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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2008 January 11th at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome to this ordinary meeting of the Royal Astronomical Society. First of all, I'd like to wish everyone a happy New Year. There's a change to the programme and I'm going to take it in a slightly different order: I'm going to take the RAS response to the STFC budget cuts first. The main thing I'd like to do here is really to have a discussion, but I will first bring you up to date with what has been happening.

At the time of the last meeting, we had already had the sudden and shocking announcement of withdrawal from the *Gemini* observatory, followed by the news of cuts to grants of 25%. We reacted very strongly to both of those announcements and secured a good deal of coverage in both the press and broadcast media, and I think set the agenda for the discussion of the whole STFC process. The result was that although Ministers were hoping that the announcement of the outcome of the Comprehensive Spending Review (CSR) would be good news, which in a way it is, with big increases in funding for science, the way the money has been distributed has meant that astronomy and particle physics have ended up facing severe cuts.

Since the previous meeting, there have been two developments: on the day that John Denham announced the outcome of the CSR, he had hastily organized a review into the health of physics, chaired by Professor Bill Wakeham of Southampton University — a very important concession. The Minister is not saying that he deliberately wanted to cut astronomy and particle physics; he seemed to be as surprised as much as anybody at the outcome, and has set up this review to consider the implications for the health of physics. That is a very important opportunity for us to make the case for our science.

The second development was that Keith Mason asked me to set up a meeting with a group of senior astronomers, and that took place on Wednesday afternoon this week. Keith had a meeting with a group of particle physicists in the morning. I will shortly give you the details of a statement that we have just put out following that meeting.

Before that, the other thing on the horizon is that the Innovation, Universities and Skills Select Committee are starting a series of hearings, and the Institute of Physics (IoP) and the RAS will attend those hearings on January 21. The RAS and IoP have combined forces to send in a written statement to the Select Committee, so we are trying to maintain a common front with the IoP and particle physicists.

[The President then summarized the discussion of the meeting of the *ad-hoc* group with Keith Mason, together with several recommendations arising from the discussion which have been proposed by the group. This was formalized in a statement which was later circulated to RAS Fellows:

"At the request of Keith Mason, CEO of STFC, an *ad-hoc* group of senior astronomers met with him and Richard Wade for informal talks on January 9th. The group consisted of Martin Rees (IoA), George Efstathiou (IoA), Roger Davies (Oxford), Carlos Frenk (Durham), Andy Lawrence (Edinburgh), Jim Hough (Hertfordshire), Mike Cruise (Birmingham), Martin Barstow (Leicester), and Michael Rowan-Robinson (Imperial).

The *ad-hoc* group recognizes that the current funding situation represents a crisis for UK astronomy greater than any we have previously experienced. Keith Mason argued that the crisis arises from a combination of two factors: (*i*) international subscriptions (ESA, ESO) now represent a larger fraction ($\sim$ 60%) of our programme than in the past; (*ii*) the STFC CSR settlement, along with those for the other research councils apart from the MRC, was flat cash for 3 years, which implies cuts to the total budget in 2008–09, 2009–10, 2010–11 of, respectively, 2·5%, 5%, and 7·5%.

Once some allowance is made for new projects starting up in the years ahead, we face by 2010–11 cuts of 25% in astronomical facilities, projects, and grants. The *ad-hoc* group remained dismayed by a cut of this size and on such a short timescale.

The group discussed what can be done to try to persuade the DIUS that this represents a very serious situation for our science, for physics departments, and for the UK's international reputation. The Innovation, Universities and Skills Select Committee hearing on January 21st offers an opportunity for the RAS and the IoP to speak on behalf of the astronomy and particle-physics communities. On a slightly longer timescale, the Wakeham review of the health of physics is an important opportunity and it is crucial that departments and universities make the strongest possible representations and that the case for astronomy is specifically put. Although the funding crisis appears to be hitting particle physics and astronomy hardest, the repercussions are likely to spread to the whole of physics as the income of physics departments is reduced, staff morale plummets, and students are deterred from studying a subject perceived to be having support withdrawn from it. *Recommendation 1: All astronomy groups are urged to make sure that their Vice-Chancellor submits a strong case for astronomy and space science to the Wakeham review.*

The group was concerned about the very short time-scale on which the STFC Delivery Plan has to be assembled in response the CSR announcement, driven by the start date of the CSR period on 2008 April 1. There is a real concern that the damage caused by cuts in the Delivery Plan would be exacerbated by the haste of the decisions.

The group felt that it was wrong that international subscriptions, which are essential for playing a major rôle in modern astrophysics and space science, are treated as if they were an optional part of the programme. Fluctuations in subscriptions, driven by changes in GDP and currency fluctuations, should not fall on the STFC programme. *Recommendation 2: DIUS should remove international subscriptions from the STFC budget line and should handle fluctuations (which may be up or down) separately.*

STFC sees its main national rôle in terms of economic impact, providing 'the economy of tomorrow'. We face a tough challenge to make the case for fundamental science within this mission. We have to make the most of the knowledge-transfer opportunities that our science offers. We have to foster and emphasize the rôle of astronomy and space science in drawing schoolchildren into science and university applicants into physics, as well as the enormous economic value of our students, postgrads, and postdoctoral researchers. *Recommendation 3: STFC needs to publicize the intrinsic value and indirect economic benefits of fundamental science and strengthen this element of its mission.*

The *ad-hoc* group had a vigorous discussion with Keith Mason and Richard Wade on the issue of consultation and had previously taken advice from the chairs of the Science Board and PPAN. We accept that these bodies have been fully involved in the preparation of the Delivery Plan and in the on-going Programmatic Review. However, the absence of any advisory structure below PPAN meant that the community felt no involvement in or ownership of the programme. *Recommendation 4: PPAN should without delay set up an advisory structure below it, so that a wider cross-section of community experts can be involved in discussions of the programme*.

Finally, we have to look ahead to the next Comprehensive Spending Review, submissions for which will begin in 18 months' time. We have to define the programme and the arguments which will enable STFC to be the research council that secures the best settlement next time around."]

The President. I think at this point we should move to discussion. I prefer not to have very long speeches, but on the other hand I would like to go over as much of this as possible.

Dr. R. J. Barber. It seems that we're caught with these international subscriptions: looking at today's *Financial Times*, it would seem that the pound devalued against the Euro by over 10% over the last 18 months. Now, if what was 60% of the total historically has now to be paid out in stronger currencies, it is going to be 66%, so if you rework the calculations the cut is even worse than 25%. As a group in a situation where we are held to ransom by exchange-rate movements, we should have, with hindsight, negotiated a deal that gave us some protection against currency movements.

The President. I can just reply on that: I went to see Sir Keith O'Nions (Director-General of the Research Councils) perhaps a year ago, with regard to STFC, which was then being set up, and I did raise this issue of protection from currency fluctuations and he said that STFC would continue to be protected against currency fluctuations as PPARC had been in the past. Now, what they actually announced is that there will be protection for fluctuations in excess of £6M. So there is *some* protection against a very big change.

Professor Dame Carole Jordan. Can I just add something? I was on the SERC when it did not have currency-fluctuation protection. I think it must have been Bill Mitchell who finally negotiated to have this picked up by the DTI. The first thing I thought when this news came out was, who has lost us this protection? It is impossible to operate a science research council of any sort with those currency fluctuations. I think you should emphasize that in the past it was only possible to operate a sensible budget *because* we have not had these large fluctuations.

Dr. G. Q. G. Stanley. You mentioned that Keith Mason wanted us to focus much more on the next CSR, in 18 months. My concern is that momentum will build up where, in 18 months' time, we may find ourselves cut back even more, because they will say that the bigger sciences will have had much more more investment and then will be taking more of the pot.

The President. I think what Keith Mason is saying is that if we don't campaign really hard, it will be even worse next time.

Professor M. G. Edmunds. On the issue of consultation, I think there was great difficulty because of the sensitivity of issues involved, not only within the astronomy section of the Council, but particularly in the former-CCLRC section, because of redundancies, *etc.*, with big implications. I think it is unfortunate that there was no better mechanism for consultation, and I absolutely agree with you, this is something that we must sort out in the future; it is sad that things happened in the way they have.

Can I just revert to the issue of international subscriptions, because I think it is one that we need to debate. I believe — and this is just a belief, not knowledge, from a conversation I heard about a year ago — that it is deliberate policy not to provide exchange-rate protection; whether on behalf of the Treasury or the Government or the Director of the Research Council, I don't know. It was felt that the international subscriptions were out of control, to a degree, and that as they grew and became a larger proportion of science budgets in general, not just for our research council, it was important that people made a choice as to how they spent their money — whether they spent it on international subscriptions or not — so that the value for money from the international subscriptions should bear some relation to the science done. It wasn't something that could be negotiated, there was no way the research councils could hold on to the exchange-rate protection; we were told we were not allowed to, and I think it's a deliberate policy because the Government wanted to force a choice. Now, if that is so — and I have no reason to believe that it is not so — it implies that at some time we will have to think seriously about which international subscriptions we do wish to continue.

Professor Jordan. I think that is doing their dirty work for them, if I may say so. I remember when people were seriously talking about pulling out of the European Space Agency, just at the time when Roger Bonnet had got together for the first time a proper long-term plan. It's not a question of choice: these international bodies we join are the result of science activities by the community, both in the UK and internationally. We don't just go and build a facility; the experimental work is being done by the community. So, the choice is made when one decides to do the science; obviously one has to set a proper budget at that time, but it isn't right then to turn round and present it as if we didn't get involved. There is a huge gap in understanding, higher up in government, as to how science is actually done; a facility is part of the physics community.

The other thing I would say is, don't let people separate us from physics: we are 'physics', but it is *astro*physics, and we need to stress that much more. So, is the physics budget being cut? And if so, what possible rationale is there for cutting just one part, which they call astronomy? Solar-terrestrial physics is 'physics' — there is no question that is not — and it trains a very important kind of physicist, especially with regard to on-going fusion and plasma-research programmes. I think we should not let them push us off into a corner as if somehow we are not doing physics, and that should be a very strong element of any evidence that we give.

Professor S. J. Schwartz. I'd like to spend a couple of minutes talking about solar-terrestrial physics (STP), if I may. The Society has within it a large body of solar-terrestrial physicists and I am slightly disappointed I didn't recognize any among the list of names you took with you to see Keith.

The President. Well, we had Martin Barstow, who has one of the biggest groups in his department.

Professor Schwartz. Well, you didn't have the chair of UK Solar Physics, you didn't have the chair of MIST.

Let me go on and talk about STP if I may. The Delivery Plan calls for the closure of all ground-based STP facilities. This is not the closure of a telescope somewhere, this is the closure of an *entire* waveband, of an *entire* community. And it is quite clear that the intention is not just to close those facilities, but also to close any science that goes with it; I think we will see that played out as time progresses.

That is part of the research and of a large community which will certainly be damaged. I don't think anybody in the STP area, be they solar physicists or interplanetary physicists, or anyone else, should be very comfortable at all. It's a very unnerving situation. It is incredibly difficult to get clear statements out. There is a statement from Keith that says this followed on from what happened in the last Programmatic Review, when some ground-based facilities were closed down, but it has never been strategy. If you ask Keith what the current strategy is, he says the Road Map on what used to be the PPARC web page is still intact, but I don't see any evidence that anybody is taking any notice of that, and I don't see any evidence that there is a coherent plan to present a new strategy, and it worries me deeply.

Professor A. Smith. A question of clarification: you said that STFC is being sold to Government as 'economic impact'. Is it being sold to Government, or is Government demanding that it shall be?

The President. The Treasury is certainly demanding that all investment, especially all investment in science, demonstrates economic impact. It doesn't have to be a quick buck, it doesn't have to be an actual device that sells in the supermarket, in other words arguments about training, impacting schools, and so on, can count if they can be quantified. But, yes, we are in a régime where the Treasury expects to see a return on its investment. STFC, with its state-of-the-art facilities and frontier science, is presented as blue-skies science and technology leading to the economy of the future.

Professor Smith. It seems that it is not something we will be able to change in the short term, so we do have to play their game.

Professor J. C. Zarnecki. Going back to the currency exchange, the impression has been given that there has perhaps been a change in policy recently, which I don't think is the case. I was on PPARC council and I seem to remember that there was no protection then; perhaps there was beyond a fairly large fluctuation, but essentially, I don't think there has been a recent change. There were times, of course, when we benefitted from currency fluctuations, and obviously we kept quiet then [restrained laughter]; it has been a combination of the weakening pound and the increasing proportion of our GDP which has exacerbated the problem.

I would also like to make a comment on economic return, which I think has many, many facets: one we should bear in mind is that a very large proportion of our subscriptions to ESA, ESO, and CERN is supposed to come back to the country in the form of contracts to industry, and also to universities. PPARC council would review those figures, and the last that I remember is that for ESA, some

90% of the subscription, amazingly, does come back to the member states, and the UK performance is quite good. But for ESO and CERN we got probably only a third of what we should. I am not saying that that's the reason for these cuts, but I don't think it helps our case at all. With ESO I suppose there are reasons, in that we are still relatively new members, and there's a very long lead-time for a lot of the work; but I think that we could do better, and it would certainly help our case.

The President. OK, one last question.

Dr. Barber. What the Government has done in the past, given the currency movement and also given the fact that Sterling is falling substantially further behind the Euro, is to allow us to go back and negotiate another deal with them.

The President. We are trying to do that. Well, thank you very much for your comments.

I think we will now move on to our main programme. Our first speaker is Dr. Hugh Hudson, of the University of California, Berkeley, and he will be speaking on 'Microflares now, major flares soon.'

