not submit any proceedings.

A JD is an inter-Commission mini-conference on a topic that usefully interests more than one Commission, and what we have here is a pretty eclectic selection. Some of the titles sound decidedly general (*e.g.*, 'Non-electromagnetic windows for astrophysics', 'Frontiers of high resolution spectroscopy', and 'Evolution in galaxy clusters: a multiwavelength approach'), while there is no doubt about the specificity of some of the others (*e.g.*, 'Quasar cores and jets', 'Atomic data for X-ray astronomy', or 'Mercury'). No fewer than five JDs are devoted to Solar System topics. The SSSs, on the other hand, are intended to be inter-Division in character; 'Astronomy in Antarctica' and 'Effective teaching and learning of astronomy' clearly are, though 'A new classification scheme for double stars' and 'Recent progress in planetary exploration' sound like misplaced JDs.

There is something new and topical for everyone in *Highlights*, be it extragalactic binaries, the evolution of dense stellar systems, astrochemistry of a wide variety of sources, large-telescope design, or the international celestial reference frame. One objective of a JD is to cross-fertilize concepts that do not ordinarily share the stage, and although the admixture of elemental abundances in old stars with damped Lyman-α systems might sound far-fetched, it proved highly fruitful in uncovering topics of common interest and concern; as the review paper pointed out, for the first time astronomers could talk *with*, instead of merely *to*, one another.

The proceedings are presented in a fairly uniform format and contain both text summaries and paper or poster abstracts. There is no thread that interweaves the whole, so each JD or SSS can be read in isolation. That may not be a bad thing, but it does mean that one can have rather a lot of extra 'luggage'. Nevertheless, *Highlights* represents a very valuable cross-section of efforts, plans, problems, and results across the whole gamut of astronomy, as well as some nicely-expressed ideas on magnetizing our science in order to attract the most gifted of our younger generations, and on fostering science in developing countries.

There is one important caveat, however. In fields such as asteroseismology or astrotomography new results can radically affect a whole line of thinking. Since the 2006 General Assembly had been and gone before this write-up of science at the 2003 one became available, the reader should bear in mind the possibility that some of the material in this volume is already dated. And, of course, for "planet" read "dwarf planet" or "small Solar System body" throughout, as appropriate. — ELIZABETH GRIFFIN.

THESIS ABSTRACTS

EXPLORING THE IONIZATION STATE OF THE INTERGALACTIC MEDIUM WITH LYMAN-α ABSORPTION

By James S. Bolton

The amount by which the spectral continuum of a quasar is attenuated bluewards of its Lyman-α (Lyα) emission line provides a unique probe of the ionization state of the hydrogen in the intergalactic medium (IGM). Combined with state-of-the-art cosmological hydrodynamical simulations, this Lyα opacity is used to infer the amplitude of the metagalactic ultraviolet (UV) background at 1 Rydberg (Ryd). The Lyα absorption due to singly-ionized helium within the IGM is further used to constrain the spectral shape of the UV background between 1 and 4 Ryd. A detailed analysis of the uncertainties on the amplitude and spectral shape of the UV background is undertaken. For the first time, the results are found to be formally consistent with observational estimates from other, independent techniques.

Within the redshift range $2 \leqslant z \leqslant 4$, a substantial contribution to the UV background at 1 Ryd from galaxies is required. Quasars dominate the ionizing photon emission at 4 Ryd. A significant contribution to the UV background at 4 Ryd by thermal emission from hot gas in galaxies and galaxy groups appears unlikely. The small spatial variance expected for a UV background dominated by ionizing emission from hot gas cannot be reconciled with the large fluctuations observed in the ratio of He II to H I column densities in the Lyα forest. It is shown that these large column-density-ratio fluctuations are expected if, at each point in space, only a small number of quasars contribute to the UV background at 4 Ryd. This is expected at the tail-end of the He II re-ionization epoch, when the mean free path for He II ionizing photons is similar to the average separation between quasars.

