THE OBSERVATORY

Vol. 136 | 2016 DECEMBER | No. 1255

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2016 April 8 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

M. A. BARSTOW, *President*
in the Chair

The President. Good afternoon. I have a couple of very pleasant things to do before we start the speaker programme for this afternoon, and the first is to award the Gold Medal for Astronomy to Professor John Barrow. It's always nice to award these prizes but it's also particularly nice when it's somebody who you work with and you can read out the impressive things they've achieved during their career.

The Gold Medal in astronomy is awarded to Professor John David Barrow. Professor Barrow is a world-renowned theoretical cosmologist. He has authored more than 500 papers over the last 35 years, widely ranging over topics such as anisotropic cosmologies, primordial nucleosynthesis, the origin of cosmic magnetic fields, limits on the time variation of physical constants — which is some of the work I'm doing with him so I claim some credit there — and extensions of General Relativity.

He has been indefatigable in his passion to demonstrate the importance of our science within the general culture of mankind. He is known to a wide public through his popular, beautifully written, and authoritative books — over twenty of them — on astronomy, mathematics, and physics. *The Anthropic Cosmological Principle*, co-authored with Frank Tipler, is an acknowledged classic, and shows his concern for philosophical issues.

He is the only person since 1642 to be elected to two different Gresham Professorships, in his case those in Astronomy and Geometry. Since 1999, Barrow has successfully led the Millennium Mathematics project, dedicated to the task, important for all sciences, of strengthening the teaching of mathematics in primary and secondary schools. He has been an extraordinary ambassador for science and mathematics, while maintaining a lifetime's work investigating novel and stimulating ideas in cosmology. For those reasons Professor John Barrow is awarded the Gold Medal of the Royal Astronomical Society for Astronomy. [Applause.] Personally, I don't know how he has time to do all that.

[Laughter.] I have a hard time just keeping one job going, never mind six or seven at the same time.

The second pleasantry this afternoon is also regarding somebody I know quite well. This is to award the certificate for the Honorary Fellowship in astronomy to Professor Alvio Renzini. The citation for Alvio for the Honorary Fellowship 2016 is as follows: The main trait of Professor Alvio Renzini is his eclecticism in astrophysics and cosmology. Professor Renzini's research interests span from the theoretical calculation of the chemical production and ejecta of asymptotic-giant-branch stars ('Advanced evolutionary stages of intermediate mass stars 1. Evolution of surface compositions', *A&A*, **94**, 175, 1981) to the formation histories of galaxies at redshift 2 ('A different approach to galaxy evolution', *MNRAS*, **398**, L58, 2009), passing through the study of how to maximize the success of photometric observations by taking into account stellar lifetimes. Currently Associate Scientist to the National Institute of Astrophysics at the astronomical observatory at Padua, Italy, Renzini has authored 360 publications in refereed journals and some other 400 in diverse forms totalling 38 000 citations. He is regarded as one of the 100 most influential scientists in the decade from 2002 to 2012 in space science. Alvio Renzini is nominated an Honorary Fellow of the Royal Astronomical Society for those reasons. [Applause.]

Congratulations to both John and Alvio. Now on to the speakers' programme this afternoon, and it's my pleasure to introduce Martin Elvis from the Harvard Smithsonian Center for Astrophysics, who is going to talk about 'The quasar rain: the origin of the broad-line region'.

Professor M. Elvis. The origin of the broad-emission-line region (BELR) in quasars and active galactic nuclei (AGN) is still unclear. I propose that condensations form in the warm accretion-disc wind of quasars creating the BEL clouds and uniting them with the other two manifestations of cool ($\sim 10^4$ K) gas in quasars, the low-ionization phase of the warm absorbers (WAs) and the clouds causing X-ray eclipses. The cool clouds will condense quickly (days to years), before the WA outflows reach escape velocity (which takes months to centuries). Cool clouds form in equilibrium with the warm phase of the wind because the rapidly varying X-ray quasar continuum changes the force multiplier, causing pressure waves to move gas into stable locations in pressure–temperature space. The narrow range of two-phase equilibrium densities may explain the $(\text{luminosity})^{½}$ scaling of the BELR size, while the scaling of cloud-formation time-scales could produce the Baldwin effect. These dense clouds have force multipliers of order unity and so cannot be accelerated to escape velocity. They fall back on a dynamical timescale (months to centuries), producing an inflow that rains down toward the central black hole. As they soon move at about Mach 10 – 100 with respect to the WA outflow, the 'raindrops' will be rapidly destroyed within months. This rain of clouds may produce the elliptical BELR orbits implied by velocity-resolved reverberation mapping in some AGN, and can explain the opening angle and destruction time-scale of the narrow 'cometary' tails of the clouds seen in X-ray-eclipse observations. Some consequences and challenges of this 'quasar rain' model are presented.

The President. Perfect timing, and we've got time for a few questions.

A Fellow. Professor Andy Lawrence, Chelsea MacLeod, and others have observed these 'changing-look' quasars, where the broad lines completely disappear on very interesting time-scales — years. So, I'm just trying to put this in context: that's one observation, but there are also the broad absorption lines that we've known about for a while. These are in slow quasars that vary bolometrically — how does this all tie in with the quasar 'rain'?

Professor Elvis. We know from the polarization of the troughs in the broad absorption lines of quasars, from Patrick Ogle's thesis work in 1999, that they're highly polarized: they're highly non-symmetric structures. So it's not some 4π wind that's coming out like that. It's that most quasars have these flows but most of the time we're not looking at them down the flow. So in my picture, if you look down one of those cones coming out, you will see the broad absorption line. The changing-look quasars could be clouds of a different kind or maybe they're clouds that just make it to escape velocity and are still there and come around on a slower time-scale. You have to have them further out for the time-scale to be longer — so it could be something like that, but I haven't worked that out.

The President. This is clearly such a good idea that nobody wants to contest it. So we'll thank Martin again. [Applause.] Occasionally when you're President you get to do some really nice things on behalf of the Society, and I really enjoyed my trip to Baikonur before Christmas as one of the members of the UK delegation for Tim Peake's launch. So it's a real pleasure to have Andrew Kuh from the UK Space Agency to talk about that: 'Tim Peake, human spaceflight, and space-environments research in the UK.'

Mr. A. Kuh. [No summary was received at the time of going to press.]

The President. Thank you, Andrew. Before we let the questions go, I think I'll just take the opportunity to say that all this came out of a Royal Astronomical Society report in 2005 about human spaceflight and human space exploration. So we can all claim the credit for all of this. [Laughter.] Anyway, has anybody got any questions?

Mr. R. Campanha. You mentioned that one of the chief aims is to get more opportunities to come into the UK. I was wondering where are those opportunities going to come from? And are trade or commercial restrictions going to limit some of the opportunities that could potentially come into the UK?

Mr. Kuh. Do you mean industrial opportunities or opportunities for research? In what sense?

Mr. Campanha. I mean more on the industrial side.

Mr. Kuh. There are always challenges, but that's true of all space programmes and we have seen American companies setting up in the UK and they definitely have an eye on our human-spaceflight programmes. So yes, there will be challenges, but also there's industrial innovation in the UK, and us being a member of the ESA club allows us to grow that more.

Professor C. Lintott. There's no doubt that Tim's mission has been a huge public-relations and outreach success. Everyone in the UK knows who Tim Peake is and that's great. When you jump to talking about the science and the research that's being done, we move away from Tim's personal stories. Can you give us an example we can use of something specific that Tim's done on the station — fiddled with an experiment, turned something on, planted some seeds, or whatever it is? So then we can say he's done this and it's led to this science — that's the question that I'm getting that I don't know how to answer.

Mr. Kuh. It's quite a hard one to refine into just a slick one-sentence answer because with most of the experiments lots of the astronauts will do lots of little bits.

Professor Lintott. One example of a little bit would be great.

Mr. Kuh. Well, there's the metallurgy research I was talking about with the electromagnetic-levitator device. In the past, the data that came out of that have led to a 40–50 per cent reduction in the weight of gas-turbine blades — not on Tim Peake's contribution, but that's what previous space flights have done.

The President. Actually, real science, as we know, takes longer to come out. So if he's doing experiments during these six months, it could be months or years before the results are actually out.

Professor Lintott. Sorry, I didn't mean to come across as if I was saying I didn't think there was science being done. I'm saying that I'm trying to get an example — it might appear silly and trivial, it might involve flicking a switch — that connects these broader stories about scientific results to the human story of Tim being up there, and we're missing that at the minute. We've got the human stories and then we've got the long-term scientific outcome, but it would be great if UKSA could say "today Tim did this".

The President. So what has he been doing as part of the research programme?

Mr. Kuh. He's been driving the rover, which is developing technology used on the ground — autonomous technology for use in the mineral-extraction industries. I think that's quite a good example, quite a succinct one. It's a nice one when you're talking to the public about it because they want to know why you would drive a rover from space. But then there is a story behind that which I think is fairly easy to tell. I understand the problem.

Mr. H. Regnart. Can you comment on whether there would be adverse consequences for co-operation with countries continuing in the European Union if the UK were to leave the European Union? [Laughter.]

Mr. Kuh. ESA, through which we do the vast majority of our programmes, is an independent treaty organization. It has Switzerland, Norway, and even Canada involved in a lot of ESA programmes. Obviously, a lot of our work is also with the EU so there would be some impact. It's hard to speculate what that would be. Suppose there's a hypothetical scenario where we leave the EU and it ruins our relations with France and Germany. I don't think that would affect space programmes; I think our space programmes would continue. Just as we haven't completely cut ties with Russia on space programmes even when the broader political scene gets difficult, space has actually been shown to be a good tool for improving relations.

The President. The RAS is obviously quite concerned about it as a body and I know Robert Massey has been doing quite a lot of work in this area. Our view is that the basic agreements with ESA and ESO will probably continue but the threat to us is the loss of European Union grants, ERC grants, and mobility.

Dr. S. Sim. Has Tim had any opportunity to do experiments with the Astro Pi kit that he took up?

Mr. Kuh. He has, but I can't remember the latest status; sorry. I should have known that question was coming. There are over 30 experiments, so I can't remember what the latest is, but I know he has been using that.

Dr. Sim. Since it involved UK school children it might be a good answer to the question that was asked before.

Dr. Karen Masters. He has been tweeting about Astro Pi. I do not remember the details but you can follow him on Twitter

The President. Did you have a question as well?

Dr. Masters. Oh yes, I did. I wanted to know about longevity. He's obviously captured the public imagination and children's imagination extraordinarily well, but what is the UK space agency going to do to ensure that inspiration keeps going?

Mr. Kuh. Good question. That's something we've been conscious of from way before he even flew. That's why the education programmes have been designed specifically so that they're not time bound, necessarily, to his mission. He already has a ridiculous schedule for the 12 months after he returns; I feel quite bad for him but he seems to enjoy it. Who knows? He could get a second flight at some point — that would be nice but that's to be determined.

Professor F. Taylor. Is Tim the only astronaut we've got or are there lots of people in training that are going to carry on this tradition when he gets too old? [Laughter.]

Mr. Kuh. He's the only British ESA astronaut and there are no plans for them to recruit new astronauts at the moment.

The President. So we had better make the most of him. Thank you very much, Andrew. [Applause.] Our third speaker is Matt Mountain, whom we probably all remember from running the *Gemini* telescope when Britain used to be involved in *Gemini*, and was then Director of the Space Telescope Science Institute. Now he is occupying the exalted position of president of AURA: 'US optical–infrared observational astronomy: challenges and opportunities'.

Professor Mountain. [No summary was received at the time of going to press.]

The President. We have time for a few questions.

Professor E. I. Robson. Great talk, Matt. Does this really mean the end of the decadal surveys if it's all political pressure by everyone agreeing to get behind the programme?

Professor Mountain. The real answer to that question is no: it's exactly the same as it's always been — decadal surveys. The first time the decadal surveys came up with their own mission was the last one, and that was probably what caused some problems. Nobody expected that to come out. If you look at all the big programmes, there are large groups who put them in and work through the surveys and pull them out. There's always been this to and fro — the concept of a linear flow of ideas really doesn't exist. When you get up, you realize suddenly that there are always loops. It's exactly the same: the decadal survey has to endorse these programmes, and to endorse these programmes they have to have the right information; but it's not enough to have the decadal survey, that's the problem. You have to have the administration, you have to have industry, and you have to have Congress, because Congress will otherwise cherry-pick the decadal survey. Everybody says "well, look what happened with Europa, they supported it". The question you have to ask is, "if Europa had not been in the decadal survey, what would have happened?" My guess is that Europa would have happened.

Dr. R. Massey. It's really interesting to compare the system in the US with what happens here, which is that we're very grateful when science doesn't get cut as much as everything else, and we carry on being super grateful for the next four years in the hope the same thing happens again. What I did think was interesting was that actually you have congressmen who are particularly interested in scientific projects, I think to a greater extent than happens here. Here, you wouldn't see people on the whole taking that level of interest — maybe if they're in their constituencies somewhere, otherwise usually not. I guess I'm just wondering about the disadvantages and advantages of the two approaches, between appropriations line by line, *versus* that sort of classic Haldane principle where politicians leave it alone and nominally trust scientists to get on with it.

Professor Mountain. You're seeing that very much play out in the difference between NASA and the National Science Foundation. Those divisions I showed you: they've normally been left completely alone. However, this time around there's a sense in Congress that they want to direct those lines. Now of course, this is politically biassed because social sciences and earth sciences aren't necessarily popular in parts of the government, parts of Congress. So there is some pressure to try and do that. On the flip side, you have to keep telling stories like this; the interesting thing about the United States is you have to keep the conversations going, because, if you don't, people will work on their own — whether it be a congressman or a society — and what we've found is that unless you try to connect the dots continuously you will get black-swan events. For example, there may be an appropriation to insert a lander into Europa irrespective of whether it's even possible. Or you may get the earth-sciences budget completely cut to zero. But interestingly enough, if you keep the conversations going, which is why you spend much more time as senior astronomers in the United States working in Congress, you can actually make progress. Everything is run by the staff, actually, and they are normally very well attuned, they've read all the reports, they've done everything, and what you have to have is a consistent story. A lot of work goes in, not always successfully, in getting the east coast and the west coast of the United States talking on the same story. A lot of marriage brokering goes on. If you can make it work you can do things like get *James Webb* through or Europa. If you don't have it working, everything falls apart because there's always another priority, there's always someone else who wants that money. That, I think, is the difference with the UK: there's always another project or another mission.

The President. I think, unfortunately, we need to move on, so thank you, Matt. [Applause.] I think we're seeing more of that kind of American-style thing in the UK now with the way the government spends its capital money, not necessarily with the Haldane principle in mind.

Our final speaker this afternoon is Paul Spudis from the Lunar and Planetary Institute and he's going to talk about 'The New Moon: some results from recent exploration'.

Professor P. Spudis. Within the last few years, several robotic missions to the Moon have mapped its surface in unprecedented detail, yielding new insights into the evolution and history of that object. The Moon has had a richer and more complex history than we had thought. Many of the new insights come from NASA's *Lunar Reconnaissance Orbiter* (*LRO*), launched in 2009 June and continuing to operate to the present. Over the past seven years, *LRO* has mapped the topography and morphology of the surface, measured the physical properties of the poles, and made several new discoveries. In addition, other robotic missions have added to this haul of knowledge.

Thanks to the laser altimeter and cameras on *LRO*, we now have a global map of the Moon's shape and surface features, in some regions at resolutions as fine as 25 cm. Over seven billion laser returns show that the Moon has surface relief (lowest to highest points) exceeding 16 km, a dynamic range comparable to that of the Earth. Images and slope measurements show that the Moon is regionally smooth at decametre-to-kilometre scales (with slopes less than 15 degrees, even in the rough highlands) but can be rough on metre-to-decimetre scales (as shown by abundant blocks and debris flows in craters). We now have complete, global coverage of lunar topography and morphology, allowing us to plan future missions in addition to uncovering new and unexpected processes.

In addition to systematic mapping, the data also permit us to construct a variety of special products, including some designed to elucidate the unusual properties of the lunar poles. Because the Moon's spin-axis obliquity is only 1°·5, lighting conditions near the poles are unusual, with topographically low areas being zones of both permanent darkness and high peaks that may be in sunlight for extended periods. At both poles, we find large expanses of permanent darkness, which receive heat only from the lunar interior. Measurements of surface temperature in the thermal infrared show that some of these dark areas have surface temperatures less than 25 K, colder than the surface temperature of Pluto. Vast areas near the poles are colder than 104 K, the temperature at which water ice is permanently stable on the surface. In combination with other remote measurements, we believe that significant quantities of water ice exist within these dark, cold regions.