Dr. H. Hudson. The subject of my talk today is the return of solar sunspot activity in its build-up to the maximum expected in about 2011, and the expectation of substantial progress on important physical problems with the observing facilities now available and in development. Only a week before this meeting the first high-latitude active region of the new cycle appeared, although as expected it did not last long nor produce many flare events. But Maunder's 'butterfly diagram' flaps again!

The 22-year Hale magnetic cycle, with its two sunspot maxima, regularly (but hardly predictably) produces major solar flares and coronal mass ejections (CMEs). I would compare this with a somewhat erratic plasma machine in a basement laboratory of some university — each 'test pulse' of the machine perturbs the plasma of the solar corona. The biggest pulses actually disrupt some of the coronal structure, and affect the solar wind. The coronal plasma is a roughly concentric low-beta volume of space; in the mean the ratio of gas to magnetic pressure is of order 0·01. In the low corona above an active region, beta can be much smaller still, and the Alfvén velocity can exceed $0 \cdot 1c$. A CME seems mainly to disrupt only a specific narrow sector of the coronal volume.

What can we expect to learn from the events to come, and what are the outstanding problems that need attention? Beginning with the problems, it must be recognized that we still have no predictive theory for flare occurrence, even though the conditions that lead up to a flare are totally transparent to our observations — the corona is optically thin to its natural EUV and soft X-radiations. The result of flare instability is extremely counter-intuitive, since the CME that results appears to expand the coronal magnetic field in a sector (about one steradian) of the coronal field to create new open field lines in the solar wind. At face value this process is endoergic, since the solar-wind field is radial and thus highly non-potential in nature, and so it is surprising that this opening of the field can happen as the result of an instability.

Non-thermal particle acceleration also remains ill-understood. The energy of a solar flare appears to be dominated initially by non-thermal electrons. This is not what would necessarily be expected if magnetic reconnection, as is widely supposed, 'explained' the flare process. We also now recognize that the collisionless shock waves driven by CMEs, analogous to supernova shocks, typically convert a large fraction of the total energy involved into other types of particle acceleration. None of this is very well understood, partly because our plasma laboratory (the corona) is so remote, and we must rely mainly upon the astronomer's remote-sensing tools.

We are nicely poised now with facilities to take advantage of the new major activity. There are space-borne observations with *RHESSI* (hard-X-ray and gamma-ray imaging spectroscopy). *Hinode*, a Japanese mission, carries the first large solar optical telescope in space (0·5-m aperture), plus an advanced soft-X-ray telescope. Also on *Hinode*, the remarkable UK instrument *EIS* does high-resolution imaging spectroscopy in the EUV. Finally there is *STEREO*, something quite revolutionary in that its two satellites introduce stereoscopy (a true third dimension) to astrophysics for the first time. In addition to the space observatories, ground-based solar observatories are making great progress *via* adaptive optics and continue to make important contributions. Recent observations have achieved angular resolution sufficient to delineate the rough structure of the photosphere, for example, the faculae that show the distortions due to magnetic flux tubes.

More fully understanding the 3D geometry of the coronal structures gives hope for solutions to the problems mentioned above — the fundamental process of a flare and its strong propensity for particle acceleration. Beyond this, a laboratory plasma physicist would want to have measurements of particle-distribution functions. In the MHD approximation to the solar corona, for example, one normally assumes a single fluid, equating the electron and ion temperatures and ignoring all of the interesting properties of the velocity-distribution functions. The *SOHO* data of the past solar cycle have shown these to be of the greatest importance even for considerations of the physical properties of the quiet Sun — coronal heating, the topic of this morning's RAS Discussion Meeting. The new *RHESSI* data now show conclusively that flare activity, including 'microflares', is a distinctly different process from general coronal heating.

I would like to close by pointing out the growing solar use of concepts originally derived to explain phenomena in the Earth's ionosphere and magnetosphere. These become more applicable to the solar corona as our observational knowledge grows. There is a parallel with the Rosetta Stone, then, in which the solar corona can act as a means of translating the *in-situ* physical knowledge of plasmas in the Earth's environment, into domains that can only be observed by remote-sensing tools.

The President. Well, thank you very much, and thank you for keeping to time so well. Questions?

Professor Jordan. You presented a cartoon illustrating a twisted magnetic flux tube in the corona. In this cartoon are you saying that the thing unwinds from the top downwards?

Dr. Hudson. Yes, exactly.

Professor Jordan. I can remember a cartoon by Parker where he always had the most tightly wound part at the top.

Dr. Hudson. Well, that's right. I shouldn't say it unwinds. I think what happens is that you launch Alfvén-mode waves and they can go in both directions. There is no change in the nett twist. The twist in the field reflects the current that's flowing through the structure. This current can't be interrupted suddenly: it has persistence due to its inductance. So you can't just switch the current on and off. If you see a cartoon that suggests that the twist suddenly changes spatially you are seeing something which is not consistent with a steady state. So this cartoon, drawn by Lyndsay Fletcher, suggests the opposite flow of waves from the Poynting flux that you may use to explain coronal heating, where the field stresses accumulate slowly in the corona.

Professor Jordan. I just thought that Parker had shown that the twist ran up rather than down.

Dr. Hudson. Parker, and I say this judiciously, might disagree upon how this flare model works. He often frames his analysis only in terms of the magnetic field, but we are also trying to understand the current injection into the corona.

Professor D. Lynden-Bell. How much of the current is in the form of fast particles?

Dr. Hudson. That's an extremely interesting question. The general idea about the coronal plasma is that it has a thermalized core distribution function, plus perhaps a non-thermal tail. You can look in the solar wind and see that sort of distribution function. It seems in flares that the energy release may be so intense that in fact the pressure may be dominated by the non-thermal tail. Thus it's a very unusual plasma. We don't have the right diagnostics so we are just beginning to think about that. There's almost no theoretical work done on this as people normally deal with Maxwellian distributions, and so this is just a hint about what may be happening. But the particle acceleration is extremely intense. The entire energy of the flare is carried in these 20 kilovolt electrons and there is a comparable amount of energy in MeV protons and high-Z particles, and so the flare, this impulsive rapid variation, is exceptionally good at making non-thermal particles and accelerating cosmic-ray-type particles.

Professor Lynden-Bell. That is actually at the reconnection line, is it?

Dr. Hudson. No, I don't believe in that — the reconnection line is very small and so we need a lot of particles. Even in the reconnection picture it's not the reconnection line itself that is important. The actual energy conversion is a set of shock waves that radiate away from the reconnection line and it involves a larger volume. I think that acceleration at the reconnection line directly is a non-starter for this kind of problem.

The President. Thank you very much. [Applause.] Our next speaker is Professor Alan Smith of the Mullard Space Science Laboratory, and he's going to speak on '*MoonLITE*: The UK-led penetrator mission to the Moon'.

Professor Smith. [No summary was received at the time of going to press. *MoonLite* is a proposed UK-led mission, part of a joint programme with NASA, involving a consortium of eight institutional and three industrial partners.

The speaker began by summarizing the concept and advantages of penetrators as an alternative to soft-landing unmanned spacecraft for exploration of a planetary surface. A penetrator is an instrumented package, perhaps about 10 kg or so, which arrives at the surface at about 300 m s^{-1}, penetrates the surface to a depth of 2–3 m, and makes scientific measurements, transmitting the results back to Earth. It has to be designed to be rugged, the payload needing to be able to withstand forces of about 10 000 g. The advantages of penetrators over soft-landers are that they can be cost effective, multiple penetrators can be deployed in a single mission, they can be used to access target areas not easily accessible to soft landers, provide ground-truth data for remote sensing, and provide immediate access to samples below the surface without the need for drilling. Among the disadvantages are that relatively few technologies can be built to survive the impact, and they have a limited lifetime. They can be used as precursors to other missions, and used to establish seismic networks. Their main objective is the science, but they address the needs of exploration too.

There are plans to use this type of technology across the Solar System, and penetrators form part of several proposals under ESA's Cosmic Vision programme, such as *LunarEx* (another lunar-penetrator mission), *in-situ* exploration of two Saturn moons, Titan and Enceladus (*TandEM*), and a mission to Europa. The exploration of near-Earth asteroids would be another possibility. Experience of the use of penetrators in planetary missions is presently very limited; although several have been planned and built before now, they have not thus far been successfully deployed. The greatest experience of the type of technology comes from the defence sector, where there is much experience in the design and testing of smart bombs which are able to penetrate fortified defences in a controlled way.

The speaker summarized the development of the *MoonLITE* mission since the formation of a consortium in 2006 to explore the idea of a lunar-penetrator mission. By 2007 July, NASA and the BNSC had set up a joint working group to examine UK involvement in a mission in the context of lunar exploration, and recommended a Phase-A study for the UK-led *MoonLITE* mission.

MoonLITE would comprise a spacecraft in polar lunar orbit and the deployment of four penetrators. The orbiter would act as a communications link for the penetrators, which would be strategically important for NASA since this would support their aims for further manned exploration of the Moon.

The science questions for such a mission are associated with the origin and evolution of the Moon and the Earth–Moon system, to determine whether water is concentrated in shaded polar craters, and the implications of such a discovery for other areas of science, including astrobiology. The penetrators will also take seismological measurements and measure heat-flow to shed light on questions about the Moon's core and mantle, and examine local lunar geochemistry and mineralogy.

The structure of the penetrator includes bays for communications equipment and the science payloads; the impact modelling and stress analysis for those systems is well developed. The payload instruments will need to be made rugged to withstand the impact, and the unit will be powered by batteries with a lifetime of one year. The penetrator will contain a drill to sample material, and other units such as radioactive heaters. Firing trials with a stripped-down instrument package will take place in 2008 March, and the current timeline would see the mission launched in 2013.]

The President. We've got time for a few questions. If I've understood this rightly, you find the areas on the Moon that have water, and then you're going to bomb them? [Laughter.]

Professor Smith. Well, I'm trying to avoid terms like 'bomb' and 'crash'; we're going to sample them.

Dr. Stanley. How much of the technologies, as a percentage, do you feel you could transfer from, say, defence into this mission?

Professor Smith. The main defence technologies provide us with the actual penetrator structure. The electronics isn't really an issue, it's more to do with how you pack electronics; what the defence sector provides is know-how on how you would assemble something to survive that sort of impact. We'll get advice on how to pack the scientific instruments, but we are going to have to develop the instruments ourselves.

Professor Edmunds. Can I ask two questions? One is, doesn't the impact spoil your sample to some extent? And what do you really believe you'll learn about the Moon that is so fundamental and which you don't already know?

Professor Smith. To answer the first question, the penetrator doesn't heat the Moon up very much as it comes in, so we believe that only a centimetre or so of the surface will be chemically changed. That's why we need to drill out and sample a few centimetres away, and that's pretty well understood from the models. On what we will learn, we're addressing questions such as, is there water ice in the surface? Is there a core to the Moon? And if there is a core to the Moon, we'd like to determine its properties, because you can do that seismically. The heat-flow measurements should tell us something about whether the mantle is uniform or not, and things like the thickness of the mantle on the far side, because we don't know anything about that. We don't know very much about the Moon's properties other than in the areas where the Apollo missions landed. So the questions address how the Moon formed, and the environment of the polar craters. The US is very interested in that of course, because that's where they want to put manned bases. They want to understand the environment of the poles, and the seismic environment of the poles, which no one knows anything about.

The President. Wouldn't you want to sample isotopic ratios and so on?

Professor Smith. We do, and what we're able to do depends on how far we take this technology forward. Our colleagues at the OU have been looking at what exactly we can measure. It's not exactly clear how sensitive we will be to isotopic ratios; that's quite a challenging thing to do. The mission doesn't replace the need to go to do things later on, but it's a stepping stone to bigger things.

Professor M. E. Bailey. I wonder if you could give us the web address of this extra source of funding you referred to, the Cosmic Spending Review? [Laughter.]

Professor Smith. Of course, this work all started before the disaster that has happened with STFC. We're not going to pull the plug on this mission just because life's tough. We're just going to keep playing this game and see what happens. But we're not in a great competition — we're not talking about a large amount of money. I believe this kind of mission will help with the case for the next CSR submission, because it's the sort of application the Government wants to see.

The President. I think it's a good question, and the answer raises some serious points, that we mustn't get into a defensive mentality about everything. We do have to look to the future, and new things have to compete with what we're doing now. Any other comments?

Professor P. G. Murdin. Just one other dimension to this, and it harps back to the very first comment. While Alan was talking, I was feeling first of all a fascination with the technology and the science, but also a slightly emotional rejection of this as an idea. I was imagining sitting in front of John Humphreys on the *Today* programme and getting a lot of attack from people who felt that this was an emotionally nasty thing to do — there's the sense of something being destroyed that was unsullied before, the idea of interference by us with another cosmic body. There's a dimension to this, perhaps, that one ought to prepare for.

Professor Smith. One of the things we're very interested in is the concept of these as time capsules for the future, that will last a hundred million years under the surface, that mankind will leave something behind. I think we can counteract a negative response with something like that. The other thing is building on the experience of the defence industry: the industry which supplies our armed forces might as well do something positive for us. I think there is a connection, but it has to be carefully managed, I agree.

The President. Thank you very much indeed. [Applause.] Our final talk is by Professor Mark Burchell, and he's going to speak about 'Impacts: drivers of change in the Solar System.'

Professor M. J. Burchell. Today's specialist discussion meeting was on the topic of impacts and how they drive change in the Solar System. In my talk here I aim to provide an overview of this topic, giving some specific examples from those who spoke earlier today.