At higher redshifts, $z \approx 6$, the almost complete attenuation of quasar spectra blueward of the Lyα emission line indicates that the neutral-hydrogen fraction in the IGM is increasing with look-back time. Radiative-transfer simulations are used to perform an in-depth examination of the small regions of high transmissivity immediately blueward of the Lyα emission lines produced by quasar emission. The sizes of these highly ionized near-zones cannot be used to constrain reliably the ionization state of the hydrogen in the surrounding IGM; the near-zone sizes provide no clear evidence for the tail-end of H I re-ionization occurring just above $z = 6$. The relative sizes of the Lyα and Lyβ near-zones, as well as the widths of the absorption features within the Lyα near-zones, should provide more sensitive measurements of the IGM neutral-hydrogen fraction at these redshifts. — *University of Cambridge; accepted 2006 September.*

The full thesis is available electronically at:
http://www.mpa-garching.mpg.de/~jsb/publications.html

EVOLUTION OF GALAXIES — STAR-FORMATION HISTORIES IN NEARBY SPHEROIDS

By Mark Steven Northeast

This thesis is an investigation into the formation of spheroidal-type galaxies. The investigation began with modelling studies of early-type galaxies and spiral bulges (SBs). From galaxy-formation modelling studies led by experiments with a sample galaxy, some results were obtained: non-solar abundance ratios in elliptical galaxies achieved better fits between model and data than solar abundance ratios. For both early-type and late-type galaxies, best fits with non-solar abundance ratios were more constrained than in the solar-abundance-ratio case. A strong link between star-formation histories and the supernova-Ia rate for the early- and late-type galaxies was shown. The model code itself was tested by way of pseudo-galaxy experiments, and shown to reproduce model parameters reliably. In the topic area of galaxy formation, regions of spectra particularly sensitive to a galaxy's age and metallicity were measured as equivalent widths and then calibrated to the common scale of the Lick Indices. The Lick Indices were used in deriving all key results throughout the thesis.

The modelled sample of galaxies from Proctor & Sansom lacked data on low-velocity-dispersion (low-σ) galaxies for line-strengths-*versus*-kinematics correlations. In regards to low-σ galaxies, low-luminosity ellipticals (LLEs) were considered to be likely candidates. Long-slit spectra of a sample of 12 LLEs, taken at the European Southern Observatory *New Technology Telescope*, were selected for their low velocity dispersions. The spectra of ten of these LLEs were successfully reduced and line strengths and kinematics were measured. The Lick Indices of these LLEs were correlated with velocity dispersion (σ), alongside the previously modelled companion data set. Ages and metallicities of the LLEs were estimated. From these results, the LLEs were found to have significant correlations of line strength *versus* σ with SBs. However, the LLEs do not appear to be younger than

SBs, but younger than ellipticals. The LLEs seem to consist of a low-metallicity group (possibly misclassified dwarf spheroidal galaxies) and a high-metallicity group. Future possible work is suggested that may uncover which models of galaxy formation for high- and low-metallicity LLEs these results support. — *University of Central Lancashire; accepted 2007 January.*

Quantifying Dark Energy

By Fergus Simpson

Around fourteen billion years ago the Universe emerged from a hot, dense, and compact state, spending the first few billion years expanding in accordance with our understanding of physics. This was a phase of continuous deceleration, due to the inherent gravitational attraction of its constituents. However, when the Universe was around half its current age, an as-yet unidentified phenomenon wrested control of the cosmological dynamics, leaving us in a phase of acceleration, and with an uncertain future.

The behaviour of this phenomenon, dubbed 'dark energy', is characterized by the equation of state of a hypothetical substance. This defines the rate at which its energy density dilutes when subject to an expansion. Several techniques are anticipated to deliver a tenfold improvement in measuring this quantity within the next decade, and therefore they form the basis of this research. In particular, I illustrate the redshifts at which weak lensing is sensitive to the equation of state. The 'weight function' is used to highlight the rôle played by geometry and the growth of structure. By deducing the close relationship between weight functions and principal components, this analysis is extended to other cosmological probes, namely supernovae and baryon acoustic oscillations.

I then demonstrate the capability of cosmic-shear surveys to identify the baryon oscillations within the dark-matter power spectrum. This may be achieved with a careful selection of redshift bins corresponding to different angular scales, and offers a complementary method to galaxy surveys.

Finally, I present a holographic model of dark energy, which arises when an energy density is associated with the area of the particle horizon in a closed universe. Varying the constant of proportionality leads to some intriguing scenarios, with current observational constraints pointing towards a series of 'Big Rips'. — *University of Cambridge; accepted 2006 December.*

The full thesis is available electronically at:
http://www.roe.ac.uk/~frgs/Thesis.pdf

Here and There

GRAVITY MORE POWERFUL THAN WE THOUGHT

A rock needs to have a mass of about 5,1020 kg for gravity to give it the nice round planet-y sort of shape that the IAU says a planet ought to have. — *The Guardian*, 2006 August 24, back page.

FOR THE STELLAR NURSERY, OF COURSE

Pink and orange hydrogen clouds set amid a field of soft blue oxygen gas. — *A & G*, **47**, 5 · 10, 2006.

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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