Certain peaks and crater rims near the poles protrude above the local Sun horizon, creating areas of 'quasi-permanent' sunlight (*i.e.*, areas that are in sunlight for more than 90% of the year). Such areas are key sites in that they would permit us to reside on the lunar surface permanently, using solar arrays to generate electrical power. In combination with the nearby deposits of water ice in the dark areas, those sunlit peaks make up oases where humans could live and work productively on another world.

The story of lunar water has become more complex and somewhat more mysterious in the past few years. In contrast to early ideas about a bone-dry Moon, we have found that water and hydroxyl are found on the Moon in several forms and in a variety of concentrations. Spectral data from the *Moon Mineralogy Mapper* (M^3) on the *Chandrayaan-1* spacecraft show the widespread presence of an absorption band caused by water on the surface at latitudes poleward of 65°. The hard-landing probe from *Chandrayaan-1* flew through an exospheric water cloud just above the south pole of the Moon. The *LCROSS* (*Lunar Crater Observation and Sensing Satellite*) experiment directed the upper-stage Centaur of the *LRO* launch vehicle into the floor of the lunar crater Cabeus, near the south pole. A following satellite monitored the collision and found both water vapour and ice particles in the ejecta plume produced by that impact. Estimates suggest about 7·5% by weight of water occurs in the sub-surface in this area. The *Mini-RF* experiment on both *Chandrayaan* and *LRO* mapped both poles and found evidence for high diffuse-radar backscatter within several permanently dark areas, probably caused by the several metres' thickness of water ice within some of the dark interiors. Neutron data from *Lunar Prospector* and *LRO* likewise indicate elevated hydrogen at both poles. Both active imaging (*via* laser) and ultraviolet mapping of the polar dark regions indicate that some bright deposits in those regions are likely to be water ice or frost.

Two additional robotic missions have rounded out our new perspective on the Moon. The *GRAIL* mission used two co-orbiting spacecraft to map the gravity field of the Moon to an unprecedented level of detail (surface resolution of about 25 km). From that mapping, we find that the lunar sub-surface is exceedingly complex, with systems of igneous dykes feeding the eruptions of mare lava and also displaying significant variations in crustal thickness, caused by the impacts that formed the giant basins that throw out thousands of cubic kilometres of crustal materials. The *LADEE* (*Lunar Atmosphere Dust and Environment Explorer*) mission was designed to measure the amount and composition of the lunar exosphere and the dust cloud surrounding the Moon and how it varies over time. Several species make up the lunar exosphere, including helium and other noble gases, sodium, and potassium. The exosphere has variable density and changes as the Moon passes through the Earth's geomagnetic tail every month. Dust varies by location and time of year; annual meteoroid showers kick up dust into ballistic trajectories that vary by location. No electrostatic levitation of dust (reported by some previous missions, including astronaut observations) was observed.

The abundant data provided by these new missions have revised and extended our understanding of the Moon. The discovery of lunar water, its complexity and history, has revolutionized our view of the role of the Moon in future exploration. We can now envisage using the Moon to create new spaceflight capabilities, *e.g.*, mining polar water to manufacture rocket propellant. Using lunar resources is the first step toward creating a permanent space-faring infrastructure, one that can access not only the lunar surface but all of cis-lunar space, where more than 95% of our satellites reside. In addition to its utilitarian value, we have found evidence for an unsuspected complexity in lunar history, including recent vulcanism, sub-surface caverns, the migration of volatiles, and locations on the surface protected from solar wind by surprisingly strong local magnetic fields. The new Moon offers humanity both a location and the resources needed to establish a permanent, off-planet human habitation, a place where we can learn the skills and develop the technology needed to settle space.

The President. I think we have time for one question. Don was first in there.

Professor D. Kurtz. Your 25K cold traps would indicate, at least to me, a surprisingly cold interior and yet that seems inconsistent with the recent vulcanism. Could you comment on that?

Professor Spudis. You're right, the global average heat flow is very low. It's at least a factor of five and maybe as much as a factor of ten lower than the Earth's. However, one thing we do know from the distribution of radioactive elements is that the crust is very heterogeneous. So some areas on the Moon might remain warm at depth whereas the areas near the pole might be more reflective of the global average.

The President. One final question, from Donald.

Professor D. Lynden-Bell. Are there any minor mascons other than the major mascons that are underneath the maria? And are there any mascons on the back side of the Moon?

Professor Spudis. Yes, dozens. There is a mascon under every major, significant basin. They vary in magnitude, some of them are very weak and some of them are very strong. There's nothing quite like the Imbrium Basin mascon, which is enormous, but we found that there are at least forty basins on the Moon of which about 35 have recognizable mass anomalies.

The President. I think you'll all agree what a fantastic programme we've had this afternoon. Just before we finish, I'd just like to make one remark. You probably don't realize that I can bask in the reflected glory of these programmes but actually it's Ian Crawford our Senior Secretary who puts them together. So when we thank all our speakers, I think we should also thank Ian for a series of wonderful programmes over the last few years. [Applause.] He will continue, I hasten to add. I'll remind you, as if you needed it, that the drinks reception follows immediately in the RAS library and I give you notice that the next monthly A&G open meeting of the Society will be on Friday 13th of May, 2016, following the AGM.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2016 May 13 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

J. A. ZARNECKI, *President*
in the Chair

The President. I have several announcements which I will go through before we proceed to the substantive business. First of all is the announcement of the thesis prizes for 2015 which have been approved today. I'm pleased to say that the Michael Penston Prize has been awarded to Dr. Matt Nicholl of Queen's University Belfast for the thesis entitled 'Observations and modelling of super luminous supernovae'; the runner up is Dr. Alexandre Ferreira of Durham University, and we hope that the prize winner will give a talk at a future RAS ordinary meeting. As will also the winner of the Keith Runcorn Prize which has been awarded to Dr. Matteo Ravasi of the University of Edinburgh for the thesis entitled 'Reciprocity-based imaging using multiply-scattered waves'; the runner up is Dr. Callum Shakespeare from the University of Cambridge.

Could I remind you that nominations are sought annually for a number of awards in astronomy and geophysics for, of course, the RAS Awards and Medals. The call for nominations has been issued and details for all of the awards are on the RAS web-site. This year we have two new awards, namely the Annie Maunder Medal for outreach, and the history of astronomy or geophysics medal; we will announce the name of that medal later this year. The deadline for nominations is 2016 July 31, except for the Patrick Moore Medal and the Annie Maunder Medal which have deadlines on September 30 this year.

We now move to the programme and we will start with an update from Professor Steve Miller on the RAS 200 Sky and Earth Programme. I'm sure everybody knows that this is a major public-engagement programme to lead up to the 200th anniversary of the foundation of the Society in 2020.

Professor S. Miller. At last year's AGM we announced the five winners of the first tranche of awards under the RAS 200 'Sky and Earth' scheme. All of those organizations accepted the grants and are now working on their respective projects. For the upcoming second round we have about £400K left. This is somewhat less than that for the first round, but this time we have appointed a team of professional evaluators, which will give us some security in the decision-making.

If you think you may have a proposal, look for groups or communities which do not normally engage with outreach activities. We want to leave a legacy not just for 2020 but beyond. There will be stakeholder meetings at Burlington House on 2016 July 13, so please let them know. Town Hall meetings will take place between 2016 September and December to discuss possibilities and proposals (in 2014 that stage generated 92 proposals). Outline proposals open on 2016 October 10 and close on 2016 December 9, with the first grant-panel meeting taking place on 2017 February 8; the final panel meeting will be 2017 April 19. The successful applicants will be announced at the 2017 Annual General Meeting of the Society.

Dr. S. Jheeta. Thank you very much for that. Two questions — first, is this a worldwide project we're talking about?

Professor Miller. We have, as you well know, a large overseas fellowship and we welcome proposals from them. We had several last year but none of them, in fact, got funded, although that was not on a principle. That was just on ranking the various projects. Are we going to leave a legacy worldwide? We're going to leave a legacy as far as the Society reaches worldwide.

Dr. Jheeta. Right; the second question being what do you mean by legacy? Are you saying that it's longevity? Is that the same thing?

Professor Miller. We are looking for projects that have some idea about being self-sustaining beyond the four or five-year period for which they're getting funding. In other words, they will become part of the permanent framework. For example, the projects that the Prince's Trust are putting together — they will be able to sustain beyond their funding which takes them up to 2020. So it's that kind of legacy which also makes a change in the community where these things are being rolled out.

The President. Any other questions?

Dr. R. E. S. Clegg. Steve [Miller], thinking about the science areas in the project so far and next round, I'm wondering about your view of the inclusion of geophysics and specifically the Earth, not planetary and Solar System science — how is that going? From the beginning, I thought that might be harder but I hope we can achieve it, of course; I thought that I might comment that in the selection criteria you developed for tranche two, I believe it's perfectly legitimate that the selection panel takes into account the spread of subjects covered so far across the RAS portfolio.

Professor Miller. I agree with your comments. I would say that frankly we have no geophysics project currently funded and that's a shame. We have — you're talking about terrestrial geophysics in particular — nothing from that field whatsoever. I don't remember that we had many proposals come in, if any at all. I'm definitely going to be beating the drum to say we want proposals from the geophysics area, and particularly from the Earth geophysics area, and as a panel if we see proposals come in that are there or thereabouts in terms of the ones we're likely to fund, then we'll take that into account because I think the point you make is very valid.

The President. Thank you very much indeed, Steve, for that update. [Applause.] We now move to the main item of this meeting and that is the 2016 Presidential Address, which will be by Professor Martin Barstow of Leicester University. The title is 'Diamonds in the sky, the importance of white dwarfs in modern astrophysics'.

Professor M. Barstow. [It is expected that a summary of this talk will appear in a future issue of *A & G*.]

The President. Thank you very much Martin, for near-perfect timing if nothing else. [Laughter.] I think we have time for a couple of quick questions.

Professor Carole Jordan. I must comment on your information about the atomic data for iron and nickel. The person you should contact is Juliet Pickering at Imperial College because she made very highly accurate measurements with the interferometer that they use there, specifically for testing the gravitational effect. Her data have been used — they're probably put into MIST as well — but she's had very little credit for doing that work. Her measurements are almost certainly the best available.

Professor Barstow. Thank you for pointing that out.

Dr. R. Barber. Thank you very much for a really interesting talk. What you are measuring is clearly something that isn't understood. Looking at the data that you expressed in some of the graphs, the ordinate was a log axis so the measurements appear to be just off but they are off by a factor of ten or too small by a factor of ten or one hundred. It's always confusing when you've got a log axis. Then you saw the error bars on some of the measurements and it seemed to me, not knowing anything about this, that the errors are not in the instrumentation. By improving the error bars, you're still going to be measuring things that are wildly adrift and the suggestion to me is that there's something fundamental in the physics of this object that is far more complicated. I know you talk about rocky debris and things, but I just don't think you're modelling the actual object accurately enough. I think that's what's the data seem to imply. There's no problem with your measurements.

Professor Barstow. I think, if you're talking about the point of view of the predictions of what the abundances should be, that the radiative levitation calculations are pretty sophisticated. I wouldn't trust them overly, but when you see orders of magnitude difference I think it's difficult to dig down into the physics understanding and find the explanation — which is why I think the rocky-planet-accretion hypothesis works much better. So if you're supplying material from the outside, the accretion is controlling the abundance and you've got to supply the material from somewhere. I think radiative levitation will still happen but it's then acting on material that is being supplied to it.

Professor A. M. Cruise. Just to give a bit of hope on the issue of measuring the mass. White dwarfs would be major sources of gravitational-wave signals for the space-borne *LISA* mission and that should see every close white-dwarf binary in the whole Galaxy and it will resolve the orbital frequencies of many of them. If the recent ground-based measurements are anything to go by, you'll get really quite precise masses from those, so if you pursue the same analysis from the spectra in white-dwarf binaries then there's the hope in ten years' time.

Professor Barstow. I was going to say it's a little bit far ahead but yes you're right. I'm well aware of the gravitational-wave interest.

The President. Last question.

Professor D. Kurtz. Martin, for your rocky hypothesis, your star was a red giant and cleaned out its inner system. When the atmosphere left and you had the core left behind as a white dwarf, anything that could possibly be accreted is way far out. Do you have any theoretical support that anything could migrate in on reasonable time-scales?

Professor Barstow. It's not my field but there's a lot of work done on the interaction of planets. So yes, you do clear out to about the current orbital radius of the Earth in the case of the Sun. There's debate as to whether the Earth would survive the red-giant phase but the red giant then disturbs the dynamics of the rest of the planetary system, and there are evolutionary paths for material to scatter into the inner part of the system after that. There's a lot of good modelling going on that demonstrates that that is possible.

The President. Martin, thank you very much indeed. [Applause.] Could I remind everybody that we have the usual drinks reception over in the Society library and could I encourage everybody to come across and we can toast the health of our retiring members of Council. Finally, I give notice that the next monthly A&G Meeting of the Society will be on Friday 14th of October 2016.

HISTORICAL BACKGROUND TO THE ECLIPSE FUNCTION

By E. Budding
Carter Observatory, Wellington, New Zealand

& M. D. Rhodes
Brigham Young University, Provo, Utah

We examine the eclipse function as discussed in the *Almagest* and applied to solar and lunar eclipses.

Introduction

In Book 6, Section 7 of the *Almagest*, Ptolemy[1] (Claudius Ptolemaeus, around 140 AD) discusses the brightness of the eclipsed solar disc at different phases of the eclipse on the assumption that this brightness is proportional to the circular disc's remaining uneclipsed area. This is the function denoted by $\alpha^0{}_0$ in the notation of Kopal[2], and it is usually calculated in dependence on the ratio of eclipsed to eclipsing radii $r_2/r_1 = k$, and the separation d of the two disc centres in units of the radius of the eclipsed star, thus

$$\alpha^0{}_0 = \{\arccos(s) + k^2 \arccos(\mu) - d\sqrt{(1-s^2)}\}/\pi\ , \qquad (1)$$

where we have introduced the auxiliary quantities $s = (d^2 + 1 - k^2)/2d$ and $\mu = (d^2 + k^2 - 1)/2dk$.

Ptolemy had his table of chords (Book 1, Ch. 11) to deal with the trigonometric functions, and he used a relatively accurate value of $3^{17}/_{120}$ for π; even so, arithmetical methods then available must have been fairly arduous and prone to error. The results of Ptolemy's analysis appear as a table in Section 8 of Book 6, with an implied accuracy of 1 part in 72 (sixth of a unit on a scale of 1 to 12). Ptolemy calculated the eclipse function for central eclipses (*i.e.*, the line of sight to the centre of the eclipsed disc lying in the plane of movement of the eclipsing disc), with two values of the ratio of radii ($1\frac{1}{36}$ and $2\frac{6}{10}$) corresponding to adopted mean apparent ratios of the lunar to solar angular diameters and the Earth's umbral cone at the distance of the Moon), and for 12 eclipse phases. Fig. 1 shows Ptolemy's results for the solar-light-loss function compared with modern computations*. The standard deviation of the differences is about 0·014, supporting the previous inference on the accuracy.

We may note, from the last paragraph of Ch. 9 in Book 6 of the *Almagest*, that Hipparchus had also carried out such calculations (around 140 BC). Ptolemy indicates that although in a particular eclipse considered by Hipparchus a different value of k had been used, the corresponding difference in d offset this, so the resulting values of α would have been closely similar.

Quantitative aspects of this subject appear to have remained dormant after Ptolemy[4] until Pickering[5], who evaluated the same $\alpha^0{}_0$ function, but applied it to the context of the light variation of Algol. Pickering provided a table (his Table X) for $\alpha^0{}_0$, giving five values of k and ten entries for different phases of the eclipse. Two of the values of k are the same, but the entries correspond to different assumed values of the orbital inclination i, which will affect the appropriate values of d for different stages of the eclipse.

* The program *WinFitter*[3] was used to carry out the calculations reported in this paper.

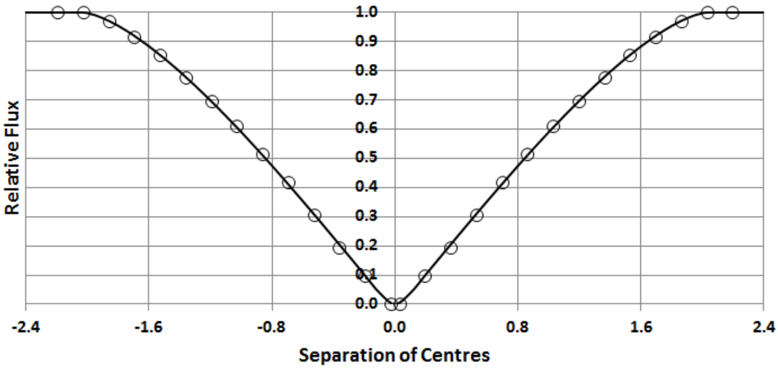

FIG. 1

Comparison of Ptolemy's eclipse hand-calculations (Almagest Book 6, Ch. 8) with the same function calculated on a modern electronic machine. The curve plots the light loss from an undarkened solar disc when totally eclipsed by a lunar disc greater in angular size by a factor 1·028. The *x*-axis is in units of the argument *d* in Eqn. 1.