Today, most people know what a giant impact is: a large body comes from space and hits the Earth at very high speed. The result is a big crater. However, 40 or so years ago this was still controversial. Since then, our knowledge of shock impacts has increased enormously, driven by field work by geophysicists, impact experiments in laboratories, and computational modelling. In addition, space missions to other Solar System bodies have shown that their surfaces are also scarred with impact craters.

In consequence, the field of impact studies has grown enormously. It ranges from identifying craters here on Earth, all the way to considering how they have influenced the development of life. Identifying craters is not easy: all large, circular, depression-like structures in rock are not craters. So simply finding such a feature (which can be hard — it is curiously hard to see something which might be 50 km across, or which may be buried under subsequently deposited sediments) is not sufficient. On Earth it is also necessary to identify a range of markers for shock compression in the rocks that make up the structure. Until these markers for shock metamorphism are located, a structure remains just that, a structure of some sort, origin unknown. It should not just be accepted as an impact crater. On other Solar System bodies we are more relaxed — unless we have returned samples, such as from the Moon, we are more willing to accept that the large features we see have an impact origin.

Here on Earth, impact craters have enormous value, literally so. Dr. Richard Grieve of Canada pointed out today that impact structures are good places to mine rocks and ores. The economic value of materials extracted from large-impact-crater sites such as Sudbury (Canada) or Vredefort (South Africa) is measured in billions of dollars. In addition, the deformation and damage to surrounding underground layers may make them sites ideal for trapping oil and gas, again with billions of dollars worth of economic utility (*e.g.*, the Chixculub impact in Mexico).

But not all impacts are subaerial, *i.e.*, occur onto rock. Today, the Earth's surface is $^2/_3$ water so submarine impacts also occur. Impacts into water produce different results to those on rock. A crater-like cavity is formed in the water, which in relatively shallow waters may reach to the basement rock in which a smaller crater may also form. In deeper water the full crater cavity is in the water. Cavities in water are not supportable without a driver, so when the impact event is over the water flows back in again. This can raise a column of water in the centre of the impact zone, which in turn subsequently collapses. This collapse leads to tsunami generation (and there may be further oscillatory cycles). Unless this occurs in relatively shallow water there may be few markers left at the impact site, except for deposition of broken up extra-terrestrial material on the ocean floor such as is found at the Eltanin site in the Bellinghausen Sea. Craters on ocean floors as a result of impacts in shallow waters are known. The Moljnir crater, north of Norway, is one such example. Research into the effects of impacts onto water has recently been underway at UCL (modelling by Emily Baldwin) and the University of Kent (experiments by Dr. Daniel Milner). This combination of laboratory-scale experimentation with modelling, which can simulate any size scale, is a particularly powerful tool for studying impacts where the scales of real events exceed those available in laboratory.

In the Solar System, water and rock are not the only materials which occur on the surfaces of bodies: ice is also widespread in the outer Solar System. Studying impacts on ice is a growing field, both with examination of images of real craters on the Galilean satellites (Veronica Bray at Imperial) and *via* impacts on ice targets in the laboratory (Andrew Lightwing at Kent). Indeed, the laboratory work at Kent focusses on a different outcome of impacts: rather than an impact crater, the target body may be completely shattered. Whilst this is an unlikely outcome for a large Solar System body, it is not implausible for smaller ones.

As well as the physical consequences of impacts in terms of cratering and disruption, there are also astrobiological implications. An impact may even kill: we all know what we think happened to the dinosaurs. But after an impact, the site, once it has cooled somewhat, is now an inviting habitat for microbial colonization, as considered by Charles Cockell of the Open University. Shattered rocks and perhaps flow of water through them, *etc.*, offer homes to a variety of hardy extremophiles. Indeed, on planets like Mars the heating effect at an impact site may leave it more habitable than its surroundings for several thousand years. As potential niche environments for life, impact craters may thus represent good targets for missions to Mars which aim to search for evidence of life. At the other extreme is extinction: we may have heard popular accounts that a stable Solar System like ours needs a Jupiter-sized planet to protect the life-bearing 'Earth'. It does this by shielding us from giant impacts, taking the hits itself, as it were, on our behalf. At the Open University, Barrie Jones and Jonty Horner are investigating this *via* modelling. The results indicate that it is a lot more complicated than that. Larger planets can also deflect small bodies onto new orbits which may collide with ours and so increase the impact hazard. Thus the true rôle of a Jupiter to either protect or endanger life on Earth is a function of its size and location.

This is only a flavour of the work reported earlier today. And beyond that there are many more, ever increasing ways in which 'impacts' are cropping up in research today here in the United Kingdom. In some cases it is the impacts themselves that are studied, in others it is their consequences. With work from over ten universities featured in today's meeting, we can safely say that there is a widespread UK community, productively active in this field. It involves geologists, shock physicists, experimentalists, and modellers, many of whom have extensive international collaborations. I am sure we will see many more impact meetings here in the UK.

The President. Thank you for that very interesting summary of this morning's meeting and the whole field. We don't really have time for questions now, but I'd just like to comment that you've made a very good case for not throwing objects at the Moon and increasing its mass!

Before we break up I'd like to say that the next meeting will be on Friday, February 8. If you weren't at the December meeting we now have our apartments at Burlington House open again and you really should go and see them. There will be a reception in the library now. The bad news is that the contribution for the costs of the reception has had to be increased: it is now £3; that is what it costs us to lay on the drinks and so on. Anyway, I very much hope to see you over there in the wonderful, new, refurbished RAS apartments and RAS library.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2008 February 8th at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

M. E. Bailey, *Vice-President*

The President. Welcome to the meeting, and may I remind you to turn off your mobile phones on pain of instant ejection! On behalf of the RAS, I would like to congratulate Professor Richard Ellis on being awarded the CBE for services to science. Richard was Director of the Institute of Astronomy at Cambridge for quite a while and he is now Professor of Astronomy at the California Institute of Technology but he is due to return to the UK later this year to Oxford, so let's congratulate him. [Applause.] And now to get on with our programme; our first speaker is Professor Andrew Roberts, of the National Oceanographic Centre in Southampton, and he is going to talk about 'Geophysics of global climate change'.

Professor A. Roberts. [No summary was received at the time of going to press. The speaker reported highlights of a two-day 'G' meeting which had just taken place on the geophysics of climate change. It was reported that normally these meetings are attended by solid-earth geophysicists who use geophysical techniques to probe the deep structure of the Earth. These techniques can also be used to access a wide range of environmental problems, and can be applied to look at global climate change.

There was a range of overview talks which discussed Earth's climate history. In the Paleocene/Eocene era, 70 million years ago, the Earth's climate was very warm and the Earth was a greenhouse planet; at the boundary of the Eocene/Oligocene eras, 34 million years ago, the Earth underwent a transition to what is known as an icehouse world. A major ice sheet appeared on East Antarctica and since then has waxed and waned through a range of mechanisms with time, whilst the planet has progressively cooled, leading to the ice ages of the last few hundred thousand years. We are currently in an interglacial period within a longer phase of colder climates. Examination of the concentration of greenhouse gases (methane, CO_2, *etc.*) in air bubbles from Antarctic ice cores indicates that the climate shows a saw-tooth variation in temperature over the last 400 000 years, with temperatures varying between glacial and interglacial. Some of the speakers at the meeting discussed how the natural climate system is forced: the variations in the Earth's orbital parameters (obliquity and eccentricity), producing variations in the amount of incident solar radiation on the Earth's surface, are periodic and predictable. By calculating the variations in these orbital parameters over time, they can be calibrated against reversals of the Earth's magnetic field in the geological record, providing a (relatively) precise clock for dating sediments and climatic variations.

One of the invited speakers at the meeting was the Director of the NASA Goddard Institute for Space Sciences in New York, Jim Hansen, whom the speaker noted has been rather a lone voice in the US for decades with his views on global warming. The chief instigator of historical climate change has been changes in the Earth's orbit, but that is a very weak forcing, and Hansen argues that the reasons for the Pleistocene global climate change are greenhouse gases, ice-sheet area, and associated feedbacks from these. On long timescales, the climate is sensitive to orbital forcings, but human forcings now dwarf the natural forcings that caused the fairly large-scale glacial/interglacial cycles; Hansen's contention is that we now control the mechanisms for global climate change, for better or for worse.

By examining the atmospheric greenhouse-gas concentrations from ice-cores, CO_2 is currently found to be about 380 parts per million by volume, whilst it has varied naturally over the last several hundred thousand years between 200 and 280 parts per million; so the atmospheric concentration is now at a much higher level. Proxy estimates of CO_2 going back even further indicate that in the next century, greenhouse-gas concentrations will be far in excess of what they have been over the last 40–50 million years, and we will enter a phase of unprecedented experimentation with the Earth's atmospheric composition. One can examine various predictions from the Intergovernmental Panel for Climate Change and apply a range of metrics to assess what the dangers of climate change will be, from ice-sheet disintegration and global sea-level rise, to extermination of animal and plant species, to regional climate disruptions. Jim Hansen is pushing hard the argument that we should ramp back the production of atmospheric CO_2 to avoid irreversible damage to the Earth system, for example, through carbon capture and storage and the rapid phasing out of coal burning; and that if we do this immediately we can revert to a steady state of, say, 350 ppm of CO_2, and avoid problems with ocean acidification and damage to coral reefs. To achieve this, there may be a need for incentives such as raising the price of carbon, as well as increasing carbon sequestration in the soil and biosphere, and reducing other gases which are contributing to the greenhouse problem. It was reported that the meeting heard from speakers from industry who addressed questions of carbon capture, transport, and storage.

A series of talks were given on sea-level changes. One estimate suggests that the rate of rise in sea level has accelerated over the last 200 years, and it can be expected that the sea level will rise another 35 cm over the next 80 years — but these estimates ignore any change in temperature. A wide range of proxy techniques are being used to reconstruct sea-level variation going back over several hundred thousand years. The sea-level at the time of the last glacial maximum was 120 metres below where it is today; from proxy evidence, it is known that the temperature difference since that time is 4 degrees. This enables one to relate temperature change to sea-level change and to estimate an upper bound on the change over the next century of about three metres (which is rather large), if melting continues to increase.

Vincent Courtillot of Paris presented global temperature records, and argued that there is considerable variability in temperature over the last century or so, and that greenhouse gases are not the principal cause of increased temperatures; rather, it is down to solar variability. Despite this there are still issues about what we should be doing about trace gases in the atmosphere. The effects of ocean acidification and the subsequent damage to coral reefs are sufficient reasons in themselves to be cautious about emissions.]

The President. Questions?

A Fellow. Can I ask if there was any discussion of the eventual impact that variations in the output of the Sun intrinsically could have on the Earth's climate?

Professor Roberts. Not directly, although Vincent touched on it; do you wish to comment, Vincent?

Dr. V. Courtillot. It is generally said that the changes in total energy coming from the Sun are too small, as you all know I'm sure, to compare with the forcing of CO_2, being only one per mil [one per one thousand] approximately of the total flux. The question is, is the forcing linear? As you probably know, at UV and EUV wavelengths the relative changes in output of the Sun are far larger than the one per mil of the total irradiance. So the question is, is there a mechanism of feedback — clearly it has not yet been discovered — that will increase some amount of the signal to the level that can explain this correlation between solar variation and temperature?

Mr H. Regnart. I was recently discussing related issues with a research-scientist friend, who was telling me about something called *Terra Prata*, 'Black Soil', and they had the interesting comment to make that research suggests that this form of soil, which has been found in Central America and which effectively regenerates itself, involves a high content of charcoal and is actually highly effective at fixing and capturing CO_2. Even when trees and other plants are cut and return their carbon dioxide, including from their roots, to the atmosphere, there is a 40% fixation rate, which is effectively permanent. So, maybe this is something that those in the field should be looking at with greater interest for carbon capture.

Professor Roberts. Yes, that is a very good and interesting point; Jim Hansen raised this as one of the ways of changing agricultural practice, for example. There are huge agricultural subsidies in most countries of the world, and one can consider giving farmers the incentive to change their practice to sequestrate carbon through a range of agricultural practices. A particular one that he was arguing for was using a thing called 'black char', which is rather similar, which takes up carbon rather well, so I think there's a fair bit of merit in arguing that.

Mr. R. Steppe. Did the people who were talking about CO_2 capture in power plants and so forth discuss in their presentations the amount of energy that would be required to accomplish this recapture?

Professor Roberts. Some questions were asked about this, and I have the presentation somewhere. It costs money and it costs energy, but in relative terms it will cost less than setting up a wind farm, and there are plenty of people out there doing that, so I think it is a credible way of moving forward.

A Fellow. We hear lots of terrifying things that are going to happen with global climate change, and you pointed to a few of them just now. Does anybody ever think about what would happen to our society and our world if there was no climate change predicted?

The President. You mean if the natural cycle continued as usual?

Professor Roberts. The planet has seen a big change over time: plus or minus four or five degrees on hundred-thousand-year cycles and humans have been around for the last thirty-odd thousand years, so humans have seen considerably colder periods and survived them; we could no doubt survive warmer ones.

The Fellow. I mean, in all of this we really are worried about humans, and not much else?

Professor Roberts. Right, but humans are clever, so if you find problems, you find solutions.

The President. Derek?

Dr. D. McNally. I am worried about the effect on humans, on astronomers. Are we likely to see as a result of the increased temperatures deterioration of observing conditions, seeing, and so on …? [Laughter.]

Professor Roberts. I am not going to try and answer that! [Laughter.]

The President. That is a bit of a narrow point of view! Well, thank you very much; that was a very interesting summary. Our next speaker is Professor Eva Grebel from Heidelberg, and she is here to talk about 'A comparative study of the dwarf companions of the Milky Way and M31'.

Professor Eva Grebel. In 1994, van den Bergh compiled the star-formation histories of the Galactic companion galaxies with luminosities fainter than -14 in absolute *V*-band magnitude. He pointed out that the fraction of intermediate-age populations in these gas-deficient dwarf spheroidal galaxies (dSphs) increases with increasing Galactocentric distance. In other words, he tied dSph properties to their environments. In subsequent years, as more detailed data on star-formation histories became available thanks to deep CCD photometry and imaging studies with the *Hubble Space Telescope* (*HST*), this trend was confirmed. On the other hand, there are also a number of problems with a purely environment-driven scenario.

Assuming that the present-day locations are representative of the dSphs' mean distances over cosmological time scales, van den Bergh proposed several processes as possible explanations of the observed trend. These include tidal stripping by the Milky Way, ram-pressure stripping by the gaseous Galactic halo, and photo-evaporation by the increased UV flux during major Galactic star-formation events (*e.g.*, bulge formation). These processes would affect close companions most severely, while the more distant dwarfs would be able to hold on to their star-forming material much longer and hence experience much more extended episodes of star formation.

If the apparent impact of environment on the evolution of low-mass galaxies as implied by the 'van den Bergh relation' is a generally valid mechanism governing dSph evolution, then we would expect to find a similar trend among the dSph companions of M31. M31's satellites span a comparable range of distances from their host as the Galactic dSphs. In recent years, the census of M31's low-luminosity companions was considerably expanded, more than doubling their number. The M31 and the Milky Way satellite systems also differ in a number of respects; for instance, M31 does not have massive irregular companions, while the Galaxy lacks massive dwarf ellipticals.

While we still do not have similarly deep and comprehensive studies of the M31 dSphs, we can combine all the available information (*e.g.*, deep *HST* colour–magnitude diagrams, presence/absence of a young main sequence, of a red clump and vertical red clump, or of luminous AGB and carbon stars) to constrain at least roughly their intermediate-age stellar-population fraction. For many of the recently discovered dSphs around M31 the distances are still quite uncertain, and our knowledge of their stellar populations is still poor. Nonetheless, the available data permit us to exclude the presence of significant intermediate-age populations of stars in these dSphs. Moreover, we do not find a population–age trend with distance from M31. Did M31's dSphs evolve differently, or is this due to M31's higher mass and larger size?

In the past few years, the census of Galactic dSphs has more than doubled. As for the new M31 companions, the information for many of the new Galactic companions is still scant, but owing to their proximity the existing colour–magnitude diagrams reach reasonably deep, facilitating population analyses. Interestingly, when including the new discoveries (all fainter than absolute *V*-band magnitudes of -9), the 'van den Bergh relation' vanishes. In particular, it seems that these faint new dwarfs did not experience any major episodes of more recent star formation. Their gravitational potential may be too low to retain the gas over sufficiently long time scales. This could suggest that for galaxies with inferred total masses of less than 10 million solar masses the initial baryon content (*i.e.*, intrinsic properties) is just as important as environment in determining their evolution. Some of the remaining major unknowns are the orbits of the dSphs, their true total masses, and the shape and extent of their dark-matter halos.

In conclusion, we can list several factors favouring environment as a prominent factor: the morphology–density relation, the morphology–distance relation, the difference in the scale radii of M31 *vs.* Milky Way dSphs, and the hints of on-going interactions in some dSphs.

Features favouring intrinsic properties as a prominent factor include the lack of intermediate-age populations in dSphs fainter than absolute magnitudes of -9, and the absence of a well-defined van den Bergh relation. As better data accumulate on the properties of the dSphs in galaxy groups, we can look forward to understanding the properties of the dSph satellites.

The President. Thank you very much. Questions?

Mr. Regnart. Does your work have implications, and if so, what, on the ratio of dark energy to dark matter to light matter, as you re-assess the amount of light matter in dwarf galaxies?

Professor Grebel. We cannot draw any conclusions about dark energy from this work. With respect to dark matter, there are some other arguments that seem to indicate that the dark-matter content of these low-mass/low-luminosity objects is very high. For instance, the velocity dispersions indicate that the dark-matter content must be pretty high; they seem in fact to be the most dark-matter-dominated objects that we know. So that actually refers to these inferences of total mass of $10^7 M_\odot$, which is much more than the observed baryonic luminosities can allow — the baryonic luminosities would indicate baryonic masses of 10^6 or 10^5 $M_\odot$. Then there are some other indications from the amount of apparent tidal destruction for the closest companions of the Milky Way — some of them actually seem to be disrupted, or seem to experience tidal forces from the Milky Way that distorted the outer boundaries, others are perfectly elliptical or spherical, not showing much of this kind of disruption. But I should point out that we do not know the orbits, so it is very difficult to trace or to fix that in any conclusive way.

The Vice-President. In addition to the mechanisms you mentioned for removal of gas, have studies been made of whether there is a critical mass above which the dwarf galaxy can retain its gas in the presence of, say, a mini galactic wind, driven by the early star formation?

Professor Grebel. Yes, there are actually several studies of that kind, simulations of the effects of supernovae going off in these systems. There was a famous paper a number of years ago, by MacLow & Ferrara in 1999, where they studied those effects, and they distinguished between two effects: basically one is called 'blow-out', the other 'blow-away'. The idea was that if you had such a supernova going off in such a shallow potential well, the gas would be ionized and the gas would be blown out of that galaxy; but depending on the energy input, it could cool down after a certain amount of time, then fall back, potentially triggering subsequent star formation. The other idea was that if you have sufficiently high-energy input that also depends on the supernova location within these galaxies, you could actually blow out the gas in such a way that it could be lost from these galaxies. So, both effects may be at work. Additional simulations have been done for the effect of supernovae overall with respect to the chemical enrichment of these objects, and there are some suggestions that SNe might account for much of the metallicity spread that is observed in these systems and that you would only need, say, one or two SN explosions to account for one dex of metallicity range. One does see these kinds of spreads; however, there have been some other simulations (by Ikuta & Arimoto) that suggested that if one actually leaves star formation going on for several Gyr then one would come to this spread, so I think it has not yet been conclusively shown which of these scenarios is true.

The President. Right, well let's thank Eva again [applause]. I think that we should thank both speakers for keeping to time. I now hand over to the Vice-President and let him do a few jokes, while I load up my laptop [laughter].

The Vice-President. I don't think so … [Laughter.] We have had a rather interesting session, it seems to me. We've heard the geophysics of climate change from the Earth's orbital parameters, extending out to the Sun's variable influence on climate, out to our own Galactic backyard. And our President is going to speak now on the final frontier: 'The cosmological distance ladder — to redshift 1000', the 2008 Presidential Address.

The President. [A summary of this talk has appeared in *Astronomy & Geophysics* **49**, 3·30, 2008.]

The Vice-President. Thank you very much for a superb overview, and no need for me to prompt you about timing. Perfect. Now I'm sure there'll be some questions.

Mr. M. F. Osmaston. You left me in the air, because your title says 'to redshift 1000' and you didn't seem to go that far.

The President. Well, I didn't explain it properly then. When we're talking about oscillations in the microwave background radiation — remember I showed you a plot of the CMB, showing very small fluctuations in the cosmic microwave background as measured by *WMAP* — then what we're looking at there is redshift 1000, or redshift 1100; we're looking back to the moment when the Universe became transparent. Prior to this we have a Universe dominated by radiation, every photon is being scattered off electrons, and at this moment, redshift 1100, hydrogen becomes neutral, electrons disappear, the Universe becomes transparent, and the photons travel straight to us, ever since. And so we are looking back to this moment at redshift 1100, and when we measure the Doppler peak, we're taking distance measurements to redshift 1100. I'm sorry if I didn't explain that clearly.

Dr. G. Q. G. Stanley. Is it feasible that our models are so simplistic, that looking for a universal H_0 is misguided, and that we could have a different H_0 at different length scales?

The President. Well … [laughter]. There have been some wacky models in which that could happen. We're in this strange position, I think, that we have a basic cosmological model, the Friedmann–Robertson–Walker model — it's a homogeneous, isotropic universe. It works so well that it's very hard to change our mind-set and think about alternatives, and yet it doesn't really make sense philosophically. It's just extremely weird that we find ourselves in such a universe. One of the weird by-products of those models is that once again we recover Aristotle's absolute time: there's a cosmic time through the whole Universe. Now, we were told that Einstein's Special Relativity demotes time to depend on the relative motion of observers; and you go to General Relativity and it becomes a completely local thing, essentially you don't even have a way of connecting time locally with the time at different distances and with varying gravitational fields. And incidentally, if we weren't in this perfect model, we would not be able to measure distances out to redshift 1100, they would not be well defined. We wouldn't have a prescription telling observers when to put their measuring rod down. I don't know whether you saw *Cranford* on BBC1 where they used a cable to measure the distance between two towns; what General Relativity tells us is that it's not very easy to do that, unless you have the situation you have, for example, in the Universe, where there is a cosmic frame, and therefore if you tell people when to do these measurements, they will get a consistent answer. So, it's a very strange model that we find ourselves in. There are explanations as to why the Universe may have been driven closer to such a model if it wasn't near it to start with — the inflation hypothesis, and so on: I think that's also not terribly convincing as a philosophy. So I think it's a deep question that you've asked. The official answer is "no" [laughter], but I think the real answer is "yes, that's interesting".

Dr. J. G. Morgan. Could I just be a bit more specific on a similar theme? There is a very big leap from the microwave background to the alternative, much more local ways of estimating H_0 you talked about. So the question is, how confident can we be in those more local measurements that the value of H_0 coming from them is not distorted by local fluctuations in the Hubble flow, caused by galaxy clusters, superclusters, and the local mass structure of the Universe?

The President. I think that's a good point. That is something we do know how to model, and when I said that I redid what Friedman *et al.* did, just to see whether it would make any difference, one of the things that I did was a more thorough modelling of the local flow. We derived such a model from the *IRAS* surveys and incorporated that. So, it does matter, yes, it absolutely matters what the contributions of peculiar motions are, but that is all corrected for, after a fashion.

Dr. R. C. Smith. You've placed quite a lot of emphasis in your local models on the Baade-Wesselink method; I understand why, but I understood that if you try to apply it to supernovae, there's a problem, because the photometric variations are not coming from the same part of the expanding atmosphere as the kinematic variations, so you need three-dimensional radiation hydrodynamic models to interpret the data.

The President. That is very true. The Baade-Wesselink method works best for something that approximates to a black body, and it's not bad for Cepheids, but it's not at all good for type-Ia supernovae, because they have such strong lines, so you have a very complex radiative-transfer problem. It works better for type-II supernovae, and people have applied this method to those. Unfortunately they're less luminous, so we can't see them so far, so that's not really going to give us much of a picture out to redshift 1, which type-1a supernovae do. My feeling is that radiative transfer in complicated atmospheres is a well-understood problem, so we can jolly well get down and do it properly and get an estimate by that route. I think that's the next task for you, and others younger perhaps!

Mr. Regnart. As a post-script to your conclusions, at the present time what ratio of dark energy to dark matter to light matter do you find least unsatisfactory?

The President. Well, the consensus is that Ω_Λ is 0·75, Ω_{Matter} is 0·25, of which 0·04 is baryonic; and of that 0·04, 10 per cent is visible, and accounted for so far. As I've made clear, I am slow to reconcile myself to a Universe with a cosmological constant in it, but I'm beginning to think that it could make sense if one thinks of it as an aspect of gravitation. Gravitation is a two-parameter effect: nearby it pulls, and far out it pushes, and it's nothing to do with dark energy and it's nothing to do with the vacuum, it's just gravitation. And if that's correct, and that seems to be what we see locally, in my mind it undermines the philosophy of inflation, and energy density of the vacuum driving the Universe, and all that. So I'm sort of reconciled to the picture out to redshift 1, but I'm not happy with the bit at redshift 10^8.

The Vice-President. I think we'll have to stop the questions, but, before our President wallows totally in heresy, let us thank him very much for a really excellent lecture. Thank you very much. [Applause.]

The President. There is a drinks party in the library in Burlington House. The next meeting is on Friday, March 14th.

THE VARIABLES OF NGC 6366

By C. Lloyd
The Open University

C. D. Pike
Pitlochry

A. Terzan
Observatoire Astronomique de Lyon

and H. B. Sawyer Hogg†

By combining early plate material with more recent CCD photometry, the single RR Lyrae variable in NGC 6366 is found to have a constant period of 0·51316255(9) days from 1933 to 1990. The other variable in the field is probably a semi-regular red variable with a time scale of 100–300 days, but it is not necessarily a cluster member.

Introduction

NGC 6366 is a Galactic globular cluster and is generally considered to be a close twin of 47 Tuc. It is rather closer to us than 47 Tuc and its open structure allows photometry into the cluster core, but it suffers from high extinction and significant differential reddening so has received less attention. Colour–magnitude (CM) diagrams of NGC 6366[1–5] show a predominantly red horizontal branch, a metal abundance close to [Fe/H] $= -0 \cdot 7$, and a sparse RR Lyrae population — in fact, just the one variable. Despite its relatively high metal abundance and proximity to the Galactic Centre its high velocity places it as a halo globular cluster, in contrast to M71 with which it is often compared.