H. N. Russell[6] made efforts to standardize notation (as indicated in the foregoing symbols), though Russell used the radius of the orbit as the unit of length. An assumption of circularity for the orbit usefully simplifies our present discussion, so enabling (in Russell's notation) δ/r_1 for d in Eqn. (1), where r_1 is the radius of the eclipsed star as a fraction of the mean separation and δ satisfies

$$\delta^2 = {r_1}^2 d^2 = \sin^2\theta \sin^2 i + \cos^2 i \tag{2}$$

where θ is the orbital phase. Russell carried out equivalent calculations to those of Ptolemy and Pickering and for a greater range of the ratio of radii of eclipsing to eclipsed discs. His standard formula for the relative light flux l during the eclipse of a star whose fraction of the total light is L_1,

$$l = 1 - L_1\alpha, \tag{3}$$

has been adopted in subsequent studies.

Russell inverted his tables so as to determine the separations δ for given values of α and k, retaining three digits in the numerical results. He noted the significance of the limb-darkening effect, *i.e.*, the inherent variation of local brightness from centre to limb of the disc that renders the underlying assumption of Ptolemy inaccurate. This issue was first tackled quantitatively by Harzer[7], and calculations extended to cover a wide range of eclipse possibilities by Tsesevich[8]. But by this time, interest in the eclipse function α was essentially confined to the context of eclipsing variable stars, the singular case of the solar eclipse arises in entirely different observational circumstances.

On the observational side, there has been general interest to account for the light variation during the solar eclipse as it may have been seen by ancient astronomers[9], but efforts to measure this variation in a simple way produce

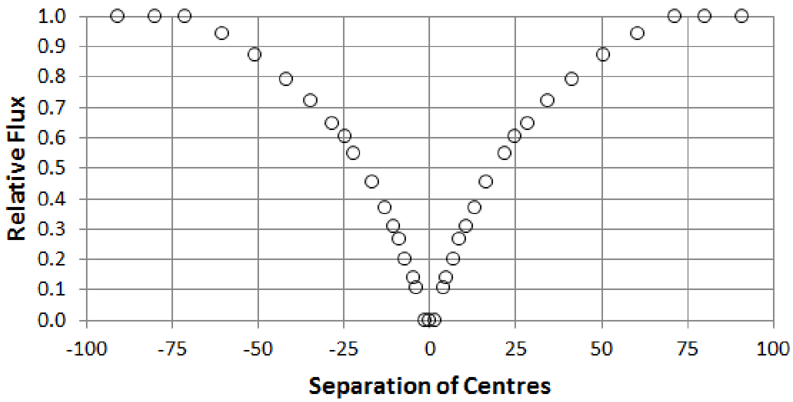

FIG. 2

Solar-eclipse observations from Liendo & Chacin[10]. The abscissae show minutes away from the time of mid-eclipse. The flux-measurement ordinates have been scaled to unity.

results that look quite different from Ptolemy's calculation[9,10]. The light variation as recorded by such authors during solar eclipse (Fig. 2) has much more of a 'trumpet-bell-shaped' descent to minimum than the more 'V-shaped' variation of the α function.

These data include, apart from the (optical range) light variation of the solar disc, an appreciable area of surrounding sky, whose scattered-light contribution will become more significant as the eclipse proceeds and more complex to evaluate accurately. A more appropriate variation corresponding to the eclipsed disc of the Sun itself is revealed in the infrared data of Julius[11].

Solar brightness variation in eclipse

Julius[11] observed the almost-total eclipse of the Sun from near Maastricht on 1912 April 17. He was mainly interested in using the very central portions of the eclipse light-curve to test theories of the heat transfer in the semi-transparent layers above the photosphere. To this end, he used two measuring instruments: a thermopile with a large dynamic range and a more sensitive bolometer used for accurate measurements of the flux during the central parts of the eclipse. The eclipse was observed through a telescope system that minimized extraneous light. The combination of the two measuring instruments enabled Julius to determine that the residual light at mid-minimum was less than 1/5000 the flux from the full disc.

The circumstances of the eclipse can be readily checked by general planetarium software, *e.g.*, *Stellarium*[12]. In this way, the lunar and solar apparent diameters during the eclipses are found to be 1910·1 and 1911·1 arcsec, respectively; the eclipse was not quite total, and the altitude at mid-minimum, close to the local noon in time, was about 48°·7. Julius did not make any corrections for atmospheric extinction and there were apparently some minor guiding errors during the observations. He reported the atmospheric conditions

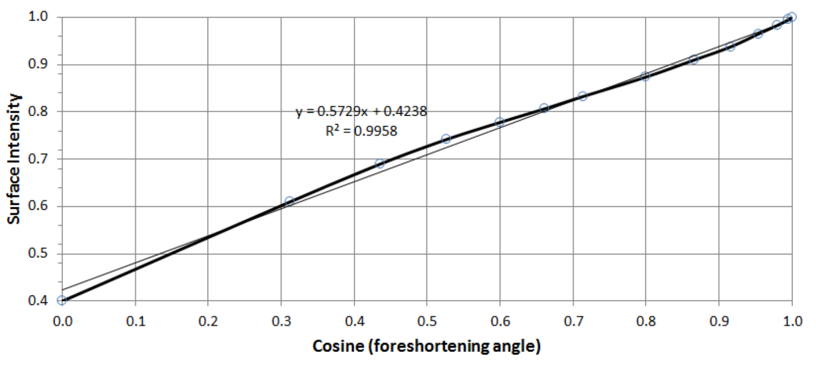

FIG. 3
Julius'[11] solar limb-darkening data with linear approximation superposed.

to be unusually good and haze-free, but the difference in the measured fluxes at the beginning and end of the eclipse suggest an overall scale of error in the thermopile measures of order 2%. In fact, although there appear to be systematic differences between the observations and a standard theoretical model with the known geometry (greater than can be accounted for by inaccuracy of a standard (linear) limb-darkening approximation, see Fig. 3), the clustering of residuals obtained when we relax parameter values to allow a good model-fitting indicate that the random component of measurement errors is about 1% (see Figs. 4 and 5).

Regarding the limb-darkening, which depends on the wavelength of observation, Julius did not give details of the spectral response of the thermopile used. General information on such equipment (*cf.*, *e.g.*, ref. 13) suggests a likely value of around 1 μm for a typical effective wavelength. This would correspond to a coefficient of about 0·35 for the solar limb-darkening in the linear approximation, and this is close to what was found empirically (0·37) in the fitting of Fig 4. On the other hand, Julius[11], combining bolometric and thermopile data in a separate numerical procedure, found a value of about 0·57 as a representative (bolometric) coefficient. Further discussion of the limb-darkening is rather outside the scope of this article, however.

For eccentric orbits, the connection between the eclipse geometry and time becomes somewhat more complicated. The lunar orbit is more complicated still as it is not simply elliptical, though it can be suitably represented by linear series of trigonometric functions. This need not concern us here, however, since a Pythagorean relationship similar to Eqn. (2), *i.e.*, the formula

$$d^2 = a^2\theta^2 + b^2 \tag{4}$$

will generally hold, at least for short time intervals, uniformity of the motion entailing that θ varies linearly with time, so that a scales with the velocity and b is the minimum separation or 'impact parameter'. The *Stellarium* data indicate that, for this eclipse, $b = 0{\cdot}0026$. Clearly Eqn. (2) tends to the form of Eqn. (4)

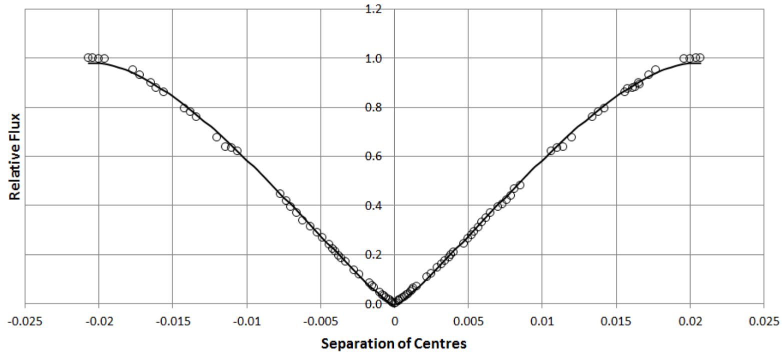

FIG. 4

Standard model fitting to the solar eclipse observed by Julius[11]. Observational circumstances deteriorated towards the end of the eclipse so the initial data have been reflected about the time of mid-eclipse, reflecting the essential symmetry of the data. The abscissae are scaled here to a circular disc of 0·01 radian radius for the eclipsed object and assume a uniform motion for the eclipsing object with a radius 0·9995 times as large.

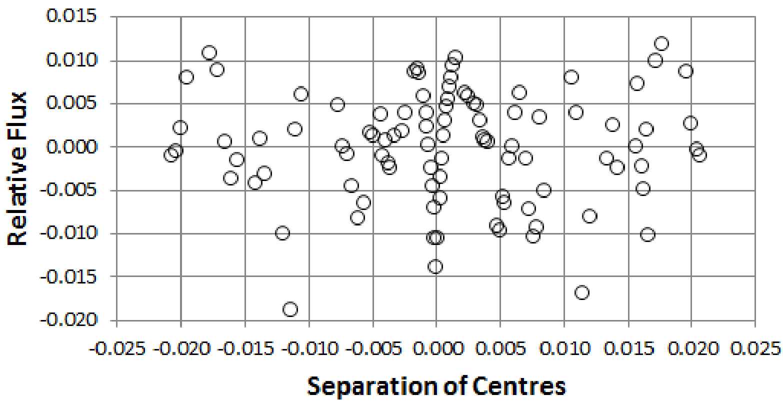

FIG. 5

Residuals from the fitting shown in Fig 4. A small systematic effect is noticed at the minimum, when the radiation becomes slightly less than in the linear approximation.

for small values of θ, and for a sufficiently small interval Δθ in the eclipse region a can be taken as constant. The range of the eclipse is given by the criterion that $|\theta| < r_1(1 + k)$, or $|\theta| < \arcsin \{r_1 \sqrt{[(1+k)^2 - b^2]}/\sin i\}$, so that, to an acceptable approximation, Eqn. (4) allows direct application of Eqn. (3) in the same way as for stellar eclipses with $r_1 < 0{\cdot}01$.

The eclipse range here is less than 3^h or ~0·0042 of the complete lunar phase cycle, while the Sun subtends an angle of about 0·0087 radians at the Earth, for which the difference between θ and sin θ ~ 3×10^{-7}, so, if the rate of progress of the eclipse with time may be taken to be uniform, Eqn. (4) can be used to evaluate the d values in Eqn. (1). Note that Julius gives the time of first contact, when $d = 1 + k$, while the mid-eclipse time, when $d = b$, is also known.

Reference data for the lunar orbit[14] indicate that the space velocity of the Moon would vary by less than 0·05% of its mean value in an hour; however, the *Stellarium* modelling shows a variation in *apparent* rate of progress of the eclipse for an observer at Maastricht through the time in question could be ~1·5% of the mean. Since d varies from approximately 2 to 0 during the eclipse, it can be seen from the light-curve that the variation of α with d is mostly around ½, indicating an error arising from applying the phasing relation Eqn. (4) with constant a to be up to 1%, so within the likely accuracy limit of the flux measurements discussed above.

Figs. 4 and 5 then show that observations of the eclipse of the solar disc, when carried out with sufficient care, can be modelled by standard formulae at least to the accuracy of photometry typical of eclipsing binary stars.

In any case, it is of interest to note that Ptolemy's calculations were essentially accurate enough to compare with the carefully gathered data of Julius[11] for the full solar eclipse, apart from its neglect of the (significant) limb-darkening effect.

Lunar brightness variation in eclipse

The variation of the light from the Moon during a lunar eclipse has a number of complicating issues. The subject was well reviewed by Westfall & Sheehan[15]. When a solar eclipse is observed at some point on the Earth, the tangent cone from the observer to the Moon defines a distinct circular region at the distance of the Sun that delimits an area of directly received flux from another completely obstructed region. Calculation of the loss of received light can proceed generally in the way discussed above. During a lunar eclipse, however, some parts of its surface may be partially illuminated while others receive no direct light, as the Moon penetrates the penumbral and umbral cones of the Earth's shadow. Calculating the integrated light from the lunar surface under such circumstances requires more mathematical development than has been presented before. Apart from the geometry of the shadow cones, differences in local reflection properties over the lunar surface together with the role of refracted and scattered light from the Earth's atmosphere, that can have local and temporal variations, were discussed by Westfall & Sheehan, as well as the difficulties of accurate photometric reductions. All in all, the application of Ptolemy's eclipse-function table to the lunar case would look to be highly optimistic. Interestingly, however, Ptolemy's value for the umbral cone's diameter at the mean distance of the Moon, at 2·6 times the lunar diameter, is surprisingly accurate (with Ptolemy's value for the mean lunar apparent radius of 15′ 40″, modern values for the lunar and solar parallaxes would give close to 2·63 for this average ratio). The essential difference that a larger relative radius to the eclipsing object makes in effecting a more rapid loss of light through most of the range of d, only to be caught up with when the eclipse becomes total, is seen in Fig 6.

Broadband photometry of the total lunar eclipse of 1982 July 6 was given by Westfall & Sheehan[15]. Again *Stellarium* can be used to check the circumstantial details. The lunar radius would subtend an angle averaging around 14·83 arcmin during that eclipse, the Earth's mean radius being about 3·67 times that value at the distance of the Moon and the radius of the Sun about 15·73 arcmin on that occasion. The umbral-cone radius at the Moon would then have amounted to 38·82 arcmin, or 2·62 times that of the Moon. The logarithmic units of the stellar magnitude scale are perhaps of more relevance to lunar-eclipse photometry (*cf*. Fig 4.6 in ref. 14). It is thus quite apparent that the Moon's disc can show striking variations in apparent brightness and colour from eclipse to eclipse and

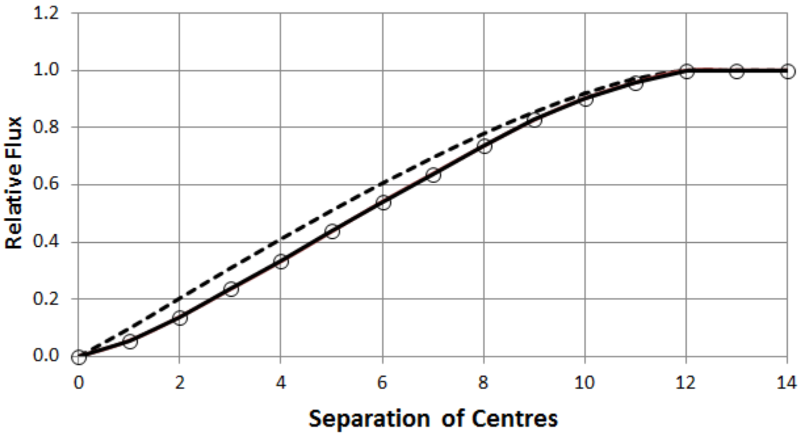

FIG. 6

The two eclipse functions for circular discs with differing ratio of radii (solar case dashed, lunar full). The *x*-axis is in Ptolemy's units of twelfths of the remaining diameter of the eclipsed surface, as advanced on by the eclipsing object[1]. His results for the lunar case are shown (circles).

even through umbral phases, when the disc never quite disappears, although in linear units the visual flux drops to a millionth part of the level outside eclipse.

Although the umbral disc is visibly identifiable as an eclipsing body, using the corresponding ratio of radii in direct application of the eclipse function to the linear light loss would give a poor general representation. The light loss in the penumbral cone quickly becomes significant, though the umbral eclipse doesn't start until the separation of the Moon's centre from the centre of the umbral circle reaches about 54 arcmin. Though the eclipsed flux has become effectively complete by 39 arcmin, the Moon's centre is only just reaching the umbral cone, *i.e.*, half its disc is still not totally eclipsed, indicating the large loss of light towards the inner edge of the penumbral cone. The apparent widening out of the totality is well-known and has been discussed in various papers supporting the need for more detailed analysis for a fuller understanding of the lunar eclipse.