In the first search for variables in NGC 6366 Sawyer[6] found six suspects, but on further investigation only two variables were confirmed[7]. Both are described as 'fairly conspicuous' as they are among the brightest stars in the cluster and both have ranges of $\sim 1 \cdot 5$ magnitudes. The first variable, known as V1, lies close to the cluster core at $x'' = -26$ and $y'' = -42$ in Sawyer's co-ordinate system (arc seconds from the nominal cluster centre measured east and north at equinox 1900, corresponding to $\alpha = 17^h\,27^m\,42^s \cdot 69$, $\delta = -05° \, 05' \, 25'' \cdot 2$ (2000) from 2MASS). A long series of observation by Sawyer Hogg confirmed that this is an RR Lyrae variable, and the only one in the cluster[1]. The second star, V2, lies in the outer parts of the cluster at $x'' = +305$, $y'' = -390$ (corresponding to $\alpha = 17^h\,28^m\,04^s \cdot 65$, $\delta = -05° \, 11' \, 13'' \cdot 9$ (2000) from 2MASS). Pike[1] and Harris[2] found that V2 is very red and lies near the tip of the giant branch, so is most likely some type of long-period variable. V2 is also coincident with the *IRAS* faint source F17254−0508. In a search for additional variables Pike[1] measured stars on the horizontal branch in the core of the cluster for variability but found none, and Harris[2] similarly found no additional variables, even amongst the blue stragglers in the cluster. The purpose of this paper is to review the data used by Sawyer Hogg and place limits on any period change in V1, and also to try to establish the nature of V2.

† Helen Sawyer Hogg, 1905–1993, remembered with affection by those who knew her.

Observations

The observations were 20–30 minute exposures on a blue emulsion taken mostly with the 72-inch at the David Dunlap Observatory, with a few of the early plates taken on the 74-inch at the Dominion Astrophysical Observatory. The plates cover the period 1933 July to 1969 August, JD = 2427273 to 2440447. The brightness of the variables, V1 and V2, was estimated visually by Sawyer Hogg and Terzan independently, with respect to nearby comparison stars as given by Sawyer[7], and listed in Table I. A few plates were missed by one or other observer but the last 17 of the 127 plates were measured only by Terzan.

Harris lists *B* and *V* magnitudes for the brighter stars in the cluster core, which includes these comparison stars. A plot of m_{pg} *vs* *B* magnitude yields a tight linear relationship with $\sigma_{pg} = 0 \cdot 12$ magnitudes, but the photographic magnitudes are typically 0·7 magnitudes too bright and there is a scale error of about 10%. To bring the data closer to reality the *pg* magnitudes have been converted to *B* using the linear relationship $B = 0 \cdot 897\, m_{pg} + 2 \cdot 273$ and the converted data have been used throughout the rest of the analysis.

TABLE I

NGC 6366 comparison star data from Harris[2] and Sawyer[7]

Name[7]	Number[2]	V[2]	$B-V$[2]	B[2]	m_{pg}[7]
a	86	13·731	1·237	14·968	14·2
b	229	13·643	2·269	15·902	15·1
c	126	14·204	1·880	16·084	15·5
d	116	14·967	1·823	16·790	16·0
e	107	15·594	1·556	17·150	16·7
f	144	15·955	1·626	17·581	17·1

Variable 1 — the RR Lyrae star

The first published measurements of V1 were by Pike[1], who made 13 photographic observations over three nights and was able to find an approximate period of 0·508 days. The mean magnitude and colour place the star ~0·2 magnitudes below the horizontal branch in the RR Lyrae gap. Most of Pike's observations lie near the minimum of the light curve, with a few showing the sharp rise to maximum, suggesting that the star is an RRab-type variable. In a note added in proof Pike quotes a comment from Sawyer Hogg confirming the classification and providing a preliminary period of 0·513162 days.

More complete observations of V1 were made by Harris[2] and these further confirmed the classification and allowed an independent measure of the period of 0·51316(2) days. By combining his and Pike's data Harris was able to improve the period to 0·5131634(4) days on the assumption that the cycles had been correctly counted between the two data sets, and that the period had not changed significantly. However, an error of one cycle equates to a change in the period of ± 0·000045 days, which is only twice the formal uncertainty on Harris' period, but given Sawyer Hogg's preliminary period it does seem likely that this value is correct.

From Sawyer Hogg's photographic data there are 106 estimates of V1 common to both Sawyer Hogg and Terzan, with an additional three by Sawyer Hogg and 18 by Terzan alone. The agreement between those made by both observers is good

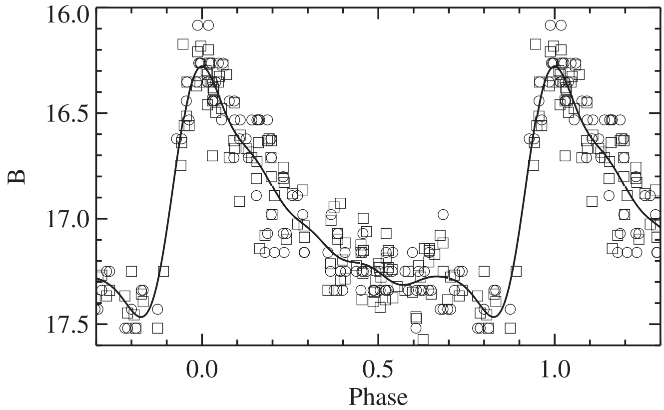

FIG. 1

The *B*-magnitude light curve of V1 using the merged estimates of Sawyer Hogg (○) and Terzan (□). The r.m.s. scatter is 0·13 magnitudes, which is entirely consistent with visual estimates from photographic plates.

with H1 − T1 = 0·00 ± 0·17, so there is no significant difference in the mean, and the distribution of differences is relatively symmetrical, with only two plates giving a difference of > 0·4 magnitudes. In view of the close agreement between the two sets of estimates they have been merged into a single set.

Although the period of V1 is known, a discrete Fourier transform (DFT) periodogram of all the data has been constructed to see if this shows anything unusual. The periodogram shows a series of clearly resolved sharp spikes centred on f = 1·9487 c/d (0·51316 days) with the usual 1 c/d and other aliases, showing no suggestion of broadening or splitting that would indicate any lack of stability in the period. A least-squares Fourier fit of order six to the all data is shown in Fig. 1 and gives a period of 0·513162(18) days, which is sensibly identical to that of Harris. In order to investigate the stability of the period, the data have been divided into five equal subsets and the time of maximum light determined for each subset. These are given in Table II together with the time of maximum light from Harris' data by fitting a Fourier series of order 6. This value is consistent with that given by Harris. The ephemeris from the five new timings alone gives a period of 0·5131630(8) days and an O − C residual of −0·0081 for Harris' data,

TABLE II

O − C residuals of the times of maximum light

HJD	*Cycle*	*O − C (days)*	*Comment*
2428517·6611 (78)	−5967	−0·0050	This paper
2429781·0790 (80)	−3505	+0·0067	This paper
2430508·7267 (108)	−2087	−0·0101	This paper
2431579·7132 (67)	0	+0·0061	This paper
2435523·3650 (116)	7685	+0·0037	This paper
2442250·4104 (120)	20794	+0·0012	Pike[1]
2448009·1194 (29)	32016	+0·0000	Harris[2]

which is well within the uncertainty and suggests that the stability of the period is very good. Pike's observations do not cover the maximum of the light curve, but the phasing has been calculated by comparison with the Fourier fit to Harris' data. To derive the best period, Harris' timing has been included with twice the weight of the other six values and this reduces its absolute O–C residual to $<0{\cdot}0001$ days. The ephemeris of maximum light is

$$\mathrm{HJD}_{\mathrm{Max}} = 2431579{\cdot}7071(28) + 0{\cdot}51316255(9)\,E$$

The O–C residuals are plotted in Fig. 2 together with those of all the estimates with magnitudes brighter than $B = 16{\cdot}4$, by way of comparison. These values tend to give slightly later O–C residuals, which probably reflects the change in gradient of the light curve on either side of the maximum. From a quadratic fit to the seven timings used to derive the linear ephemeris, the second order term is $(-0{\cdot}3 \pm 2{\cdot}0) \times 10^{-11}$ d c^{-2}, so there is no indication of any change in the period, and the uncertainty places a 1-σ upper limit on any period change of $\dot{P}/P < 1{\cdot}5 \times 10^{-10}$ d^{-1}.

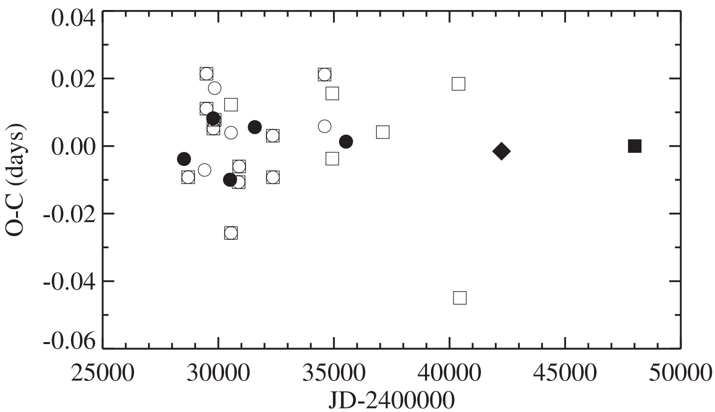

FIG. 2

O–C diagram of the times of maximum light of V1 for the five subsets of the data (●), from Harris (■) and Pike (◆), together with the brightest estimates of Sawyer Hogg (○) and Terzan (□) with $B < 16.4$.

Fourier decomposition components of Harris' V data

It has been demonstrated empirically that some of the Fourier decomposition components of RR Lyrae light curves are related to physical parameters of the stars. There are three values that can be derived from the light curve of V1 that may be compared with those derived from the cluster; these are [Fe/H], M_V, and hence $(m - M_V)_0$ and $(B - V)_0$. The relationship between these parameters and the Fourier decomposition components are given by Jurcsik[8] as,

$$[\mathrm{Fe/H}] = -5{\cdot}038 - 5{\cdot}394\,P + 1{\cdot}345\,\varphi_{31}$$

$$M_V = 1{\cdot}221 - 1{\cdot}396\,P - 0{\cdot}477\,A_1 + 0{\cdot}103\,\varphi_{31}$$

$$(B-V)_0 = 0\cdot308 + 0\cdot163\,P - 0\cdot187\,A_1$$

and are based on the observed properties of 272 RRab stars. The Fourier fit to Harris' V data has been calculated using the form $y = A_0 + \Sigma A_k \cos(k\omega t + \varphi_k)$ and the usual decomposition components have been calculated, which are given in Table III. Unfortunately, the relationships given by Jurcsik were constructed using the sine form of the Fourier series, so before they can be used the φ_{31} value in Table III must be converted by (effectively) adding π. Substituting the appropriate values in the expressions above gives [Fe/H] $= -0\cdot29 \pm 0\cdot16$, $M_V = 0\cdot94 \pm 0\cdot01$, and $(B-V)_0 = 0\cdot338 \pm 0\cdot001$. The most widely quoted values for the metallicity of NGC 6366 are $[\mathrm{Fe/H}]_{\mathrm{ZW}} = -0\cdot58$ and $[\mathrm{Fe/H}]_{\mathrm{CG}} = -0\cdot73$[4,5,9] but the published values[3] range from $-0\cdot99$ to $-0\cdot50$, with the original value of Pike being consistent with the more modern ones. However, by any measure the value derived from the Fourier decomposition components is extreme, and does stretch the uncertainties. How much can be read into this is not clear. Harris' light curve is of high quality but it is not complete so there is a possibility that the Fourier components of the complete curve may be different; however, fits with order 4 and 8 give the same results within the errors.

To calculate the distance modulus requires the reddening, and the consensus seems to be[1,3,4,5] that $E_{(B-V)}$ is $0\cdot70 - 0\cdot75$, with Harris's value of $0\cdot8$ looking rather extreme. The luminosity-weighted mean V magnitude of V1 from the Fourier fit to Harris' data is $\langle V\rangle = 15\cdot777$ (practically identical to Harris' value), so assuming $R = 3\cdot1$ then $(m - M_V)_0$ is $12\cdot51 - 12\cdot66$, which is within the range found from the CM diagrams, $12\cdot6$[1], $12\cdot42$[2], $12\cdot26$[3], $12\cdot78$[4], and $12\cdot87$[5].

Harris estimated that V1 has a mean $B-V = 1\cdot18$, so taking the range of $E_{(B-V)}$ given above this yields $(B-V)_0$ between $0\cdot43$ and $0\cdot48$, but this does not compare favourably with $(B-V)_0 = 0\cdot338$ derived from the Fourier decomposition. This could be overcome by an increase of at least $0\cdot1$ in $E_{(B-V)}$ but that would have serious implications for the distance, so it seems that M_V and $(B-V)_0$ are difficult to reconcile.

TABLE III

Fourier decomposition components for Harris' V data

k	A_k	σ	R_{k1}	σ	φ_{k1}	σ
1	0·2849	0·0050				
2	0·1576	0·0088	0·5534	0·0324	4·1629	0·0827
3	0·0989	0·0074	0·3471	0·0266	2·4448	0·1209
4	0·0536	0·0062	0·1881	0·0221	0·7575	0·1982

Variable 2 — the red variable

There are 103 estimates of V2 made by both observers, with an additional 13 by Terzan and four by Sawyer Hogg alone. The difference between the two observers is much larger than was the case for V1, with H2–T2 $= 0\cdot11 \pm 0\cdot20$; there is a more skewed distribution with the peak at $-0\cdot2$ magnitudes, and four observations have differences $> 0\cdot4$ magnitudes. The reason for the poorer agreement is probably because V2 is over 8 arc minutes from the cluster core and the comparisons stars. To analyse the light curve of V2, two combinations of the data have been constructed. Firstly, the mean of the best observations with magnitude differences of $\leqslant 0\cdot4$, and secondly, all the data, in both cases with the mean offset removed to place the magnitudes on the system of Sawyer Hogg.

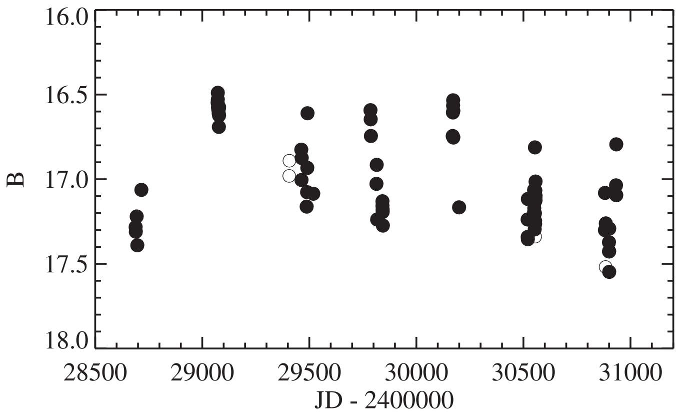

FIG. 3

The densest part of the light curve of V2 showing the 'corrected' mean *B* data (●) and the individual estimates of Sawyer Hogg (○). Terzan had no isolated observations during this period.

The densest part of the light curve of V2 is given in Fig. 3 and it shows a variation of about one magnitude with coherent runs of observations over 30 – 50 days, but with no obvious time scale. A DFT periodogram of both sets of data shows a strong series of aliases near integer numbers of cycles per day indicating a long period. There are several comparable features between 100 and 2000 days, with the nominally strongest peak at a period of 118 days giving a full amplitude of 0·5 magnitudes, but this does not produce a convincing phase diagram.

The variations of V2 are clearly not periodic but are coherent over 30 – 50 days and cycle on a time scale of < 300 days. Unfortunately, the time distribution of the observations is not suited to monitoring variations on this time scale, but it seems most likely that the time scale of the variations is 100 – 300 days.

Harris[2] made one observation of V2 with $V = 15{\cdot}022$ and $(B-V) = 2{\cdot}218$, which placed the star about a magnitude below the tip of the giant branch, and Pike[1], in a note added in proof, also placed V2 near the tip of the giant branch. The full range of variation of V2 is less than 1·5 magnitudes so it seems likely that the mean magnitude of V2 lies below the giant branch, which raises the possibility that V2 is not a member of the cluster. V2 also lies over 8 arc minutes from the cluster centre where the density of evolved stars is about two orders of magnitude lower than in the cluster core[2].

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THE DISTRIBUTION OF BINARY-SYSTEM MASS RATIOS: AN EXTENDED, LESS-BIASSED SAMPLE

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University of California, Irvine,
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During the past 30 years, the astronomical literature has included orbits by Roger Griffin and colleagues for at least 297 single-lined and 97 double-lined spectroscopic binaries derived from data gathered wholly or partially with the *Cambridge Radial Velocity Spectrometer* and its close relatives. These systems have, on average, considerably longer periods than SB systems in other compilations and considerably smaller velocity amplitudes (except in comparison to samples of stars with planets), with medians near 700 days and 9·5 km s^{-1} for the SB1s and the extremes rising to several decades and falling to less than 2 km s^{-1}. From these orbits, I here extract a distribution of mass ratios, using long-established but imperfect methods. A number of correction factors might be adopted: for over-representation of SB2s, observational selection against long periods, small amplitudes (especially when combined with large rotation speeds and/or near-equal brightness of the stars), large eccentricities, and very possibly other facts of life. Some combination of such corrections can undoubtedly produce whatever distribution your favourite models for binary-star formation predict; but the unvarnished graph does not look much like any such model.

Introduction

Curiously, the subject of binary-star statistics and Roger Griffin were both born in 1935[1,2]. Kuiper found, using only single-line binary systems, that $N(M_2/M_1)$ was reasonably well described by $(1+M_2/M_1)^{-2}$. Since then, astronomers have continued to take a sporadic interest in the subject down to the present time[3,4], and it is a bit disconcerting to realize that my own first contribution to the topic[5], undertaken in hopes of providing useful information to the folks attempting to model populations of X-ray binaries, goes back almost half way to the beginning. That study, which I shall hereafter call paper 0 (for which no Roman numeral equivalent exists), resulted in a bimodal distribution, with peaks at $M_2/M_1 = 0 \cdot 3$ and 1·0. Recent studies[3,4] have more or less confirmed this finding, but such bimodality was unpopular for a decade or two in between.

The occasion for the present discussion of the topic is the publication in the most recent issue of *The Observatory* of R. F. Griffin's 200th paper[6] in the series 'Spectroscopic Binary Orbits from Photoelectric Radial Velocities'. It may (or may not) interest you to know that this is not the only large number with which he is associated. His Eddington cycling number* is 62, and the corresponding number for running is 23.

The data base

The 394 orbits examined here come, first and foremost, from Griffin's papers I[7] to 200[6] in *The Observatory* series. Additional ones appeared in a series of 20-some papers in the *Journal of Astronomy and Astrophysics* up to 1994[8], and smaller numbers were published in *PASP*[9], the *Journal of the Royal Astronomical Society of Canada*, and the *Astronomical Journal* (including use of a radial-velocity spectrometer on the Palomar 200-inch telescope). The subset of all these series published up to 1988 were analyzed earlier[10] in what I would like to call paper $^1/_2$ (which also has no Roman numeral equivalent).

That earlier subset of 132 SB1s and 32 SB2s already had much smaller velocity amplitudes (median 9 $\mathrm{km s^{-1}}$) and much longer periods (median 590 d) than systems in other catalogues and compilations. For the present larger group, the median K_1 remains near 9 km $\mathrm{s^{-1}}$ and the median period is even longer (about 700 d).

You must consult the almost 250 original papers to see how all these stars were chosen for radial-velocity monitoring, but the targets range from stars recorded as having composite spectra as far back as the time of the *Henry Draper Catalogue* (and almost bound to be binaries, often SB2s) to IAU radial-velocity standards, which ought not have been binaries at all.

Methods

This means what I did and why. "Methodology" would convey an even more misleading impression of rigour. Deriving the mass ratios for double-lined systems is easy; they are the ratios of the two velocity amplitudes, at least in the case of the relatively wide separations considered here. Contact systems like W UMa and cataclysmic variables like U Gem, with gas streams, accretion and decretion discs, winds, and hot spots, cannot make that claim.

Systems where the radial-velocity spectrometer reveals features from only one star (though the other might be visible by other methods) are more difficult. The published papers tabulate the period, P, velocity amplitude, K, eccentricity, e, and mass function $f(M) = M_2^3 \sin^3 i/(M_1 + M_2)^2$, all in customary astronomers' units; i is the angle of inclination of orbit to sky (90° = edge-on; 0° = face-on). If both i and one of the masses were known, then only a bit of arithmetic would be required. Generally neither is, though in the relatively few cases where specific values of one or both were recommended along with the orbit, these have been used. Otherwise, an average value of $\sin^3 i = 0{\cdot}679$ was adopted. This assumes that systems will be represented in the sample in proportion to $\sin^2 i$, one power coming from the available 'phase space' (there are more ways for an orbit to be edge-on to you than face-on, if you think of spinning it around X, Y, and Z axes) and the other from easier detectability of larger velocity amplitudes. This is the same choice as made in papers 0 and $^1/_2$.

*Your Eddington cycling number is the largest number N, such that you have cycled at least N miles on N days. Your Hirsch index is a similar N for numbers of papers published under or around your name and the number of times they have been cited. One could perhaps divide astronomers into two classes, based on which is larger, their Eddington numbers or Hirsch indices. Mine are nearly equal and both modest (picture a figure 9 clasping a *négligée* around herself).

A value of M_1 (the star represented in the velocity traces) then has to be adopted from its spectral type. The published papers typically give as much information about this and about apparent and absolute magnitudes as was available to the author(s) at the time of publication. As in paper 0, all K giants are assumed to have masses of 3 $M_\odot$, Am stars are 2·0 $M_\odot$, and Gamma Dor stars 1·6 $M_\odot$, for our purposes. Ordinary main-sequence-star masses of adequate accuracy for this study can be found in any introductory astronomy text (or taken from *Astrophysical Quantities, 3rd Edition*, 1976). Luminosity class IIs are taken to be 4–5 $M_\odot$, and luminosity class IVs somewhere between 3 $M_\odot$ and the value for a main-sequence star of the same surface-temperature class. Moving all the assumed M_1s down will move all the mass ratios up, and conversely, though not a great deal within the range of plausible M_1s for most spectral types except the highest-luminosity classes.

A notable trend through the sequence of *The Observatory* papers is gradual increase in length. Part of this is a change in ground rules from a strict one star per paper to, sometimes, as many as 4–6 per paper. The other factor is more thorough discussion of earlier and complementary work on each star. The amount of this has, obviously, increased over the years (this is called the progress of astronomy). In addition, *Simbad* and other data bases have made what is available easier to find. *Hipparcos* photometry and parallaxes are of particular importance in trying to deduce a likely value for M_1 and are addressed where they exist.

Results and implications

Table I and Fig. 1 show the distributions binned as q = 0·0 – 0·1, 0·1 – 0·2, and so forth. The most obvious questions are (*a*) what to do about combining single- and double-lined systems, and (*b*) what does $q >$ 1 mean? Just how to merge SB1 and SB2 samples is always a topic of dispute. The table and figure show both the simple sum and the SB1s plus half the SB2s. The second choice has two motivations. First, the Griffin sample is partly magnitude limited, and an SB2 has two stars of nearly equal brightness, so the class will be somewhat over-represented. Second, in a typical catalogue like those compiled by Batten and his colleagues, the SB2s are clearly over-represented because they have also called attention to themselves by showing two sets of lines on first examination. A number of the Griffin systems were taken from lists of composite-spectrum stars, for which this consideration applies. So, there is no one right answer to (*a*), and they are listed separately in the table so that you can do whatever you want; the figure shows SB1 + SB2 and SB1 + 1/2 SB2.

Apart from major observational errors, four causes can contribute to derived mass ratios in excess of one. For the SB1 systems, the primary is, by definition,

TABLE I

Distribution of the SB1, SB2, and sums across the range of mass ratios

	Mass Ratio Range										
	0·0– 0·1	*0·1– 0·2*	*0·2– 0·3*	*0·3– 0·4*	*0·4– 0·5*	*0·5– 0·6*	*0·6– 0·7*	*0·7– 0·8*	*0·8– 0·9*	*0·9– 1·0*	*>1·0*	*Total*
SB1s	14	67	57	45	26	24	24	18	7	4	11	297
SB2s	0	1	0	0	1	2	5	14	23	51	0	97
SB1 + SB2	14	68	57	45	27	26	29	32	30	55	11	394
SB1 + 1/2 SB2	14	67·5	57	45	26·5	25	26·5	25	18·5	29·5	11	325·5

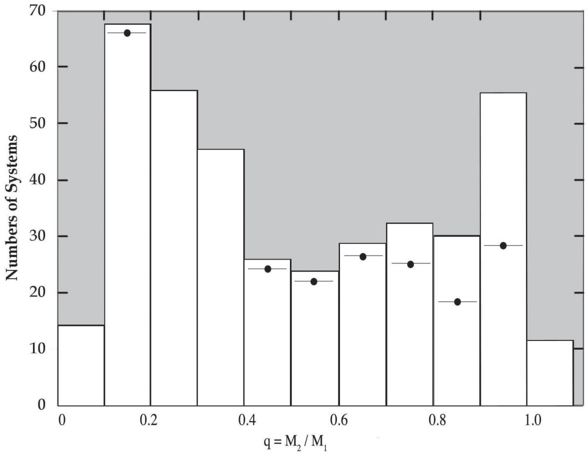

FIG. 1

Numbers of systems as a function of mass ratio. The tops of the bars represent all the binaries from the radial-velocity-spectrometer papers (297 SB1s and 97 SB2s). Lines beneath the tops of the bars are SB1s plus one-half the SB2s.

the star whose lines are visible in the radial-velocity-spectrometer traces. It is, therefore, the more luminous, at least in the red, but there are several reasons why it might not be the more massive. These are: (*i*) A nearly edge-on orbit, so that $\sin i = 1$ (relevant only up to $q = 1 \cdot 24$). (*ii*) Significant under-estimate of M_1 (probably relevant only for the brightest stars). (*iii*) A secondary which is itself a double of shorter period, since two stars of mass $M/2$ are a good bit fainter than one of mass M (systems with periods more than a year or two will certainly have room for this). (*iv*) A secondary which was the initial primary and has evolved to become a white dwarf, neutron star, or black hole (possible only for quite wide systems since there will otherwise be at least some accretion onto the compact star contributing to the light at short wavelengths).

How might one interpret the numbers in the table and figure? The total sample is conspicuously bimodal, with peaks at $q = 0 \cdot 2 - 0 \cdot 3$ (*vs.* 0·3 in Paper 1/2) and at $q = 1$. There is not enough range in spectral types or masses to claim any correlation between M_2/M_1 and the masses of the stars. It would probably also be a mistake to look for clues to the nature of the star-formation process in the larger velocity amplitudes of the SB2s with mass ratios of 0·8 or more (median $K_1 =$ 29 km s^{-1} *vs.* 9 km s^{-1} for the SB1s), since there is a strong selection effect against small amplitudes with nearly equal line strength (because the two dips will look like a single, broad-lined, constant-velocity star).