When the magnitude data of Westfall & Sheehan[15] are converted to linear flux units a fair approximation to the light loss, at least in the middle ranges of the decline, is found from setting the eclipsing body to have relative size halfway between the penumbral and umbral cones, *i.e.*, the full Earth, for which $k = 3{\cdot}67$ (Fig. 7). At half-immersion the gradual penumbral loss across the disc could be effectively regarded as half in full shadow and half in full clear. The average limb-darkening effect for the Moon, regarded as an empirical coefficient, is small — around 0·13 in optical light[16] — permitting the uniform-disc approximation of Ptolemy to give a tolerable match to the data (Fig. 7).

The scatter in the final few uneclipsed measures suggests an inherent datum accuracy of about 0·03 in the relative flux values, and if that value is used in a light-curve fitting, adopting the geometric parameters from the known circumstances of the eclipse, a reduced χ^2 value of 1·4 is found. This is outside the 95% confidence interval for the model, so clearly the data would permit

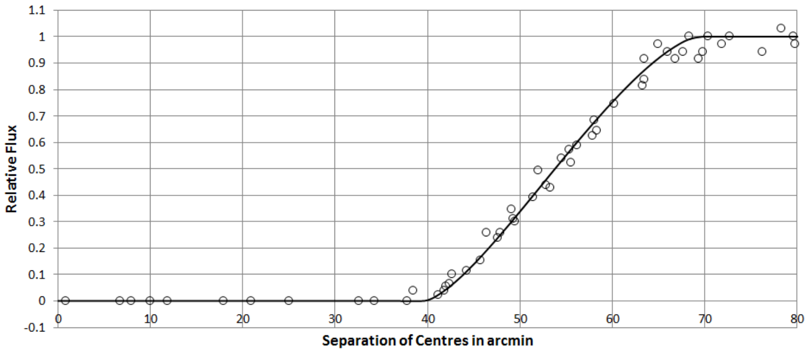

FIG. 7

Lunar-eclipse observations[14] of the total eclipse of 1982 July 6 matched by Ptolemy's classical eclipse function. The abscissae give the separation of the lunar disc centre from that of the Earth's shadow in arcminutes.

an improved treatment of the light-loss function. The present discussion of approximate representation by standard eclipse models is then not to gainsay any argument for more detailed lunar-eclipse photometry and analysis[17,18], but rather in recognition of the interesting presentation in the *Almagest* and its earlier sources, notably that of Hipparchus, which has to be seen as highly painstaking for its time.

References

(1) R. C. Taliaferro, *The Almagest by Ptolemy* (*English translation*) (Encyclopaedia Britannica, Inc., Chicago), 1948.
(2) Z. Kopal, *Close Binary Systems* (Chapman & Hall, London & New York), 1959.
(3) M. D. Rhodes, *WinFitter Manual* (*3·50*), http://michaelrhodesbyu.weebly.com/astronomy.html
(4) S. McCluskey, *Astronomies and Cultures in Early Medieval Europe* (CUP), 1998.
(5) E. C. Pickering, *Publ. Amer. Acad. Sci.*, **16**, 1, 1880.
(6) H. N. Russell, *ApJ*, **35**, 315, 1912.
(7) P. Harzer, *Kiel Publ.*, no. 16, 1927.
(8) V. P. Tsesevich, *Publ. Univ. Obs. Leningrad*, **6**, 48, 1936.
(9) F. Verbelen, http://users.skynet.be/felixverbelen/selight.html, 1999.
(10) J. A. Liendo & G. H. Chacín, *Rev. Bras., Ensino Fis.*, **26**, no. 4 (ISSN 1806-9126), 2004.
(11) W. H. Julius, *ApJ*, **37**, 225, 1913.
(12) F. Chéreau *et al.*, http://www.stellarium.org, 2015.
(13) H. K. Hammond & H. L. Mason, *Radiometry and Photometry*, NBS Special Publ., 300 Ser., 7, 1971.
(14) D. R. Williams, http://nssdc.gsfc.nasa.gov/planetary/factsheet/moonfact.html, 2016.
(15) J. Westfall & W. Sheehan, *Celestial Shadows: Eclipses, Transits and Occultations* (Springer, New York), 2015.
(16) G. Fielder, *Structure of the Moon's Surface* (Pergamon, Oxford), 1961.
(17) J. Westfall, *JALPO*, **29** (7–8), August, 168, 1982.
(18) J. Westfall, *JALPO*, **30** (1–2), June, 6, 1983.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 251: HD 146989, HD 148068, HD 148294, AND HD 148800

By R. F. Griffin
Cambridge Observatories

This paper presents orbits for four stars, all of which are in Area 2 of the 'Clube Selected Areas'[1], which is centred at $\alpha = 17^h\ 30^m$, $\delta = 60°$, in the constellation Draco. The selection criteria for the Clube programme include requirements for the stars to be of *HD* spectral type K0 and to have visual magnitudes within half a magnitude of $9^m{\cdot}0$ in the *HD Catalogue*, although subsequent more-sophisticated classifications and magnitude measurements often stray outside those criteria.

In this case HD 148068 is a star that is not on the actual Clube programme but is a field star near to such an object; in the *HD* it is listed as slightly too faint ($9^m{\cdot}6$) and of the wrong type (G5) to qualify for the programme in its own right. It is double-lined, and has a very eccentric orbit ($e \sim 0{\cdot}6$) and the colour index of a late-F star; its orbital period of 247 days is known to an hour. The minimum masses (1·5 and 1·3 $M_{\odot}$) of the components are just about consonant with its being a pair of main-sequence stars if the orbital inclination is high, but the substantial rotational velocities of the components may well be indicating that they are giants.

The other three objects have *Hipparcos* parallaxes that show that their luminosities are in the range of giant stars (that of HD 148294 is probably higher still). HD 146989 has an orbit with a period of some 8 years and an eccentricity of 0·45; the orbits of HD 148294 and 148800 have periods of 1040 and 330 days, respectively, with eccentricities near 0·2.

Introduction

The 'Clube Selected Areas' programme[1] of radial-velocity observations was initiated almost 50 years ago, shortly after the successful development[2] of a novel method of measuring the radial velocities of late-type stars with high accuracy and greatly improved sensitivity. The basis of the method was the 'real-time' cross-correlation of a considerable range of the spectrum with a suitable mask, replacing the comparatively laborious measurement of a photographically recorded spectrum line by line. The sensitivity gain arose through (*a*) combining the light from hundreds of absorption lines and measuring it with just a single detector, instead of recording a dispersed spectrum consisting of hundreds of separately measured resolution elements, each of which would incur some detector noise, and (*b*) the much greater quantum efficiency of the photomutiplier detector in comparison with the photographic plate*. It is fair to point out, at this juncture, that a somewhat comparable principle had been used by Evershed[4] 50 years earlier still, and also that the *idea* of cross-correlation had been explicitly canvassed by Fellgett[5] and Babcock[6] in 1953 and 1955, respectively. In developing the method for routine use, the writer's contribution was, therefore, largely the *implementation* of ideas that had been seeded by others.

Soon after the 'radial-velocity spectrometer' had been brought into use[2], Dr. S. V. M. Clube invited the writer to measure the velocities of about 250 ninth-magnitude stars distributed in 16 discrete 'Areas', all at ±35° Galactic latitude and at 45° intervals in Galactic longitude. The deal was that I would measure the velocities and he would discuss the results. It took me a long time — *too* long, as it may now appear — to do my part of the work, although in the end I 'over-fulfilled my norm' (as they used to say in the Soviet Union): the velocities of 406 stars, all repeatedly observed, in the ten Areas accessible to the Cambridge telescope were published[1] in 1986, and those of a further 625 stars in the remaining six southern Areas were published[7] in 2006 after several visits had been made to ESO for that specific purpose. Even now, no general discussion of the results has been made, but in the course of the observations it was natural to find that the radial velocities were variable for a number of the stars, and some of them (about 70 in total) have been discussed individually[8] by the present writer. This paper now presents the orbits of four more such stars; they are all in Area 2 of the Clube Areas, but one of them is not a star on the actual Clube programme but is adjacent to such a star and was initially observed just out of curiosity.

All but four of the observations utilized in this paper have been obtained with the Cambridge *Coravel*. They are listed 'as observed', but the zero-point of the velocities given by the instrument is known to exhibit a dependence on the colour index of the star observed. The difference of the zero-point from the one now established by the *Elodie* instrument at Haute-Provence has been plotted as a function of colour index for a considerable number of stars in Fig. 1 of ref. 9, where also a brief discussion of the issue can be found. It could be concluded that, to obtain improved estimates of the true kinematic radial velocities (which are *not* the quantities of prime interest here) of the four stars treated in this paper, negative corrections of 1·1, 1·5, 0·7, and 1·4 km s^{-1} should be applied to all the entries in Tables I, II, III, and V, respectively, and thus to the corresponding γ-velocities in Table VI.

HD 146989

HD 146989 is to be found in the constellation Draco, about 3° preceding, and the best part of a degree north of, the 5^{m} K star 18 Draconis. The *Henry Draper Catalogue* lists it as 9^{m}·3, K0 — both of which data have held up very well against modern values, since *Hipparcos* photometry, transformed to the usual system, has given V = 9^{m}·24, $(B-V)$ = 1^{m}·03. The parallax measured by the satellite, according to the revised reduction by van Leeuwen[10], is 3·36 ± 0·71 milliseconds of arc. That translates to a distance modulus of about 7^{m}·4 ± 0^{m}·5, implying an absolute magnitude of about +2. Tabular values[11] corresponding to the M_V and colour index then suggest a spectral type close to K0 III–IV for HD 146989.

*In a presentation[3] to a meeting of the RAS, the writer claimed a gain of a factor of 4000 over the (previously universally adopted) photographic method. The factor, though still very large, would be reduced now by that part of it that was represented by the advantage (*b*) above, since CCDs became available to replace photographic plates.

TABLE I

Radial-velocity observations of HD 146989

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

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1998 July 28·99*	51022·99	+0·9	0·193	−0·3
2002 Sept. 4·89	52521·89	−5·1	0·689	−0·3
8·90	525·90	−4·9	·690	0·0
Oct. 23·77	570·77	−5·2	·705	−0·1
2003 Mar. 3·21	52701·21	−6·2	0·748	−0·4
May 8·01	767·01	−6·3	·770	−0·1
Aug. 8·91	859·91	−6·7	·800	0·0
Oct. 3·84	915·84	−7·2	·819	−0·2
2004 Mar. 2·22	53066·22	−7·4	0·868	+0·5
Apr. 15·11	110·11	−8·1	·883	0·0
May 19·05	144·05	−8·4	·894	−0·2
June 15·02	171·02	−8·4	·903	−0·1
July 16·96	202·96	−8·4	·914	−0·1
Aug. 10·98	227·98	−7·8	·922	+0·5
Sept. 3·90	251·90	−8·6	·930	−0·3
Oct. 25·82	303·82	−8·1	·947	−0·2
Nov. 26·71	335·71	−7·1	·957	+0·5
Dec. 26·28	365·28	−7·1	·967	0·0
2005 Jan. 22·27	53392·27	−6·9	0·976	−0·3
Mar. 12·21	441·21	−5·7	·992	−0·4
Apr. 22·11	482·11	−3·9	1·006	+0·2
May 8·13	498·13	−3·6	·011	+0·1
June 1·01	522·01	−2·8	·019	+0·1
22·99	543·99	−2·2	·026	+0·1
July 16·97	567·97	−1·4	·034	+0·3
Aug. 6·90	588·90	−1·2	·041	0·0
25·93	607·93	−1·0	·047	−0·2
Sept. 23·86	636·86	−0·3	·057	−0·1
Oct. 25·81	668·81	+0·1	·068	−0·2
Nov. 19·74	693·74	+0·3	·076	−0·3
Dec. 18·28	722·28	+0·5	·085	−0·3
2006 Mar. 1·22	53795·22	+1·4	1·109	+0·1
Apr. 5·14	830·14	+1·2	·121	−0·2
May 6·12	861·12	+1·1	·131	−0·3
June 4·04	890·04	+1·7	·141	+0·3
July 4·01	920·01	+1·4	·151	0·0
Aug. 2·00	949·00	+0·9	·160	−0·5
Sept. 10·86	988·86	+1·5	·173	+0·2
Oct. 24·80	54032·80	+1·4	·188	+0·2
Nov. 18·76	057·76	+1·3	·196	+0·1
2007 Feb. 15·23	54146·23	+1·0	1·225	+0·1
Apr. 4·17	194·17	+0·9	·241	+0·2
June 16·03	267·03	+0·7	·265	+0·2
Aug. 4·93	316·93	+0·6	·282	+0·3
Oct. 4·84	377·84	+0·4	·302	+0·3
2008 May 20·03	54606·03	−0·4	1·377	+0·4
July 21·97	668·97	−0·8	·398	+0·2
Sept. 13·89	722·89	−1·6	·416	−0·4
Nov. 7·79	777·79	−1·4	·434	+0·1

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2009 Mar. 21·20	54911·20	−2·5	1·478	−0·5
May 7·09	958·09	−2·4	·494	−0·2
July 2·97	55014·97	−2·3	·512	+0·1
Sept. 9·92	083·92	−2·7	·535	0·0
Nov. 23·71	158·71	−2·9	·560	+0·1
2010 Apr. 8·18	55294·18	−3·3	1·605	+0·3
June 4·09	351·09	−3·6	·623	+0·3
Aug. 30·94	438·94	−4·5	·652	−0·2
Oct. 30·74	499·74	−4·5	·672	+0·1
2011 May 19·05	55700·05	−5·4	1·739	+0·2
Sept. 12·84	816·84	−6·3	·777	0·0
2012 Apr. 30·09	56047·09	−7·3	1·853	+0·4
2013 Apr. 2·17	56384·17	−7·5	1·965	−0·2
June 14·00	457·00	−5·8	·989	−0·2
July 14·96	487·96	−4·7	·999	+0·1
Sept. 14·85	549·85	−2·9	2·019	0·0
Nov. 12·73	608·73	−1·0	·039	+0·3
2014 Apr. 27·15	56774·15	+1·2	2·094	+0·2
2016 July 1·98	57570·98	−1·1	2·357	−0·6

*Observed with Haute-Provence *Coravel*

The only other information of interest to be found in the literature concerning the star seems to be the single radial-velocity measurement of −0·19 ± 0·65 km s^{-1}, which features in a table referred to by Famaey *et al.*[12]. No date is given, but the writer can supply it, as he recognizes the observation as his own — the only one that he ever made of HD 146989 with the Haute-Provence *Coravel*; it is listed at the head of Table I here. The rest of that table lists the 67 measurements subsequently made of the star with the Cambridge *Coravel*. They cover reasonably well about 1½ cycles of a leisurely orbit whose period is found to be 3027 days (about 8·3 years) with an uncertainty of only 5 days. The full set of elements is given, with the corresponding data for the other stars treated in this paper, in Table VI below; a diagram of the orbit, which is seen to be of moderate eccentricity, appears here as Fig. 1.

The mass function is quite small, at 0·026 $M_\odot$, and requires the companion star to have a minimum mass of about 0·36 or 0·71 $M_\odot$ if the mass of the primary star is taken as 1 or 3 $M_\odot$, respectively. Those minimum masses correspond to main-sequence stars with types of about M2 and K5, whose luminosities would be of the order of 1/1000 and 1/100 of the primary's, respectively, so it is far from surprising that no evidence of the secondary has been seen in the radial-velocity traces. The 'dips' in the traces typically have equivalent widths near 5½ km s^{-1}, at least as great as are normally given by K0 giants, so the metal abundances in HD 146989 can be expected to be well up to normal. The dips are scarcely wider than would correspond to zero rotational velocity; nearly half the observations give values of $v \sin i$ no greater than 1 km s^{-1}, though the distribution has a tail of larger values, in six cases slightly exceeding 5 km s^{-1}. Non-linearity between

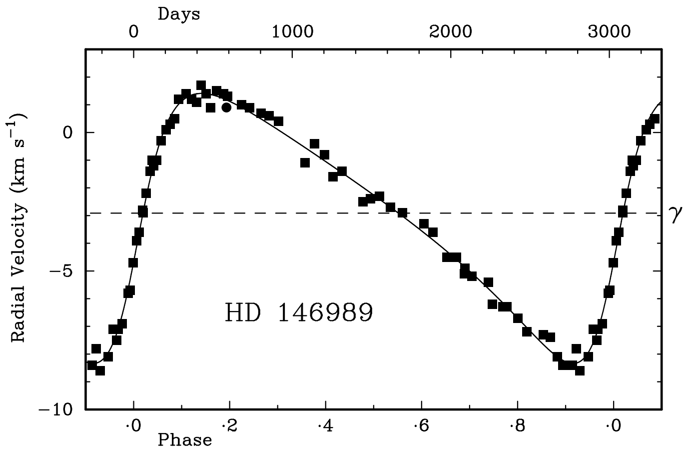

FIG. 1

The observed radial velocities of HD 146989 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All but one of the observations were made with the Cambridge *Coravel* and are plotted as filled squares, but the first measurement was made at Haute-Provence and appears as a filled circle. The same plotting conventions are used in subsequent orbit figures.

dip width and the inferred $v \sin i$ when the latter is small could make an average value misleading, but for orientation we could say that a $v \sin i$ of 2 km s^{-1} (like the Sun's) would represent, for a giant star with a radius of 10 $R_{\odot}$, a rotation period of the order of a year, provided that the orbital inclination is not very small.