If you give SB2s only half credit, then you can take your ruler and draw a couple of lines across the figure (only, of course, if this is your own copy of *The Observatory*, as the author and Editors hope it is). There is a rapid decline from the $q = 0 \cdot 1 - 0 \cdot 2$ bin to $0 \cdot 4 - 0 \cdot 5$ and nearly constant numbers beyond. This doesn't really look like the predictions of any particular star-formation scenario or like the suggestions made in Papers 0 and 1/2. It is, of course, possible to change the shape of the distribution a good deal by ignoring the SB2s completely, or by going back to the original papers and giving extra credit to long-period, high-eccentricity systems, because they are very difficult to find, or making any other correction for observational selection effects that might appeal to you.

The very small number of systems in the $q = 0{\cdot}0 - 0{\cdot}1$ bin will surely trigger the remark "observational selection" again, though notice that $M_1 = 3\, M_\odot$ and $q = 0{\cdot}05$ takes us down into what is often called the brown-dwarf desert, because exoplanet searches have found very few (short period!) companions in that mass range.

Acknowledgements

I am indebted to the late Daniel Magnes Popper and Bohdan Paczynski for an introduction to observations and theory of binary stars, respectively; to Roger Griffin for detailed input to Paper 1/2 and for numerous discussions of binary stars and other items over the years; and to David Stickland for providing copies of the 2008 April and June papers in advance of publication.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 201: HDE 245814 AND HDE 260988

By R. F. Griffin
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The two stars were discovered by Miss Cannon to exhibit composite spectra. Very little is known about them, but HDE 260988 was observed by *Hipparcos* and was found to have almost no parallax and to be a very close 'visual' binary. Both stars have proved to be spectroscopic binaries, and single-lined orbits are presented for the late-type components.

They have low eccentricities, and periods of about 7 and 6 years, respectively. HDE 245814 has the enormous mass function of 1·3 $M_\odot$ — larger than the mass functions of the classical ζ Aurigae binaries. Unless the secondary star is itself double, it would have to be a very massive and luminous system, but its spectral type has been given as G9 II–III, which argues against that. HDE 260988, too, has a large mass function, although at 0·32 $M_\odot$ it is nothing like as large as that of HDE 245814. It is not possible to present a coherent model of HDE 260988 because the *Hipparcos* and radial-velocity indications are mutually contradictory.

Introduction

HDE 245814 and 260988 are stars that were too faint to feature in the original *Henry Draper Catalogue* but were picked up by Miss Cannon subsequently in the *Henry Draper Extension*[1]. She noted both of them as having composite spectra and accordingly assigned them twin *HDE* numbers — HDE 245814/5 as G5 and A2, and HDE 260988/9 as K0 and A0. For convenience they are identified here by the numbers of the late-type components alone, since those are the ones whose orbits are determined. The two systems are both close to the Galactic plane in the anti-centre direction, in that part of the Milky Way which in a northerner's view runs down to the left of Taurus and Orion. HDE 245814 is a $9^1/_2{}^{\mathrm{m}}$ star in the north-eastern corner of Taurus, about 3° south-following β Tau, while HDE 260988 is about a magnitude brighter and is in Monoceros, about 10° following Betelgeuse.

Both systems were listed by Hynek in his comprehensive catalogue[2] of composite spectra, but he had no information about them beyond their classifications in the *HDE*. Both also appear in the classifications made by Ginestet *et al.*[3] in the near infrared of the late-type components of composite systems; there, HDE 245814 is listed as being of type G9 II–III and HDE 260988 as K6: Ib:.

Although HDE 245814 does not feature in the *Hipparcos Catalogue*, it was observed by *Tycho*; its magnitudes, as derived from the *Tycho 2*[4] V_T and B_T by the transformations given by equations 1·3·20 in the *Introduction* in Vol. **1** of the *Hipparcos Catalogue*, are $V = 9^{\mathrm{m}}{\cdot}56$, $(B-V) = 0^{\mathrm{m}}{\cdot}83$. The V magnitude of HDE 260988 is listed by *Hipparcos* from the satellite's own measurements as $8^{\mathrm{m}}{\cdot}64$; *Vizier* gives the magnitudes transformed from *Tycho* as $V = 8^{\mathrm{m}}{\cdot}77$, $(B-V) = 1^{\mathrm{m}}{\cdot}53$. The parallax of HDE 260988 is less than its standard error, so all that it tells us is that the object is a long way away, probably at least 500 pc and possibly a lot more: if the luminosity class of the late-type component is really Ib, then the apparent distance modulus must be about 13 magnitudes. The true modulus could be considerably less, since the object is less than a degree from the Galactic plane and there could be substantial absorption and reddening between it and us. Indeed, taken at face value the $(B-V)$ colour would hardly be consonant with the admixture of enough light from a hot star for the composite nature of the spectrum to have been recognized, unless the colour index were significantly increased by interstellar reddening. The *Tycho 2* proper motion is zero within its uncertainties of less than 0″·002 in each coördinate; the *Hipparcos* motion in right ascension is significantly non-zero, but does not differ from the *Tycho 2* value by enough for the difference to be attributed with much confidence to evidence of photocentric motion.

Radial velocities and orbits

The principal source of the radial-velocity measurements for the two stars has been the Cambridge *Coravel*, with which 49 observations have been made of HDE 245814 and 48 of HDE 260988, both objects having been seen round a complete cycle with it. Both, however, were placed on the observing programme in the early 1990s, before the Cambridge instrument was available, and additional velocities were obtained with the Haute-Provence *Coravel* (14 and 15 observations for the respective stars), the DAO spectrometer (one and two, respectively), and the ESO *Coravel* (one and two). Furthermore, Dr. J.-M. Carquillat has very kindly contributed 12 further Haute-Provence velocities of HDE 245814. The total

TABLE I

Radial-velocity observations of HDE 245814

Except as noted, the sources of the observations are as follows:
1992–1999 — Haute-Provence Coravel (weighted 1/2 in orbital solution);
2000–2008 — Cambridge Coravel

Date (UT)			*MJD*	*Velocity* km s⁻¹	*Phase*	*(O – C)* km s⁻¹
1992	Jan.	24·00	48645·00	+12·2	0·443	+0·2
1993	Feb.	16·00	49034·00	19·9	0·590	−1·2
	Nov.	3·37*	294·37	28·6	·688	−0·9
1994	Jan.	2·07	49354·07	33·2	0·710	+1·6
		8·06	360·06	32·3	·713	+0·5
	Feb.	17·93	400·93	34·3	·728	+1·1
	May	1·82	473·82	35·1	·756	−0·5
	Oct.	18·17†	643·17	40·4	·819	−0·2
	Dec.	11·11	697·11	42·4	·840	+0·6
		29·06	715·06	41·3	·847	−0·8
1995	Mar.	8·87†	49784·87	43·1	0·873	0·0
1996	Jan.	1·02	50083·02	41·0	0·985	+1·1
	Feb.	10·88†	123·88	38·9	1·001	+0·5
	Mar.	29·88	171·88	36·4	·019	−0·1
	Aug.	31·16†	326·16	28·1	·077	−1·0
	Nov.	22·15	409·15	24·2	·108	−0·7
	Dec.	2·04†	419·04	25·3	·112	+0·8
		16·05	433·05	23·7	·117	−0·1
1997	Jan.	26·00	50474·00	21·4	1·133	−0·5
		29·02†	477·02	22·5	·134	+0·7
		31·95†	479·95	21·9	·135	+0·3
	Mar.	11·91‡	518·91	20·0	·150	+0·1
	Apr.	11·87‡	549·87	18·9	·161	+0·2
	Sept.	14·16	705·16	13·3	·220	−0·3
	Oct.	19·04†	740·04	13·2	·233	+0·4
	Dec.	22·10	804·10	12·6	·257	+1·1
1998	Jan.	21·91†	50834·91	11·1	1·269	0·0
		24·99†	837·99	11·2	·270	+0·2
	Oct.	20·17†	51106·17	9·7	·371	−0·3
1999	Jan.	6·94†	51184·94	10·2	1·401	−0·4
	Apr.	15·19§	283·19	11·5	·438	−0·3
	Dec.	29·04‡	541·04	+17·7	·535	+0·7

TABLE I (*concluded*)

Date (UT)			*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2000	Jan.	9·00	51552·00	+18·0	1·539	+0·7
	Feb.	11·98	585·98	18·6	·552	+0·4
	Mar.	3·88	606·88	18·7	·560	−0·1
	Apr.	5·85	639·85	19·7	·572	0·0
	Sept.	21·18	808·18	24·9	·636	0·0
	Nov.	13·15	861·15	25·9	·656	−0·7
	Dec.	3·12	881·12	27·5	·663	+0·2
2001	Jan.	6·99	51915·99	28·7	1·676	+0·2
	Feb.	14·91	954·91	31·6	·691	+1·8
	Mar.	13·85	981·85	29·9	·701	−0·8
	Oct.	7·15	52189·15	36·7	·779	−0·9
	Dec.	14·10	257·10	39·6	·805	0·0
2002	Feb.	5·95	52310·95	40·5	1·825	−0·4
	Mar.	1·91	334·91	41·6	·834	+0·1
	Apr.	3·86	367·86	42·2	·847	+0·1
	Oct.	18·18	565·18	43·6	·921	+0·2
	Dec.	11·16	619·16	42·7	·941	−0·1
2003	Jan.	10·07	52649·07	42·0	1·953	−0·2
	Feb.	13·94	683·94	41·5	·966	+0·1
		19·92	689·92	41·7	·968	+0·4
	Mar.	16·90	714·90	40·6	·977	+0·1
	Apr.	7·84	736·84	39·2	·986	−0·6
	Sept.	29·18	911·18	32·4	2·051	0·0
	Oct.	25·18	937·18	31·0	·061	−0·1
	Nov.	17·10	960·10	29·9	·070	−0·1
	Dec.	8·05	981·05	28·9	·078	0·0
2004	Jan.	9·04	53013·04	27·9	2·090	+0·6
	Feb.	9·01	044·01	26·4	·101	+0·6
	Apr.	22·85	117·85	21·0	·129	−1·3
	Sept.	4·17	252·17	16·1	·180	−0·7
	Oct.	7·20	285·20	15·7	·192	0·0
	Nov.	13·18	322·18	14·6	·206	0·0
2005	Sept.	17·18	53630·18	10·2	2·322	+0·4
	Nov.	4·16	678·16	9·9	·341	+0·1
	Dec.	26·98	730·98	9·6	·360	−0·3
2006	Feb.	8·90	53774·90	10·3	2·377	+0·2
	Apr.	10·84	835·84	10·3	·400	−0·3
	Sept.	21·16	999·16	13·1	·462	+0·3
	Nov.	26·16	54065·16	13·7	·486	−0·4
2007	Feb.	6·88	54137·88	15·0	2·514	−0·7
	Mar.	21·88	180·88	16·6	·530	−0·1
	Sept.	16·18	359·18	21·1	·597	−0·5
	Oct.	18·21	391·21	22·6	·609	0·0
	Nov.	24·10	428·10	23·3	·623	−0·5
2008	Jan.	6·04	54471·04	+25·6	2·639	+0·4

*Observed with ESO *Coravel*; wt. 1/2.
† Observed by Dr. J.-M. Carquillat; wt. 1.
‡ Observed with Cambridge *Coravel*.
§ Observed with DAO 48-inch telescope; wt. 1/2.