HD 148068

This star, though not part of the 'Clube Selected Areas' programme in its own right, is perhaps the most interesting and significant of the stars treated here, among which it is the only one that is double-lined. It was brought within the field of Area 2 after the northern Clube Areas were arbitrarily enlarged so as to make the numbers of eligible stars more comparable with those in the southern Areas where the *Henry Draper Catalogue* is much richer. It is to be found preceding, and about 3° away from, on the one hand, 42 Herculis, whose declination is 2° lower, and on the other hand, the 'binocular' visual pair 16/17 Draconis which is 2° further north. Just brighter than the limit for mapping in *Uranometria 2000·0*[13], HD 148068 is shown there closely south-following a brighter star — an actual Clube-programme star — HD 148017; the angular distance between them is 2½ minutes of arc. It was when he was observing HD 148017 one night in 2003 that the writer's curiosity got the better of him and prompted him to make the initial observation of the adjacent star too.

Simbad gives magnitudes derived from *Tycho 2* for HD 148068: they are $V = 9^{m}\cdot58$, $(B-V) = 0^{m}\cdot47$. The colour index is too blue to belong to a giant star; it corresponds to that of a main-sequence star with a type of about F6, though the *HD* type is actually G5. The rest of the literature will not detain us, since *Simbad* knows of *no* paper that has anything to say about the object.

When the first radial-velocity observation of HD 148068 was made (at Cambridge like all the others), on 2003 August 8, by bad luck it happened to be taken at a time when the system was single-lined, so it did not create any immediate impression on the observer. It was three years before another observation was made; the new one was in wild disagreement with the first, and in retrospect it is seen that only the primary star was measured — the relatively weak dip of the secondary was outside the range of the scan, which had been centred on the more obvious primary. Not until the third observation, in 2008, was the double-lined nature of the trace recognized. A significant effort to watch the object ensued in 2009, when 21 observations of the star were made and the orbital period of about eight months became apparent, as did the rather high eccentricity of the orbit. At the more extreme node the velocities of the components differ by more than 70 km s^{-1}. A *Coravel* trace taken at such a time is reproduced in Fig. 2, and incidentally illustrates how shallow the dips are; even the one belonging to the primary star has a depth of only about 6% of the continuum.

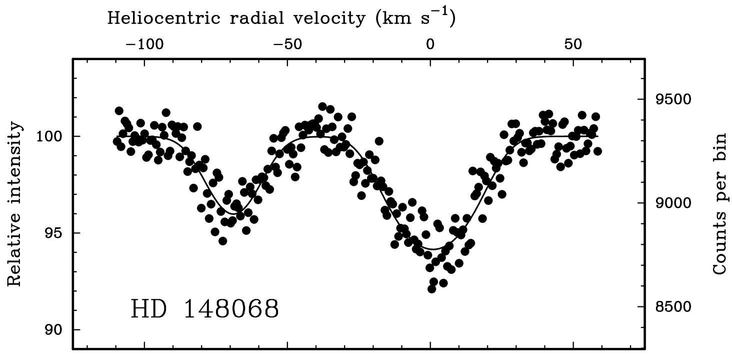

FIG. 2

Radial-velocity trace of HD 148068, obtained with the Cambridge *Coravel* on 2013 April 20, when the two 'dips' were at nearly their maximum separation.

The shallowness of the dips is exacerbated by their being smeared out by rotational broadening, with $v \sin i$ values of about 23 km s^{-1} for the primary and about 13 (but not accurately determinable) for the weak secondary, whose equivalent width averages about 0·4 times that of the primary. By imposing that ratio and the mean values for the rotational velocities of the two stars, even heavily blended radial-velocity traces can in many cases be reduced to give the stars' individual velocities; only eight of the 69 observations that are set out in Table II have had to be listed as single-lined. There are some quite unusually bad residuals, especially among the early measurements, but without any better reason to reject or down-weight the relevant observations it seems best not to interfere subjectively with the solution of the orbit but to 'take the rough with the smooth'.

TABLE II

Radial-velocity observations of HD 148068

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity*		*Phase*	*(O – C)*	
		Prim. $km\ s^{-1}$	*Sec.* $km\ s^{-1}$		*Prim.* $km\ s^{-1}$	*Sec.* $km\ s^{-1}$
2003 Aug. 8·94	52859·94	−30·6		$\bar{8}$·597	—	—
2006 July 25·06	53941·06	+5·8	—	$\bar{4}$·972	+2·2	—
2008 Oct. 21·84	54760·84	−47·2	−13·1	0·290	−0·3	+1·7
22·81	761·81	−47·2	−16·1	·294	−0·4	−1·2
27·79	766·79	−46·7	−13·0	·314	−0·7	+2·8
2009 Mar. 30·17	54920·17	+3·5	−70·3	0·935	+1·6	−0·4
Apr. 22·11	943·11	−27·4	−39·2	1·028	+1·4	−4·0
29·08	950·08	−43·0	−21·1	·056	−1·0	−0·8
30·09	951·09	−42·5	−22·8	·060	+0·7	−3·9
May 7·07	958·07	−47·8	−14·3	·089	+0·6	−1·3
20·04	971·04	−51·7	−13·1	·141	−0·9	−2·8
29·06	980·06	−51·9	−13·9	·178	−1·4	−3·2
June 2·05	984·05	−50·3	−11·0	·194	−0·2	+0·1
11·98	993·98	−50·2	−12·8	·234	−1·3	−0·3
24·03	55006·03	−48·1	−13·6	·283	−0·9	+0·8
July 5·03	017·03	−45·3	−13·7	·327	+0·2	+2·7
27·96	039·96	−41·2	−20·7	·420	+0·4	+0·1
Aug. 7·94	050·94	−35·3	−24·5	·464	+4·3	−1·5
18·88	061·88	−35·9	−25·9	·509	+1·6	−0·6
29·88	072·88	−34·8	−27·2	·553	+0·5	+0·6
Sept. 9·88	083·88	−32·1		·598	—	—
25·84	099·84	−32·5		·662	—	—
Oct. 8·79	112·79	−31·0		·715	—	—
22·80	126·80	−22·6	−46·2	·771	−1·9	−1·9
Nov. 3·79	138·79	−13·6	−49·5	·820	+2·1	+0·5
17·79	152·79	−7·3	−60·6	·877	+0·7	−1·9
2010 May 19·04	55335·04	−31·1		2·614	—	—
July 17·96	394·96	−11·8	−55·2	·857	−0·9	+0·1
29·93	406·93	−2·9	−64·2	·905	+0·3	−0·1
Aug. 10·93	418·93	+2·4	−73·9	·954	−1·8	−1·4
15·90	423·90	+1·9	−70·7	·974	−1·4	+0·8
18·93	426·93	+2·6	−66·3	·986	+3·1	+0·8
23·90	431·90	−14·6	−52·3	3·006	−1·6	+0·8
24·96	432·96	−17·8	−49·3	·011	−1·6	+0·1
30·87	438·87	−33·7	−29·5	·034	−1·0	+1·3
31·88	439·88	−35·8	−26·3	·039	−0·9	+2·1
Sept. 1·87	440·87	−36·7	−26·5	·043	+0·1	−0·3
2·88	441·88	−38·2	−24·0	·047	+0·4	+0·1
16·89	455·89	−50·5	−12·3	·103	−0·8	−0·7
21·88	460·88	−49·1	−9·6	·124	+1·5	+1·0
Oct. 11·80	480·80	−49·4	−10·2	·204	+0·5	+1·2
Nov. 25·73	525·73	−44·0	−21·0	·386	−1·0	−1·9
2011 May 15·11	55696·11	−47·4	−16·0	4·076	−0·8	−0·9
July 24·93	766·93	−43·9	−18·6	·362	+0·1	−0·6
Oct. 26·78	860·78	−22·6	−40·6	·742	+0·7	+0·8
Nov. 9·77	874·77	−16·8	−46·6	·799	+1·2	+0·8
2012 July 22·97	56130·97	−13·2	−51·0	5·836	+0·6	+1·2
Aug. 28·87	167·87	−1·6	−67·5	·985	−1·6	+0·2
29·86	168·86	−1·5	−65·7	·989	+0·3	0·0
30·89	169·89	−3·8	−63·2	·993	+0·2	−0·1

TABLE II (*concluded*)

Date (UT)	*MJD*	*Velocity* *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*	*Phase*	*(O–C)* *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
2012 Sept. 3·85	56173·85	−15·4	−51·3	6·009	−0·3	−0·7
6·87	176·87	−24·5	−38·4	·021	−0·2	+1·9
7·89	177·89	−26·5	−35·0	·026	+0·6	+2·1
13·87	183·87	−38·9	−20·9	·050	+0·9	+1·9
2013 Apr. 20·13	56402·13	+1·3	−68·4	6·933	−0·3	+1·1
28·09	410·09	+3·0	−72·3	·965	−1·4	+0·3
May 6·05	418·05	−5·8	−61·6	·998	+0·9	−1·5
7·07	419·07	−9·7	−58·3	7·002	−0·1	−1·4
9·07	421·07	−16·4	−51·7	·010	−0·8	−1·6
14·05	426·05	−30·0	−32·4	·030	0·0	+1·5
June 14·02	457·02	−51·3	−10·1	·155	−0·5	+0·3
July 10·01	483·01	−46·1	−15·0	·260	+1·9	−1·5
Sept. 2·89	537·89	−37·1	−24·1	·483	+1·7	−0·2
14·97	549·97	−36·7	−25·7	·531	−0·3	+0·9
2014 June 13·08	56821·08	−30·4		8·629	—	—
25·00	833·00	−31·9		·677	—	—
July 1·01	839·01	−32·1		·701	—	—
Sept. 9·90	909·90	−1·7	−66·6	·988	−0·3	−0·5
16·83	916·83	−20·2	−46·8	9·016	+0·3	−2·2
2015 May 13·09	57155·09	+1·5	−70·1	9·981	−0·2	−0·5
Sept. 6·86	271·86	−38·8	−21·7	10·453	+1·3	+0·7

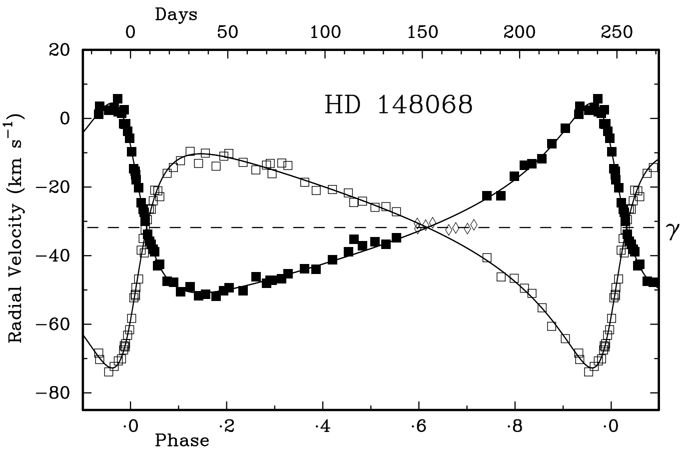

FIG. 3

The observed radial velocities of HD 148068 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel* and are plotted as squares, filled for the primary and open for the secondary; the secondary's velocities were given weight 0·7 in the solution of the orbit. Blends (not useable in the solution) are plotted as open diamonds.

The secondary dip, though so much weaker than that of the primary, is not so wide, and in the solution of the orbit the velocities derived from it have merited a weighting of 0·7 with respect to those of the primary. It will be readily understood that the velocities obtainable on a rather faint star whose traces exhibit two weak and wide dips that are usually more or less blended together are not likely to be of the highest accuracy; the r.m.s. residuals are 1·2 km s^{-1} for the primary and nearly 1·5 km s^{-1} for the secondary. All the same, the illustration of the orbit in Fig. 3 may convince the reader that a very reasonable approximation to the orbit has been found; for that we have partly to thank the large amplitude of the velocity changes, which exceed the measuring error by a factor that is greater in this case than in some instances where the actual velocity measurements are much more accurate. The orbital elements are included in Table VI.

HD 148294

HD 148294 is to be found very nearly 1° north of η Draconis, a $2^{m}·7$ late-type giant star that featured in the writer's Ph.D. work[14] on CN and G-band strengths 56 years ago, and in his subsequent papers[15,16] on photometric (as distinct from spectroscopic) measurements of the strengths of Fe I and Sc I lines in very narrow bands of wavelength. Although HD 148294 is listed in the *HD* catalogue with a magnitude of 9·1 (and a type of K0, like all the Clube stars), modern (*Tycho 2*) measurements put it nearly a magnitude brighter, at $8^{m}·26$. The discrepancy is perhaps partly related to a countervailing error in the *HD* in the colour of the star: the $(B - V)$ derived from *Tycho 2* is as much as $1^{m}·42$ (which would characterize a *late*-K star), so the brightness in the photometric B region would not be out of the range to be expected for stars on the Clube observing programme (defined by $m_{pv} = 9^{m}·0 \pm 0^{m}·5$ and spectral type K0, as listed in the *HD*).

The luminosity of HD 148294 seems to be unusually high: the (revised) *Hipparcos* paralllax[10] is only 0·64 ± 0·43 milliseconds of arc, and leads to a distance modulus close to 11 magnitudes, with 1-σ limits near 10 and 13 magnitudes*. Formally, therefore, the absolute magnitude could be expected to be roughly in the range –2 to –5, but the fractional uncertainty of the parallax is so large that the only safe conclusion must be that the star has a high luminosity, probably in the range of luminosity class II.

Alone among the stars discussed in this paper, HD 148294 has already had its radial velocity given in the literature. Being within the region of sky initially defined for Area 2, the star was observed twice in the 1970s with the original radial-velocity instrument[2] at Cambridge, and the results were published in the first paper[1] on the Clube Selected Areas. The two measurements differed by 2·4 km s^{-1} — not quite enough, at that time, to mark the star out with any confidence as a binary system. Another velocity is reported in a quasi-published supplement on the Web to a paper by Famaey *et al.*[12]; it is actually another measurement made (at Haute-Provence in 1998) by the present writer, and the velocity obtained then was between the two Cambridge ones and so did not by any means encourage any suspicion of duplicity. Not until a further measurement was made, with the Cambridge *Coravel* in 2002, did there arise a

*The original *Hipparcos* catalogue[17] gave the parallax as 1·06 ± 0·63 milliseconds; the distance that is listed with great precision by *Simbad*, of 602·3 ± 106·6 pc, is of unknown provenance, is far from corresponding with either the original or the revised value, but is made to appear much less uncertain than either.

TABLE III

Radial-velocity observations of HD 148294

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

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
1971	Aug.	1·03*	41164·03	−36·2	0·525	+0·4
1974	Sept.	21·91*	42311·91	−33·8	1·629	+0·1
1998	July	29·01†	51023·01	−34·9	10·006	−0·3
2002	Sept.	4·89	52521·89	−38·6	11·448	−0·2
		8·91	525·91	−38·0	·452	+0·3
	Oct.	23·77	570·77	−37·1	·495	+0·2
2003	Mar.	3·21	52701·21	−34·1	11·620	+0·1
	June	13·04	803·04	−31·9	·718	−0·1
	July	12·97	832·97	−31·4	·747	−0·3
	Aug.	8·96	859·96	−30·7	·773	0·0
	Sept.	14·82	896·82	−29·9	·808	+0·3
	Oct.	3·84	915·84	−29·9	·827	+0·1
	Nov.	5·75	948·75	−29·8	·858	+0·1
	Dec.	7·73	980·73	−29·8	·889	+0·4
2004	Jan.	15·28	53019·28	−30·9	11·926	+0·1
	Mar.	1·25	065·25	−32·7	·970	+0·1
	Apr.	15·12	110·12	−34·8	12·014	+0·2
	May	19·06	144·06	−36·8	·046	0·0
	June	8·02	164·02	−38·0	·065	−0·2
		25·99	181·99	−38·4	·083	+0·2
	July	9·97	195·97	−39·0	·096	+0·1
	Aug.	7·98	224·98	−39·7	·124	+0·4
		29·92	246·92	−40·3	·145	+0·4
	Sept.	13·85	261·85	−40·4	·159	+0·6
	Oct.	25·82	303·82	−41·6	·200	−0·2
	Nov.	30·72	339·72	−41·7	·234	−0·2
	Dec.	28·29	367·29	−41·3	·261	+0·1
2005	Jan.	22·27	53392·27	−41·2	12·285	0·0
	Mar.	12·21	441·21	−40·4	·332	+0·2
	Apr.	22·10	482·10	−40·4	·371	−0·4
	May	15·06	505·06	−39·9	·393	−0·3
	June	7·04	528·04	−39·5	·415	−0·4
	July	16·97	567·97	−38·4	·454	−0·1
	Aug.	6·90	588·90	−37·3	·474	+0·5
	Sept.	25·83	638·83	−36·6	·522	+0·1
	Oct.	25·81	668·81	−35·7	·551	+0·2
	Nov.	19·74	693·74	−35·2	·575	+0·1
	Dec.	11·72	715·72	−34·4	·596	+0·4
2006	Mar.	1·23	53795·23	−32·4	12·672	+0·5
	Oct.	24·80	54032·80	−30·9	·901	−0·5
	Nov.	23·75	062·75	−31·3	·930	−0·2
	Dec.	16·70	085·70	−31·8	·952	+0·1
2007	Mar.	2·22	54161·22	−35·9	13·024	−0·3
	Apr.	4·18	194·18	−37·1	·056	+0·2
	Aug.	4·93	316·93	−41·3	·174	−0·1
	Dec.	15·71	449·71	−40·8	·302	+0·2
2008	Nov.	25·73	54795·73	−34·4	13·635	−0·6

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2009 May 7·09	54958·09	−30·7	13·791	−0·3
Nov. 17·78	55152·78	−32·8	·978	+0·3
2010 June 4·09	55351·09	−41·5	14·169	−0·4
July 17·97	394·97	−41·9	·211	−0·4
2011 Oct. 15·79	55849·79	−33·8	14·648	−0·3
Nov. 27·72	892·72	−32·7	·690	−0·3
2012 Apr. 30·09	56047·09	−30·1	14·838	−0·2
Sept. 3·90	173·90	−32·4	·960	−0·1
Nov. 5·79	236·79	−35·3	15·020	+0·1
2013 Apr. 2·17	56384·17	−41·0	15·162	0·0
Sept. 16·87	551·87	−41·0	·323	−0·2
Oct. 16·76	581·76	−40·2	·352	+0·1
2015 Apr. 15·12	57127·12	−29·7	15·877	+0·4
Sept. 30·85	295·85	−36·6	16·039	−0·2
Oct. 2·79	297·79	−36·8	·041	−0·3

*Observed with original spectrometer; weight 0·1
†Observed with Haute-Provence *Coravel*; weight 1

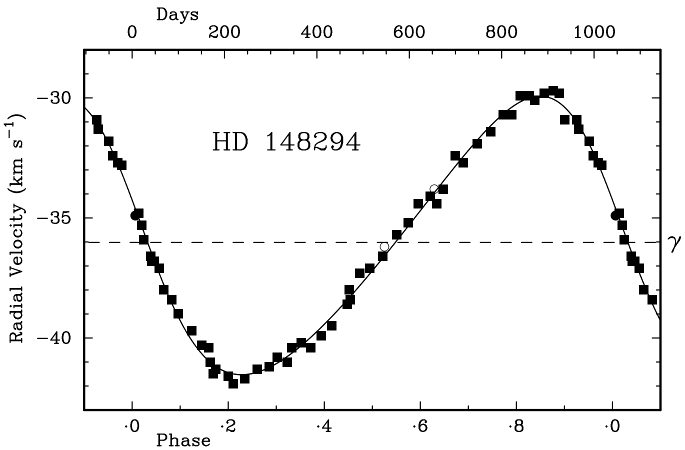

FIG. 4

The observed radial velocities of HD 148294 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The two earliest observations were made with the original spectrometer at Cambridge; they are plotted as open circles, and were weighted 0·1 in the solution of the orbit, although their residuals are actually no greater than those of the other observations.

distinct discordance, which was immediately confirmed by another observation. The star was then transferred to the binary programme and observed reasonably regularly; 59 measurements have been made with the *Coravel*, and they define an orbit with a period of 1040 days (determined to better than a day) and an eccentricity of 0·2. As befits a late-K giant or supergiant star, HD 148294 gives very nice deep dips in radial-velocity traces, their equivalent widths being about 6·5 km s^{-1}. All the data are given in Table III, while the orbit is illustrated in Fig. 4; its elements are included in Table VI, where it will be seen that the residuals have an r.m.s. value of 0·27 km s^{-1} — agreeably small for an object that (as a Clube star) is nominally of the ninth magnitude, though it has been helped by the star's being both brighter and of later type than was the nominal intention.

The mass function, 0·02 $M_{\odot}$, is very small and does not lead to much hope that any feature representing the secondary star should be seen in the radial-velocity traces. It has, however, been possible to measure an 11th-magnitude visual companion about 85″ north-preceding the principal star. The companion was observed by *Hipparcos* and has its own designation, *Tycho* 4190–256–1; the photometry derived from *Tycho 2* is $V = 10^{m}{\cdot}97$, $(B - V) = 0^{m}{\cdot}98$, so it may be supposed that the object has a type near K0. Four radial-velocity measurements have been made of it; they are mutually accordant, are listed in Table IV, and give a mean value of −51·8 ± 0·2 km s^{-1}. That is far from the γ-velocity of the principal star, clearly showing that the companion is only optical. Such a conclusion was to be expected in any case, because at the distance of HD 148294 — taken here to be a kiloparsec, notwithstanding its substantial uncertainty — the angular distance between the stars would represent a linear separation of 85 000 AU (well over a light-year; about ⅖ of a parsec) even if the two objects were at the same distance from us.

TABLE IV

Cambridge radial-velocity observations of Tycho 4190–256–1 (HD 148294 B)

Date (UT)	*Velocity km s^{-1}*
2002 Sept. 4·89	−51·6
Oct. 23·77	−52·2
2003 May 8·01	−51·3
2009 May 7·09	−52·1

HD 148800

HD 148800 is to be found nearly 1° south-following the $5^{m}{\cdot}7$ A-type star HR 6127 (actually a variable star of 'α^2 CVn' type; it is also designated DQ Dra). The star of present interest has been measured by *Hipparcos* at magnitudes (transformed to the *UBV* system) of $V = 8^{m}{\cdot}50$, $(B - V) = 0^{m}{\cdot}84$; the revised parallax[10] is 2·17 ± 0·67 milliseconds, equating to a distance modulus of about $8^{m}{\cdot}4 \pm 0^{m}{\cdot}7$. Thus the absolute magnitude of HD 148800 is close to zero, so the star can confidently be expected to be a normal giant, though the colour index then suggests a type of G5 III rather than K0 III.

Simbad knows of four papers that refer to HD 148800, but none of them tells us anything that is much worth recounting here.

HD 148800 was one of the stars that became included within Area 2 when the northern Clube Areas were increased in size in order to embrace larger samples of stars. The first radial-velocity observations were made (with the

TABLE V

Radial-velocity observations of HD 148800

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2002 Sept. 4·91	52521·91	−17·5	$\bar{5}$·608	−0·5
2003 Aug. 9·93	52860·93	−18·6	$\bar{4}$·635	+0·2
2006 Sept. 22·92	54000·92	−14·1	0·086	0·0
Oct. 24·80	032·80	−4·4	·183	+0·5
26·76	034·76	−4·1	·189	+0·5
Nov. 1·80	040·80	−4·9	·207	−1·0
Dec. 2·71	071·71	−2·5	·300	+0·7
9·72	078·72	−4·0	·322	−0·4
16·71	085·71	−3·9	·343	+0·2
2007 Mar. 27·14	54186·14	−19·9	0·647	−0·3
Apr. 10·14	200·14	−23·2	·689	−0·5
16·12	206·12	−23·6	·707	+0·4
30·10	220·10	−26·8	·750	+0·4
May 8·06	228·06	−28·9	·774	+0·2
19·04	239·04	−31·3	·807	+0·1
30·05	250·05	−33·5	·840	0·0
June 16·02	267·02	−35·9	·892	−0·5
27·95	278·95	−34·6	·928	+0·3
July 7·00	288·00	−33·4	·955	−0·2
12·99	293·99	−31·2	·973	+0·2
18·95	299·95	−29·0	·991	+0·1
24·97	305·97	−26·4	1·010	0·0
29·89	310·89	−24·3	·025	−0·3
Aug. 4·88	316·88	−20·8	·043	+0·2
9·91	321·91	−18·5	·058	−0·1
26·85	338·85	−11·4	·109	−0·4
Sept. 6·83	349·83	−7·8	·143	−0·2
Oct. 13·85	386·85	−2·9	·255	+0·1
Dec. 12·71	446·71	−6·8	·436	+0·6
2008 Oct. 16·79	54755·79	−4·9	2·372	0·0
27·77	766·77	−5·8	·405	+0·3
Nov. 22·73	792·73	−10·6	·483	−1·0
Dec. 6·71	806·71	−12·3	·526	−0·4
2009 Mar. 30·16	54920·16	−35·4	2·869	−0·6
June 21·93	55003·93	−9·4	3·123	+0·1
July 4·98	016·98	−6·1	·162	0·0
27·95	039·95	−3·5	·232	−0·2
Nov. 17·77	152·77	−15·1	·573	−0·4
2010 July 6·02	55383·02	−2·9	4·271	+0·1
Sept. 11·83	450·83	−9·1	·476	+0·2
21·91	460·91	−10·7	·506	+0·1
Oct. 6·81	475·81	−13·3	·551	+0·1
30·74	499·74	−18·0	·624	0·0
Nov. 15·73	515·73	−21·0	·672	+0·4
2012 June 29·00	56107·00	−8·6	6·462	0·0
Aug. 14·88	153·88	−16·5	·604	+0·2
2015 Oct. 2·79	57297·79	−16·6	10·068	+0·3
2016 June 7·06	57546·06	−32·0	10·819	+0·3

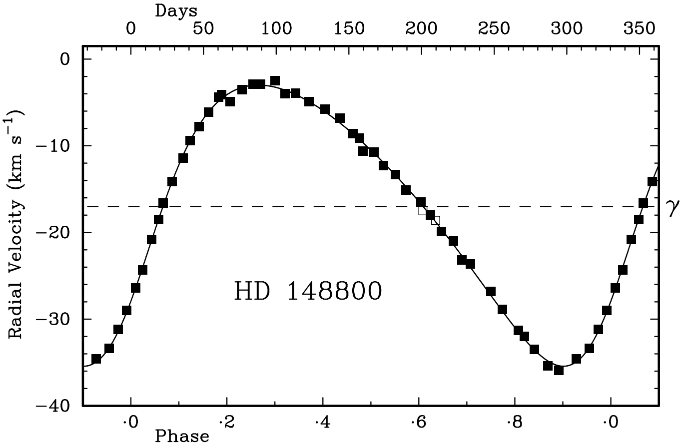

FIG. 5

The observed radial velocities of HD 148800 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel* and have been weighted equally in the orbit, but two that were made much earlier than the others (and fortuitously failed to disclose the velocity variation) are plotted with open symbols.

Cambridge *Coravel*, like all subsequent ones) in 2002 and 2003, and were in agreement with one another. (They just happened to be made at similar phases of the orbit, one cycle apart.) It was not until a third measurement was made, in 2006, that a modest discordance, of about 4 km s^{-1}, arose, and the star was promptly transferred to the binary programme. At the next observation, only a month later, the velocity had changed by 10 km s^{-1}! — so naturally it was then followed with greater frequency and enthusiasm, and the 11-month orbital period was determined in the ensuing observing season. Table V gives the 48

TABLE VI

Orbital elements for HD 146989, HD 148068, HD 148294, and HD 148800

Element	*HD 146989*	*HD 148068*	*HD 148294*	*HD 148800*
P (days)	3027 ± 5	247·08 ± 0·04	1039·9 ± 0·8	330·30 ± 0·08
T (MJD)	53464 ± 7	55677·43 ± 0·25	54136 ± 7	54633·1 ± 1·0
γ (km s^{-1})	−2·91 ± 0·04	−31·80 ± 0·13	−36·02 ± 0·04	−17·02 ± 0·06
K_1 (km s^{-1})	4·87 ± 0·5	27·66 ± 0·24	5·80 ± 0·05	16·22 ± 0·09
K_2 (km s^{-1})		31·22 ± 0·29		
e	0·457 ± 0·008	0·584 ± 0·004	0·198 ± 0·009	0·252 ± 0·005
ω (degrees)	255·7 ± 1·4	57·7 ± 0·8	75·3 ± 2·7	237·5 ± 1·2
$a_1 \sin i$ (Gm)	180·4 ± 2·0	76·3 ± 0·7	81·3 ± 0·8	71·3 ± 0·4
$a_2 \sin i$ (Gm)		86·1 ± 0·9		
$f(m_1)$ ($M_\odot$)	0·0256 ± 0·0008	0·290 ± 0·008	0·0199 ± 0·0006	0·1326 ± 0·0023
$f(m_2)$ ($M_\odot$)		0·418 ± 0·013		
$m_1 \sin^3 i$ ($M_\odot$)		1·49 ± 0·04		
$m_2 \sin^3 i$ ($M_\odot$)		1·32 ± 0·03		
R.m.s. residual (wt. 1) (km s^{-1})	0·25	1·22	0·27	0·37

measurements that have now been accumulated; the orbital elements derived from them appear in the last column of Table VI and the solution is illustrated in Fig. 5.

The mass function is significantly large, at 0·133 $M_{\odot}$. For putative primary masses of 1, 2, and 3 $M_{\odot}$, the companion would need to have minimum masses (corresponding to the case where $i = 90°$) of about 0·75, 1·1, and 1·35 $M_{\odot}$, respectively. Such masses belong to main-sequence stars whose respective types are about K2, G0, and F4, with absolute magnitudes of about 6·5, 4·4, and 3·2, so the secondary star might well not be seen in the radial-velocity traces. Even in the last case, there could be a difference of some three magnitudes (a factor of 16) between the components, and the secondary would give intrinsically a much shallower 'dip' than the primary in radial-velocity traces, so no significance can be read into the absence of evidence of the secondary star.

References

(1) R. F. Griffin, *MNRAS*, **219**, 95, 1986.
(2) R. F. Griffin, *ApJ*, **148**, 465, 1967.
(3) R. F. Griffin, *The Observatory*, **90**, 46, 1970.
(4) J. Evershed, *Kodaikanal Bull.*, no. 32, 1913.
(5) P. Fellgett, *Optica Acta*, **2**, 9, 1953.
(6) H. W. Babcock, *Annual Report of the Director of Mount Wilson and Palomar Observatories, 1954/55*, 1956, p. 27.
(7) R. F. Griffin & A. P. Cornell, *MNRAS*, **371**, 1140, 2006.
(8) R. F. Griffin, *MNRAS*, **201**, 487, 1982; **210**, 745, 1984, **212**, 663, 1985.
(9) R. F. Griffin & A Stroe, *JA&A*, **33**, 245, 2012.
(10) F. van Leeuwen, *Hipparcos, the new reduction of the raw data* (Springer, Dordrecht), 2007.
(11) T. Schmidt-Kaler, in K. Schaifers & H. H. Voigt (eds.), *Landolt-Börnstein* (Springer-Verlag, Berlin), **2b**, 1982, pp. 15, 18.
(12) [Announced by] B. Famaey *et al.*, *A&A*, **430**, 165, 2005.
(13) W. Tirion, B. Rappaport & W. Remaklus, *Uranometria 2000·0 Deep Sky Atlas* (2nd edn.) (Willmann-Bell, Richmond, Virginia), 2001.
(14) R. F. Griffin, *Ph.D. Dissertation* (unpublished) (Cambridge University), 1960, Table 1.
(15) R. F. Griffin, *MNRAS*, **122**, 181, 1961.
(16) R. F. Griffin, *MNRAS*, **147**, 303, 1970.
(17) *The Hipparcos and Tycho Catalogues* (ESA SP-1200) (ESA, Noordwijk), 1997, **8**, p. 1612.

CORRESPONDENCE

To the Editors of 'The Observatory'

Fred Hoyle's unpublished theory of a 12×10^9-yr-old Solar System

In the autumn of 1997 Fred Hoyle suffered an accident while walking in Shipley Glen near his family home in Gilstead, West Yorkshire. It took the best part of three months for a broken shoulder to heal and another month before he was able to write again. It was towards the end of his convalescence that he revisited a problem that he had first began to ponder in the 1930s in collaboration with Ray Lyttleton[1,2]. An unpublished document written in 1997 that develops a remarkable and provocative theory is currently lodged in the archives at St John's College, Cambridge. The document is titled *A Different Approach to the Age of the Earth*. He had shown this to several friends and colleagues who urged him to publish, but it remains unpublished to the present day.

Once or twice a month Fred would go out for the day with one of us (GH), often lunching in a Devon or Cornish pub. The conversation on those occasions would swing through many subjects, and inevitably on one particular day the age of the Sun and planets reached the top of the agenda. There were two avenues he said he was following. One was to explain the very large angular momentum of the Sun in its orbit around the centre of the Galaxy, and the second was to explain the distribution of angular momentum between the Sun and the planets. The Sun carries 99% of the mass of the Solar System but only contained one per cent of the total angular momentum, a situation that demanded an explanation.

The answer to the second question had progressed well through Fred's career and an account of it was set out, for instance, in his collaboration with one of us (CW) in a paper published in 1968[3]. But the answer to the first of the problems had continued to elude him. The difficulty was to explain why the *total angular momentum per unit of mass* in the Solar System had exactly the value it had. The nub of the problem, it seemed to Fred, lay in describing the Sun as a star in a wrong sort of galaxy, in an elliptical galaxy instead of a spiral galaxy — bizarre as it would appear at first sight, but by no means absolutely impossible.

By examining the way that angular momentum is distributed in galaxies, it became clear to Fred that the Solar System's value for the angular momentum (per unit of mass) was correct for stars in elliptical galaxies; it was not correct for stars in the spiral arms of galaxies like our Milky Way. To be right for our Galaxy the Sun had to be a much older, so-called Population II star, which stars are mainly located in the Galactic bulge and halo. Pursuing this line of thinking would surely put the cat amongst the pigeons, but such considerations never deterred him. For many decades astronomers had been working on the assumption that the Sun was a so-called Population I star about 5×10^9 years old. Could the Sun really be a Population II star that is nearly 10×10^9 years old?

Fred Hoyle pointed out that the present appearance and state of the Sun is equally well explained for an older star if the starting conditions were different from what are generally assumed. In particular he pointed out that the initial ratio of helium to hydrogen was an arbitrary input in standard calculations that he himself had pioneered[4], and could be easily changed to produce a greater age of the Sun.

The only difficult sticking point, however, was the age of the Earth that all geologists without exception would swear to be $4{\cdot}5 \times 10^9$ years. That canonical age estimate is based mainly on radioactivity of surface rocks. The age could be wrong only if an impact event or events that happened $4{\cdot}5 \times 10^9$ years ago actually delivered the uranium-laden younger rocks. Similarly the well-attested ages of meteorites could also be explained. Later impacts at $4{\cdot}1 \times 10^9$ years ago would mark the inception of biology on the Earth[5,6]. The radioactive clocks from which ages of rocks are determined were set at the time of a more recent supernova event. Not impossible, but could perhaps be criticized as an artificial fix! Only by invoking the anthropic principle can that criticism be overcome. Yes, an accident it was, but if not for that accident we would not be here to talk about it! That was another example of the so-called anthropic principle that Fred had inaugurated in another context — a prediction of an excited level of the carbon nucleus[4].

Yours faithfully,
GEOFFREY HOYLE &
CHANDRA WICKRAMASINGHE*

* Churchill College
Cambridge, CB3 0DS
and
Buckingham Centre for Astrobiology
Buckingham University
Buckingham, MK18 1EG

2016 July 8

References

(1) F. Hoyle & R. A. Lyttleton, *Proc. Camb. Phil. Soc.*, **35**, 405, 1938.
(2) F. Hoyle & R. A. Lyttleton, *Proc. Camb. Phil. Soc.*, **36**, 325, 1938.
(3) F. Hoyle & N. C. Wickramasinghe, *Nature*, **217**:5127, 415, 1968.
(4) E. M. Burbidge *et al.*, *Rev. Mod. Phys*. **29**, 547, 1957.
(5) F. Hoyle & N. C. Wickramasinghe, *Astronomical Origins of Life: Steps towards Panspermia* (Kluwer, Dordrecht), 2000.
(6) N. C. Wickramasinghe, *The Search for Our Cosmic Ancestry* (World Scientific, Singapore), 2013.

The Heliographic Latitude of the Sunspot of 1676 June

The Maunder Minimum was an epoch with prolonged low solar activity which occurred during the second half of the 17th Century[1]. It is generally accepted that this phenomenon spanned the period 1645–1715, although some authors have proposed a redefinition of its extent[2]. That was the only grand minimum of solar activity registered during the telescopic era and it is of great interest for solar physics and geophysics owing to its importance for the behaviour of long-term solar activity and its influence on the climate of our planet[3,4].

Hoyt & Schatten[5] compiled a large number of observations corresponding to the Maunder Minimum, obtaining good temporal coverage. However, a part of those observations was obtained from solar meridian observations[6]. Recently, Vaquero *et al.*[7] have presented a revised collection of the sunspot group number based on the data-base of Hoyt & Schatten, including the observations corresponding to the Maunder Minimum. They have discarded those problematic observations located in Hoyt & Schatten's data-base and they have also added some new records. The level of solar activity during the Maunder Minimum is currently a controversial topic. Zolotova & Ponyavin[8] have suggested that the Maunder Minimum was not a grand minimum and they estimated solar-maximum amplitudes significantly greater than the level obtained by Hoyt & Schatten[5]. However, Usoskin *et al.*[9], in response to Zolotova & Ponyavin[8], revised the information available for that epoch and they concluded that the Maunder Minimum was in fact a grand minimum. The level of solar activity during the Maunder Minimum obtained from the revised collection of data by Vaquero *et al.*[7] is also compatible with a grand minimum.

During the Maunder Minimum, most sunspots appeared on the solar southern hemisphere, presenting a strong north–south asymmetry[10]. Spörer[11] published several sunspot heliographic-latitude records made by observers during the Maunder Minimum. However, that source does not contain the sunspot observation registered by Cassini in 1676 June[12]. Therefore, we present the original extract of this record and a translation of the Latin text into English.

Original text: "[...] *Habemus in Sole satis ingentem Maculam, que Solem ipsum mediavit die 28 Junii h. 4. Post meridiem, cum latitudine Australi 4′ ¼; ejus distantiam à polo Australi Solis ex pluribus observationibus supputavi gr. 78¼. Si satis habuerit consistentiae ad absolvendum circulum, expectanda restitutio ejus ad medium diei 25 Julii, vespere, cum majore latitudine Australi.*"

English translation: "[...] We have on the Sun a rather large sunspot which was placed in the centre of the Sun on June 28, at four p.m., with 4′¼ southern latitude; its distance from the south pole of the Sun, from several observations, I calculated as 78¼ degrees. If it had enough consistency to complete its cycle, the returning sunspot would be expected to cross the meridian on July 25, in the afternoon, with latitude further south."

First, we highlight that the date assigned to Cassini's letter in the original source is a typographical mistake and this sunspot was observed in 1676 (not in 1671 as can be seen in the original date of the letter). Second, we can extract from the text that the heliographic latitude measured by Cassini for this sunspot was approximately –12°, *i.e.*, the sunspot was located in the southern hemisphere. One can see in Spörer[11] that the heliographic latitude determined by Lalande for this same sunspot observed from June 26 to July 1 was equal to –13°. Thanks to the similarity of the values, these two independent observations validate the measurements of the heliographic latitude of that sunspot record.

Yours faithfully,

VÍCTOR M. S. CARRASCO

Departamento de Física
Facultad de Ciencias, Universidad de Extremadura
Avda. Elvas, s/n
06006 Badajoz, Spain

and

JOAQUÍN VILLALBA ÁLVAREZ

Facultad de Filosofía y Letras, Universidad de Extremadura
Facultad de Ciencias, Universidad de Extremadura
Avda. de la Universidad s/n
10003 Cáceres, Spain

2016 July 7

References

(1) J. A. Eddy, *Science*, **192**, 1189, 1976.
(2) J. M. Vaquero & R. M. Trigo, *New Astron.*, **34**, 120, 2015.
(3) J. M. Vaquero & M. Vázquez, *The Sun Recorded Through History* (Springer, Heidelberg), 2009.
(4) I. G. Usoskin, *Living Rev. Solar Phys.*, **10**, 1, 2013.
(5) D. V. Hoyt & K. H. Schatten, *Solar Phys.*, **179**, 189, 1998.
(6) F. Clette *et al.*, *Space Sci. Rev.*, **186**, 35, 2014.
(7) J. M. Vaquero *et al.*, *Solar Phys.*, in press, 2016.
(8) N. V. Zolotova & D. I. Ponyavin, *ApJ*, **800**, 42, 2015.
(9) I. G. Usoskin *et al.*, *A&A*, **581**, A95, 2015.
(10) J. C. Ribes & E. Nesme-Ribes, *A&A*, **276**, 549, 1993.
(11) G. Spörer, *Nova Acta der Ksl. Leop.-Carol. Deutschen Akademie der Naturforscher*, **53**, 281, 1889.
(12) G. D. Cassini, *Phil. Trans.*, **11**, 665, 1676.

REVIEWS

The History of the Universe, by D. H. Lyth (Springer, Heidelberg), 2016. Pp. 120, 23·5 × 15·5 cm. Price £16·99/$29·99 (paperback; ISBN 978 3 319 22743 6).

This book is the 46th to be published in the series *Astronomers' Universe* that Springer launched a decade ago. The series is aimed at active amateurs and a wider readership of astronomically savvy readers or 'armchair astronomers'. These are not primers, nor are they popular-science or trade books. The assumption is that readers will already have a basic knowledge of astronomical concepts and nomenclature, and that allows for a compact presentation. The books are at a slightly higher level than the Oxford series of *Very Short Introductions*: informative graphs and tables are plentiful here, as well as equations with calculus. But nothing too scary.

David Lyth has crafted a thoroughly engaging history of the Universe in 17 very short chapters. He starts by clearly explaining his terms of engagement: to use elementary physics to explain the Universe. Chapter 3, for example, is a masterpiece of concision and clarity: the importance of General Relativity and the standard model is reduced to 400 words, and the history of cosmological physics from 1687 to the present wrapped up in about 600 words. Lively and colloquial story-telling takes the reader on a splendid cosmic voyage through time and space, in which by turns the Big Bang, inflation, galaxy formation, the microwave background, the Higgs field, and many other actors take a bow. Lyth's toolbox for the final assembly line of his narrative makes exceptionally good use of physics. I liked a short paragraph on thermodynamics that begins "Suppose I get into my car on a cold morning". Which is followed by a homely analysis of the heat flows within the car, including that from the human body.

This is an exceptionally good example of scientific writing in the short format, as exemplified by Jeans, Eddington, Hoyle, and Hawking. Armchair astronomers will surely like this book, but who else? I recommend it to those in high school aspiring to study for degrees in physics and astronomy, as well as the instructors of their first-year courses. The book delivers a big boost for the power of physics to aid our understanding of the Universe. — SIMON MITTON.

A Passion for Space: Adventures of a Pioneering Female NASA Flight Controller, by M. J. Dyson (Springer, Heidelberg), 2016. Pp. 381, 23·5 × 15·5 cm. Price £24·99/$39·99 (paperback; ISBN 978 3 319 20257 0).

Marianne Dyson already has an established reputation as a writer on topics to do with space and space science, in particular in writing for children. Most of her output, including magazine and journal articles, focusses on some specific aspect, but here we have the full story — the autobiography. Starting from a rather humble and disjointed background in the Midwest, and culminating at the Johnson Space Center in Houston at the prestigious level of Flight Controller *via* a number of hair-raising adventures on the way, Dyson's achievement of a career in space science has made her an important role model. The same passion that so enthused her at the moment of the 1969 Moon landing throbs through the pages, a passion that is infectious and also vital for any ambassador to children.

The 1980s (the decade in question) were uncomfortable years for any woman in science, particularly for self-made people like Dyson. The debates between Equal Opportunity and tradition were public, often noisy, and mostly very polarized, and one could feel some sympathy for organizations which had strict schedules to keep, when members of its highly trained work-force requested to put family matters first, possibly with inconvenient dates. The battle of Equal Opportunity has been a long haul and is still by no means won, and it came as a surprise (to me) that such blinkered policies as those at NASA could still be practised in the US when Europe had already started to address them. Part-time employment and paternity leave are now part of the system, but it took the courage of people like Dyson to help bring the need for those improvements into focus, even at the cost of a somewhat premature end to her own career with NASA. But she does not let the matter rankle, or dampen her fascination for the operations at NASA of which she became part; it was only when faced with difficult choices between time-critical assignments and family needs that she started to look seriously at the statistics.

The book is well written, and flows energetically with a diarist's detail through all the many assignments to which she was entrusted at NASA, however rigorous and unforgiving the duties. It is made the more readable by personal interjections all through: what it actually felt like when one's parents divorced, how to obtain qualifications when even the school didn't seem to care about the future of one of its brightest pupils, how to work round obstacles without a guide or mentor, and how to turn roadblocks into instruments of experience and resourcefulness.

If there is one criticism, it concerns the mix of technical NASA-speak (fortunately there is a glossary) with personal passages about maternal instincts, a mix which perplexed me somewhat when identifying her intended readership. A complex person with a complex background needs to trim the tale in places so that it flows for the great majority, and for me it would have been easier if the technical accounts were replaced with layman's language. That is not to suggest watering-down; the details can still be there, but would be more digestible if the actual role of the person or equipment were given rather than the in-house acronym.

This book encourages, inspires, and delights, and should be on the reading list of everyone — educators, scientists, and particularly women — scientists or not. — ELIZABETH GRIFFIN.

Weird Astronomical Theories of the Solar System and Beyond, by David A. J. Seargent (Springer Heidelberg), 2016. Pp. 270, 23·5 × 15·5 cm. Price £26·99/$34·99 (paperback; ISBN 978 3 319 25293 3).

The word 'weird' has a rather appealing ring to it. It intimates unorthodoxy, excitement, radicalism, strangeness, thinking outside the box, the bizarre; and a departure from the normal conservatism (with a small 'c' of course) of the typical professional astronomer. Maybe we do not want to go right back to Shakespeare's *Macbeth* and the three weird sisters, but at least we can go back to Albert Einstein and his famous quote "the most incomprehensible thing about the universe is that it is comprehensible" (*Physics and Reality*, 1936).

In his latest book, David Seargent (the Australian comet expert) dives into a host of strange and weird theoretical proposals associated with our Solar System. The main thesis is to contrast them with the highly orthodox views that most astronomers enjoy holding today. We start with the origin. The 'normal' approach, which ties planetary origin to an equatorial nebular cloud of gas and dust left behind by the shrinking infant Sun, is compared with ideas based on stars nearly colliding and pulling material out of each other. Then we have the orthodox proposal that the planets today have the same ordering as they always did, contrasted with the possibility of major planetary migration and Velikovsky's most unusual suggestion that Venus is a relative newcomer to the inner system. In 'the good old days' comets were comets and asteroids were asteroids, but we are now encouraged to adopt a more fluid boundary. And then we have the 'popular' suggestion that comets are just some of the left-over building blocks of the rather messy and wasteful production of the gas-giant planets balanced against the ideas of Vsekhsvyatskii who envisaged comets as being condensing ejecta from the volcanic satellites of Jupiter and Saturn. The origin of life is considered. Did we all develop from warm, slimy, tidal water pools on the early Earth, or was interplanetary and interstellar panspermia the seeding mechanism? Turning to the development of our planetary surface and biosphere, just how big a role did asteroidal impact play?

Scientific theories can easily slip into dogma. They need to be continually challenged. We need to be bold and adventurous and Seargent is encouraging us. Don't become completely crackpot but at least consider the fact that weird and strange things can happen and weird and strange theories might have a large grain of hidden truth. This is an extremely enjoyable and thought-provoking book. It should do much to shake us out of our complacency. — DAVID W. HUGHES.

Eclipses, Transits, and Comets of the Nineteenth Century: How America's Perception of the Skies Changed, by Stella Cottam & Wayne Orchiston (Springer, Heidelberg), 2015. Pp. 336, 23·5 × 15 cm. Price £90/$129 (hardcover; ISBN 978 3 319 08340 7).

The authors focus on the total solar eclipses of 1868, 1869, and 1878 and the transits of Venus of 1874 and 1882, with shorter treatments of meteor showers and transits of Mercury. The primary point is that the observations of those events by both amateur and professional astronomers and the media coverage they received made a large contribution toward support for astronomy and science in general in the late 19th Century in the United States, which had lagged behind Europe earlier in the century. Less pompously, this is a delightful volume to have and to hold, simply brimming with wonderful pictures of people, places, astronomical images and the beginning of spectroscopy, historical markers, and so forth.

The pictures of Richard Proctor (a pioneer of astrophotography) and Sir George Airy (an Astronomer Royal who did not discover Neptune) are caricatures, but they were perhaps not terribly handsome to begin with. The very large number of specialized, small-circulation newspapers and magazines came as a surprise to me. They had names like *Eclectic Magazine*, *Flag of Our Union*, *Appleton's Journal*, and *Massachusetts Ploughman and New England Journal of Agriculture*. A surprise of the contrary sort is that the eclipse of 1842 was the first to inspire the initiation of formal expeditions.

Women were involved in many of the eclipse and transit expeditions and some, at least, participated by sketching the corona in real time. Yes, such drawings look quite different from photographs, and neither is entirely 'right'. They also wrote in periodicals intended for female readership, general publications, and sometimes even books, like *Familiar Astronomy* by Hannah M. Bouvier (later Peterson), 1st edition 1855.

But things really are better now, at least in some ways. Chabot Observatory, near Oakland California, averaged about 1600 visitors per year in the 1880s. There were 29 069 in 1975–76!

Conflict of interest statement: my copy was neither purchased nor sent by the publisher for review, but was free because it has been nominated for a book prize given by a committee of which I'm a member. I wish we had eight prizes to give, because every nominee is special in some way, this one in its unusual point of view and fabulous 'visuals'! There are also maps and pages of books and lab notebooks and more. — VIRGINIA TRIMBLE.

The Moon's Largest Craters and Basins, by C. J. Byrne (Springer, Heidelberg), 2016. Pp. 246, 26 × 18 cm. Price £19·99/$34·99 (hardbound; ISBN 978 3 319 22031 4).

Charles J. Byrne is already well known for his published contributions to lunar studies, including his useful atlases of the nearside and farside of the Moon, based upon *Lunar Orbiter* imagery. With the present book he has provided the student of our satellite with another useful resource. Our understanding of lunar impact basins and proto-basins has been much refined in recent times by results from an armada of spacecraft, and Byrne has drawn upon data from the *Lunar Reconnaissance Orbiter*, *GRAIL*, and *Kaguya* missions to compile an informative catalogue of 72 of the Moon's largest impact features. This catalogue includes all basins, defined as features more than 300 km in diameter, as well as the larger craters (over 200-km diameter). Clear double-page spreads devoted to each feature are arranged sequentially according to relative ages as determined by stratigraphic methods, as well as by relative crater densities and evidence of degradation. The catalogue entry for each feature gives a medium-resolution mono image and a false-colour topographical map (both derived from *LRO* data), a radial elevation profile based on *Kaguya* results, Bouger gravity-anomaly maps derived from *GRAIL*, and various quantitative data including a horizontal distance scale.

The catalogue itself is preceded by chapters explaining the data and the methods used to determine the sequence, including estimates of feature diameter and initial depth in addition to the methods mentioned above. The first item in the sequence is the still-hypothetical nearside megabasin (NSM), about which Byrne has already written a separate monograph. In an appendix to the present book he argues the case that the creation of the NSM and the depositing of ejecta at its antipode account for many of the differences between the nearside and farside hemispheres. Further appendices consider the origins of the extensive lava fields making up the Procellarum region and the nature of the South Pole–Aitken Basin.

Although many uncertainties still attend the sequence of events giving rise to the Moon's impact basins, this book nevertheless provides much essential information about those features in a relatively compact and convenient form. It will prove of interest to all selenographers and lunar geologists, amateur and professional. — BILL LEATHERBARROW.

Moons of the Solar System: from Giant Ganymede to Dainty Dactyl, by James A. Hall III (Springer, Heidelberg), 2016, Pp. 297, 23·5 × 15·5 cm. Price £19·99/$34·99 (paperback; ISBN 978 3 319 20635 6).

Our planetary moons come in three types: co-genic, captured, and mystery. The co-genic moons were formed at the same time as the parent planet, out of the same condensing cloud of gas and dust, and are relatively large and in the equatorial plane. The four Galilean satellites of Jupiter are perfect examples. The captured moons have been stolen from the asteroid belt, and as you would expect are small, irregular, and asteroidal. Phobos and Deimos, the two moons of Mars, are typical examples. Finally you have the intriguing mystery moons — Charon the moon of Pluto with its $\frac{1}{12}$ mass ratio with its primary, hinting at the bifurcation of a pear-shaped shrinking fluid body (as predicted by Sir James Hopwood Jeans). And then there is our lonely Moon. If it was ripped from the mantle of the early Earth by a massive asteroidal impact why does Earth have just the one? Why didn't other asteroidal impacts produce other moons? And why are Mercury and Venus moonless?

James Hall has produced a beautifully illustrated book which gives a detailed description of each moon in turn. But description is really all you get. I was expecting a bit more. How long did it take our Moon to fall into a 1:1 spin–orbital resonance? How has the Earth–Moon distance changed with time? Why does one side of our Moon differ so much from the other? How long did it take the Jovian and Saturnian moons to establish their orbital resonances? Why did distance from the parent planet have such a huge effect on their physical characteristics? What is the relationship between moons and rings? Could Pluto have escaped from Neptune or Uranus? Is it possible that Mercury was once a moon of Venus? When will Phobos hit Mars? There is so much we do not know about these enigmatic members of the Solar System. Maybe the author is saving up his answers for a second volume. — DAVID W. HUGHES.

Essential Radio Astronomy, by J. J. Condon & S. M. Ransom (Princeton University Press, Woodstock), 2016. Pp. 363, 26 × 18 cm. Price £59/$85 (hardbound; ISBN 978 0 691 13779 7).

Any radio astronomer in an educational establishment will most likely be invited to give a course of lectures to instruct the new intake of graduate students. Here is the good news: this book is just what you will need to help you on the way. There are seven chapters on specific subject areas, such as 'Radio telescopes and radiometers', 'Synchrotron radiation', and appendices such as 'Fourier transforms', and a good set of reference material. Virtually every topic covered is recognizable as one which my colleagues or I have covered in lecture courses. The topics are treated in a clear manner, not making too much in the way of assumptions, and including about the right amount of historical background. I can foresee a number of lecturers round the world adapting their presentations to make good use of the material here.

The text is available on the web, together with other material such as examples. I have not, I fear, read every single word in the book, but so far I have found no typos or other mistakes — well done to the proof-reader(s). The only gentle moan that I have is that the use of cgs units and equations alongside SI leads to the appearance of units like 'statcoulomb', when I thought that they had long since been discarded. (When I was an undergraduate in my first year, we had a charismatic, very clever lecturer who took the view that "a physicist should be able to use any appropriate set of units"; I think that he managed to inspire a good number of students, but that he also lost many of the others. After I started teaching, I believed that the clearest way to proceed is to use SI more or less uniformly.)

There is a set of colour plates, well-reproduced (though perhaps it is a shame there are no images of any of the classic Cambridge radio telescopes). — GUY POOLEY.

Principles of Applied Remote Sensing, by S. Khorram *et al.* (Springer, Heidelberg), 2016. Pp. 307, 24 × 16 cm. Price £55·99/$99 (hardbound; ISBN 978 3 319 22559 3).

This book aims to be a study of the entire field of remote sensing for a readership of undergraduate through to graduate level. This scope and depth would be challenging to fit into a thousand pages, so to fit it into three hundred the authors have had to make some compromises. The majority of the book focusses on satellites and satellite instruments; other platforms are discussed early on, but when data sets are discussed it is nearly always space-based assets that are used as exemplars. That is perhaps understandable given the amount and ease of access to satellite data, but there is considerable reliance on more-modest platforms and it is on those that most new sensing methods are first developed. Remotely piloted vehicles, particularly small 'drones', are likely to produce enormous amounts of data in the future, so discussion of their particular issues would have been timely.

I found the title a little confusing — the leading "Principles of" suggested that there would be explanations of the key insights on which instruments have been designed or data reduced. Some are there to be found, so the section covering accuracy assessment describes error-matrix analysis briefly and succinctly. But an earlier chapter considers the concept of atmospheric and radiometric corrections, then refers the reader to other sources to find any example of how they might be performed. Overall the book reads far more like a review; even the problem sections are populated with review questions. In fact it is in the references, suggested reading lists, and directions to other sources of information that the book excels. The earlier chapters on data acquisition and processing ensure that the latest methods, such as fuzzy-logic land-surface-change detection, are given as references. The 'Terrestrial' chapter gives good descriptions of the issues of measuring biomass in forests and how specific methods, such as SAR, LiDAR, and altimetry, can be used together to provide better estimates. The chapter on 'Coastal and ocean ecosystems' provides a clear overview of observation techniques and their outputs, and finishes with an indication of the impact those measures can have on the commercial uses of the sea for both good and ill.

The chapter on 'Planetary and extrasolar observation' is a brief guide to our race's exploration of the Solar System, which is a well-written timeline covering the majority of major missions, together with an even briefer section on extrasolar remote sensing which shows a bias towards US space assets. Whilst *Hubble* has been the source of numerous breakthroughs, so have *Herschel* and *XMM-Newton* and they deserve references. The concept that observing distant stars through ground-based telescopes is a form of remote sensing isn't apparent, and the obvious reference to Michel Mayor and Didier Queloz's breakthrough discovery of 51 Pegasi b when discussing the first discoveries of exoplanets is missing.

At the end of the book are two shorter chapters, the first of which covers international and legal aspects of space-based Earth observation with as much detail as most people would want. The second is a brief discussion of future trends, when remotely piloted aircraft (UAVs) are mentioned and some conjectures on crowd-sourced and big data are expounded.

On finishing the book I realized that, with the exception of the 'Extrasolar' section and a few of the badly reproduced figures and tables, I had found the book informative and easy to read. I would recommend it as a review of a very large subject area, one to have on the shelf and lend to others perhaps, since it is best used as a guide to other more specific works. — ANDY VICK.

Introduction to Stellar Structure, by W. J. Maciel (Springer, Heidelberg), 2016. Pp. 215, 24 × 16 cm. Price £35·99/$49·99 (hardbound; ISBN 978 3 319 16141 9).

I have many books on stellar structure on my shelves, from Eddington's classic of 1926 (not 1930, as quoted on p. 96) to the two comprehensive 2012 volumes by Iben, but not many have stood the test of time. Those that have, in addition to Eddington and Iben, tend to be more than just textbooks, and include (in alphabetical order) Chandrasekhar, Cox & Giuli, Hansen & Kawaler, Kippenhahn & Weigert, and, of course, Schwarzschild. Other books are aimed more at educating the new student and are valuable for that purpose. The book under review seems to me to fall into the second category. It is a translation (presumably by the author, since no other translator is credited) of a Portuguese original published in 1999, and the English is generally good, with some verbal infelicities and a few places where the meaning is obscured by the style.

However, I also found the style rather breathless — there are no wasted words, and explanations, though usually clear, tend to be very concise, so the student has to think (no bad thing, perhaps). There are useful exercises (with very concise solutions) at the end of each chapter, which will help the thinking process. The book arose from a set of graduate lectures, so the reader is assumed already to have a good knowledge of physics and mathematics, and the book concentrates on the application of physics to stellar conditions. There is a bibliography of books for further reading at the end of each chapter, which inevitably involves some repetition but does allow the author to make comments on them that are relevant to the chapter.

There are a couple of places where the translation has not taken the opportunity to bring the original up to date: when discussing spectral classes, there is no mention of the very cool L, T, and Y dwarfs revealed in large numbers in the last 15 years by IR surveys such as 2MASS, and the section on solar neutrinos says that the "solar neutrino problem is still under discussion", although it does mention the MSW effect and *SNO*, without adding that the *SNO* data, published just after 1999, made the explanation in terms of neutrino oscillations generally accepted.

Because the author's style is very different from my own, there were quite a few places where I felt that he had got the emphasis wrong without actually saying anything wrong (although he does claim on p. 185 that novae are caused by mass transfer from a red *giant*, which is certainly not the case for most novae). However, I will only mention one example, which is his treatment of polytropes in Chapter 6. Here he unnecessarily complicates the derivation of the Lane–Emden equation by introducing the temperature, which strictly speaking has no place in the discussion of polytropes, which are entirely defined by a pressure–density relation; he thus conflates the application of the equation to real stars with the equation itself — not wrong, but I would have separated those ideas.

In summary, this book is like the proverbial curate's egg: good in parts. It is not the first book I would recommend to graduate students beginning to study stellar structure, but it might be on a longer list of relevant books. — ROBERT CONNON SMITH.

Dynamics of Young Star Clusters and Associations, by C. J. Clarke, R. D. Mathieu & I. N. Reid (Springer, Heidelberg), 2015. Pp. 348, 24 × 16 cm. Price £67·99/$99 (hardbound; ISBN 978 3 662 47289 7).

As the title suggests, this book covers the dynamics of young stellar systems. As usual for a Saas-Fee volume this is both a good introduction to the field, and a useful reference for those who already work in the area.

All three contributors provide very good, readable, and thorough overviews of a wide variety of topics. Both observations and theory are covered in enough detail to follow the basic arguments, and plenty of good references are provided to go deeper into any area.

Cathie Clarke's section is first, which covers the formation of clusters/associations. It focusses on the theoretical/simulation side, but with some really good discussion of the physics going into simulations, and numerical problems. I would recommend this section to any observer wanting a quick overview of what simulations are and how they work.

Robert Mathieu reviews the observation and theory of N-body stellar systems once they have formed. There is a good introduction to N-body dynamics and time-scales, and nice examples of different regions comparing theory and observation.

Neill Reid finishes the volume by putting the cluster and associations in a Galactic context. His title 'From whence the field' sums this up better than I could, and also moves away from young clusters to including historical star formation back to globular clusters.

This is a great book for both observers and theorists in any area around star formation, star clusters/associations, and galactic dynamics — especially timely with *Gaia* data arriving soon. I gave it to my PhD students with the instruction "read this from cover to cover — it's really useful". — SIMON GOODWIN.

Tidal Streams in the Local Group and Beyond: Observations and Implications, edited by H. J. Newberg & J. L. Carlin (Springer, Heidelberg), 2016. Pp. 250, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 19335 9).

The phase space defined by three dimensions of configuration space and three dimensions of the associated velocities is a rich and powerful way to describe the way in which galaxies like our Milky Way assemble and evolve. Sub-structure in that space is especially attractive, being both relatively easy to find, and a valuable tool to measure gravitational potential gradients. From these follow our knowledge of the distribution of dark matter, and the kinematic evolution of our spiral plus bar, disc, and inner bulge.

This book focusses (mostly) on substructures generated by the tidal dissolution of previously gravitationally-bound small stellar systems — satellite galaxies and globular star clusters. The subject has a long history, but sprang into vigorous maturity with the local disc complexity defined by the *Hipparcos* astrometry, and the discovery of stellar streams at higher latitudes. Halo streams started being discovered in the early 1990s (see, *e.g.*, *MNRAS*, **257**, 225, 1992) and became mainstream with the 1994 discovery of the Sagittarius dwarf galaxy, and its all-sky tidal tails. The poster-child is the 'Field of Streams' map of high-latitude sub-structure derived from the Sloan Digital Sky Survey by Vasily Belokurov and colleagues in 2006. A similar richness is evident in M 31, well-described in this volume by Annette Ferguson and Dougal Mackey. The application of stream dynamics — and holes — to probe dark-matter distributions is beginning, but deep down everyone keeps saying "wait for *Gaia*, then the revolution begins!".

Not long to wait now. In the interim this book provides a good introduction and overview of the field from a somewhat SDSS-centric perspective. — GERRY GILMORE.

OTHER BOOKS RECEIVED

Calibration and Standardization of Missions and Large Surveys in Astronomy and Astrophysics (ASP Conference Series, Vol. 503), edited by S. Deustua, S. Allam, D. Tucker & J. A. Smith (Astronomical Society of the Pacific, San Francisco), 2016. Pp. 289, 23·5 × 15·5 cm. Price $88 (about £58) (hardbound; ISBN 978 1 58381 890 9).

These proceedings report the deliberations of a meeting at Fermilab in 2012 April whose primary aim was to "foster communications among the various large surveys and missions".

Here and There

DIVINE OBJECTS

Variable stars are used to measure the dimensions and study the spiritual structures of remote corners of the vast Milky Way. — *The New York Times*, 1956 May 6.