TABLE II

Radial-velocity observations of HDE 260988

Except as noted, the sources of the observations are as follows:
1990–1997 — Haute-Provence Coravel (weighted 1/2 in orbital solution);
1999–2008 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1990 Feb. 14·19*	47936·19	+12·9	0·091	−0·5
1992 Jan. 16·03	48637·03	24·7	0·425	−0·4
Feb. 28·29†	680·29	25·8	·446	−0·6
Dec. 19·08	975·08	33·3	·586	0·0
1993 Feb. 15·03	49033·03	34·7	0·613	+0·5
Mar. 18·90	064·90	35·1	·629	+0·5
Nov. 5·34*	296·34	35·0	·739	−0·1
Dec. 30·12	351·12	35·5	·765	+1·0
1994 Feb. 17·94	49400·94	34·9	0·789	+1·2
Dec. 11·14	697·14	24·7	·930	+0·3
1995 Jan. 2·05	49719·05	23·8	0·940	+0·3
Dec. 24·08	50075·08	13·1	1·110	+0·1
1996 Apr. 2·86	50175·86	13·3	1·157	+0·7
Nov. 21·15‡	408·15	15·4	·268	−0·5
Dec. 15·14	432·14	17·2	·279	+0·7
1997 Jan. 26·05	50474·05	17·3	1·299	−0·3
Mar. 30·84‡	537·84	20·0	·330	+0·7
Apr. 27·83	565·83	20·0	·343	−0·1
Sept. 15·13	706·13	24·3	·410	+0·1
Dec. 21·08	803·08	27·0	·456	0·0
1999 Nov. 4·48†	51486·48	33·9	1·781	0·0
Dec. 29·05	541·05	33·4	·807	+0·5
2000 Jan. 20·00	51563·00	32·6	1·818	+0·3
Feb. 12·00	586·00	31·6	·829	−0·2
Mar. 3·94	606·94	30·8	·839	−0·4
Apr. 5·85	639·85	29·3	·854	−0·9
10·84	644·84	29·9	·857	−0·1
Sept. 25·20	812·20	23·7	·936	−0·1
Oct. 17·22	834·22	22·6	·947	−0·3
Nov. 13·19	861·19	21·6	·960	−0·2
Dec. 30·04	908·04	20·4	·982	+0·4
2001 Jan. 6·99	51915·99	19·2	1·986	−0·5
Feb. 16·95	956·95	18·4	2·005	+0·2
Mar. 12·93	980·93	17·3	·017	0·0
Oct. 12·20	52194·20	12·3	·118	−0·5
Nov. 2·15	215·15	12·2	·128	−0·5
2002 Jan. 2·06	52276·06	12·5	2·157	−0·1
Feb. 6·97	311·97	13·1	·174	+0·3
20·86	325·86	13·2	·181	+0·3
Mar. 26·87	359·87	13·1	·197	−0·1
Oct. 19·21	566·21	17·2	·295	−0·1
Dec. 5·08	613·08	+18·6	·318	0·0

TABLE II (*concluded*)

Date (UT)			*MJD*	*Velocity* km s^{-1}	*Phase*	*(O – C)* km s^{-1}
2003	Jan.	10·07	52649·07	+19·0	2·335	−0·6
	Feb.	13·95	683·95	20·4	·351	−0·2
	Mar.	16·81	714·81	22·1	·366	+0·6
	Apr.	17·84	746·84	22·6	·381	+0·2
	Sept.	29·19	911·19	27·0	·460	−0·2
	Oct.	25·19	937·19	27·7	·472	−0·2
	Nov.	28·12	971·12	28·6	·488	−0·2
	Dec.	29·04	53002·04	29·8	·503	+0·2
2004	Jan.	30·03	53034·03	30·5	2·518	+0·2
	Feb.	27·92	062·92	31·5	·532	+0·5
	Apr.	5·83	100·83	32·0	·550	+0·2
	Sept.	21·18	269·18	33·6	·630	−1·0
	Oct.	22·22	300·22	34·3	·645	−0·6
	Dec.	26·11	365·11	35·4	·676	+0·1
2005	Jan.	26·07	53396·07	35·4	2·690	0·0
	Nov.	14·13	688·13	31·3	·829	−0·4
2006	Jan.	28·95	53763·95	29·4	2·865	0·0
	Apr.	3·84	828·84	27·3	·896	+0·2
		10·85	835·85	27·1	·900	+0·3
2007	Jan.	14·09	54114·09	16·6	3·032	+0·3
	Feb.	6·95	137·95	16·0	·043	+0·4
	Mar.	21·87	180·87	14·5	·064	0·0
	Apr.	14·84	204·84	14·1	·075	+0·1
2008	Jan.	24·97	54489·97	13·6	3·211	0·0
	Mar.	9·92	534·92	+14·5	·232	+0·1

*Observed with ESO *Coravel*; wt. 1/2.
†Observed with DAO 48-inch telescope; wt. 1/2.
‡Observed with Cambridge *Coravel*.

numbers of data for the objects are therefore 77 and 67, respectively; they are set out in Tables I and II. A radial velocity given for HDE 260988 by Famaey *et al.*[5] is not an independent measurement but is based on such of the observations in Table II as were to be found in the *Coravel* data base which served as the source for those authors' compilation.

The Haute-Provence and ESO velocities in Tables I and II have been adjusted by +0·8 km s^{-1}, as usual in this series of papers. In the solutions of the orbits, the Cambridge measures have been given unit weight and the other sources half-weight, with the exception that the HDE 245814 velocities provided by Dr. Carquillat were exempted from such down-weighting. It is very likely that the difference noticeable in the qualities of the velocities obtained with the same instrument by the different observers arose from impatience on the part of the present writer, resulting in integration times too short really to do justice to the potentialities of the instrument. No doubt the same fault was responsible for the need to down-weight the Haute-Provence measures of the other star too. The orbits are readily solved; they are illustrated in Figs. 1 and 2, and their elements are given here in Table III.

TABLE III

Orbital elements for HDE 245814 and 260988

Element	*HDE 245814*	*HDE 260988*
P (days)	2652·7 ± 4·0	2100·8 ± 3·1
T (MJD)	52775 ± 17	51946 ± 24
γ (km s^{-1})	+25·04 ± 0·07	+24·48 ± 0·05
K_1 (km s^{-1})	16·90 ± 0·10	11·41 ± 0·08
e	0·137 ± 0·005	0·091 ± 0·006
ω (degrees)	45·6 ± 2·4	118 ± 4
$a_1 \sin i$ (Gm)	610·5 ± 3·9	328·2 ± 2·3
$f(m)$ ($M_\odot$)	1·292 ± 0·024	0·320 ± 0·007
R.m.s. residual (wt. 1) (km s^{-1})	0·52	0·37

Discussion

The two objects have orbits that are of a quite normal character for those of giant stars, except that in both cases the amplitudes are large in relation to the periods, exceptionally so in the case of HDE 245814. The mass functions are therefore unusual. That of HDE 260988 is at the level at which eclipses might well be looked for in systems in which one component is a giant — the mass function of the eclipsing G8 IIIa + A3/4V system[6] τ Per, for example, is scarcely larger (0·34 $M_\odot$). If the luminosity class of the primary star is as bright as the Ib tentatively given by Ginestet *et al.*[3], however, there is little likelihood of eclipses: among the classical ζ Aur stars with supergiant primaries, the smallest mass function (that of 32 Cyg[7]) is nearly 0·5 $M_\odot$. The projected orbital radius, $a_1 \sin i$, is 2·2 AU; that

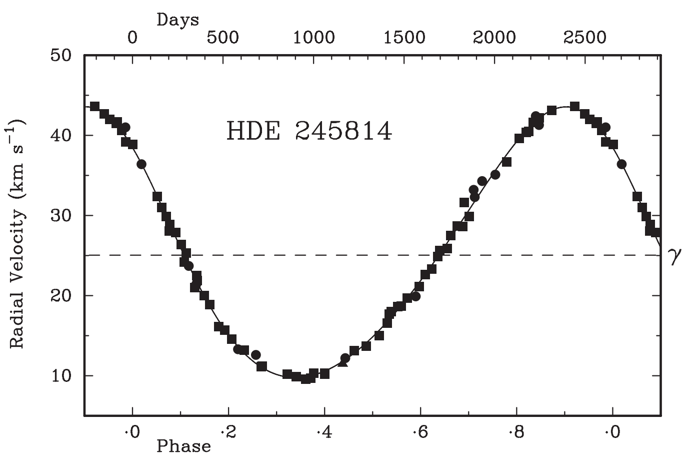

FIG. 1

The observed radial velocities of HDE 245814 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Cambridge measurements are plotted as squares, Haute-Provence velocities (and one ESO one) as circles, and the single DAO one as a triangle.

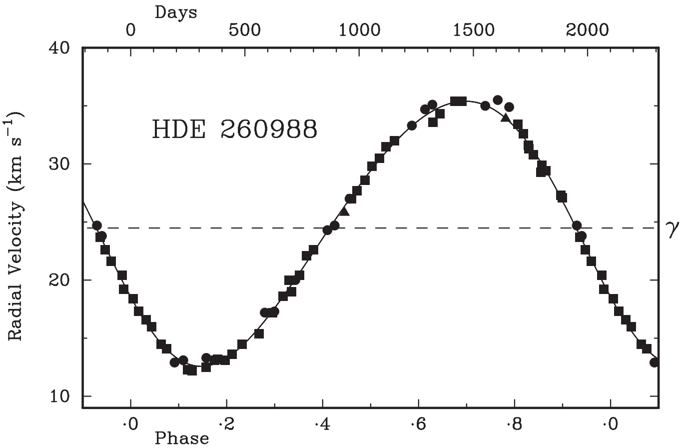

FIG. 2
As Fig. 1, but for HDE 260988.

refers to the orbit of the primary star around the centre of gravity, so we might expect the separation of the component stars to be of the order of 5–6 AU. The object was just about resolved on the sky by *Hipparcos* into two stars whose magnitudes were the same within their (large) uncertainties ($\Delta m = 0^m \cdot 44 \pm 0^m \cdot 73$) and whose angular separation was about $0'' \cdot 1$ (less than one-third of the Dawes[8] limit for the *Hipparcos* 290-mm effective aperture); at the distance of 500 pc or more, that would correspond to a linear separation of upwards of 50 AU, so the *Hipparcos* companion could not be identical with the spectroscopic one and makes the system at least triple.

The discovery of the 'visual' duplicity calls into question the nature of the spectroscopic secondary, and indeed of the whole system. If the system is a near-equal 'visual' binary, then something like half the light in the *Hp* passband, $\sim V$, must come from a component that is not involved in the spectroscopic orbit, and is not itself a late-type star, else the radial-velocity traces would register it. The equivalent width of the dip seen in radial-velocity traces, however, is something like three-quarters as great as that of an undiluted late-K giant or supergiant, seemingly showing that at least *that* proportion the light in the blue/violet part of the spectrum comes from the late-type star whose velocity has been measured. There is a clear contradiction between those deductions that follow directly from the *Hipparcos* results on the one hand and from the writer's on the other: an early-type star that contributes perhaps one-quarter of the light in the blue, so far from increasing its share to about one-half in *V*, would be down by at least another magnitude at that wavelength owing to the disparity in colour indices between the components. Clearly, adjudication must be left to others; they might be encouraged to verify, for a start, that the object is resolved optically. It must also be recalled that a significant amount of flux must be assigned somewhere in the system to the source of the early-type spectrum recognized by Miss Cannon.

The mass function of HDE 245814 is remarkable; among composite-spectrum binaries it is exceeded only by VV Cep (which is in a class of its own: "indeed, even the name of the object seems to have been ordained to remind us that it is a Very Very 'Ceptional star!"[9]) and of HD 187299[10]. There are other systems with very large mass functions, but they are explained by duplicity of the secondary. If the primary of HDE 245814 is really of the luminosity class listed by Ginestet *et al.*[3] (II–III), then the conclusion has to be that it too has a double secondary.

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THE ORBITAL AND PHYSICAL PARAMETERS OF THE ECLIPSING BINARY OW GEMINORUM

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We present our multicolour photometric data of the primary and secondary eclipses of OW Gem that took place in 1995, 2002, and 2006, as well as new radial-velocity data collected since 1993 by R. F. Griffin and A. Duquennoy. The Wilson–Devinney code was used for the simultaneous solution of both photometric and spectroscopic data. A complete set of orbital and physical parameters of the components was obtained. The pair of values, eccentricity $e = 0{\cdot}5286$ and argument of periastron $\omega = 140^\circ{\cdot}73$, give better compatibility of the moment of the secondary minimum with the observations compared to previous estimates.

Introduction

OW Gem is an unusual, long-period eclipsing binary, composed of two evolved supergiant stars. Variability of the star was noticed by Kaiser[1] during photographic searches for novae in 1988 March. Photoelectric observation soon showed a shallow secondary minimum[2] at phase 0·23 in the *V* band, indicating that the orbit has high eccentricity. It turned out that eclipses were visible on Harvard photographic plates[3] as far back as 1902. The orbital period (1258·59 days — about 3·45 years) was derived from 11 eclipse events, which took place before 1992[4]. Radial-velocity data of very good quality were obtained by Griffin & Duquennoy[5] and used by them for the first reliable analysis of the system. The presence of eclipses distinguishes OW Gem from several known similar systems[6], hence we know the exact masses of the components in this case (5·5 and 3·8 $M_{\odot}$). The object has turned out to be unusually interesting: both its components have quite large, but very different masses and yet they are now both apparently in the short phase of supergiant evolution. Therefore the evolutionary status of the system seems to be in contradiction to current models of stellar evolution.

Many attempts at modelling the system parameters have appeared in recent years as a result of several authors carrying out good-quality multicolour photometric measurements of the eclipses. Derekas *et al.*[7] have presented a simple model based on observations of the main minimum at the turn of the years 2001 and 2002, in which they estimate the temperature and the luminosity of the components. Another group (Terrell *et al.*[8]) has used the Wilson–Devinney code (hereafter WD) for modelling based on the photoelectric observational data of Kaiser *et al.*[9] together with the only radial-velocity measurements[5] available at that time, and they obtained a complete set of parameters. In the same year, we presented a simple model[10] within the confines of which we have obtained the inclination of the orbit and the most probable temperature T_2 = 4950 K of the secondary (cool) component for an assumed value of the primary temperature T_1 = 7100K. In that model, the limb darkening was neglected and stellar fluxes were approximated by black bodies. The present work includes new multicolour photometry covering one (1995) primary eclipse and three (1995, 2002, 2006) secondary eclipses. We have independently obtained a complete set of parameters using our own photometry and part of the photometric data of Derekas *et al.*[7]. The radial-velocity data — some of which are new — are shown in Table I; they were kindly supplied by Roger Griffin who, together with A. Duquennoy, collected them over the last fourteen years; weights suggested by Professor Griffin have been applied. Additionally three of our own radial-velocity measurements have been included in the analysis (Table II).

Observational photometric data

An international observational campaign to monitor primary and secondary eclipses was organized by Terrell *et al.* during 1995[11]. Responding to this, we obtained multicolour photometry for both events with a 60-cm Cassegrain reflector at Piwnice Observatory near Toruń (Poland). We used a single-channel diaphragm photometer with an uncooled EMI 9558B photomultiplier. Our response curves for *U*, *B*, and *V* bands are very close to the standard Johnson system, whereas our broad *R* and *I* bands have significantly shorter mean wavelengths than the Johnson *RI* and Cousins $(RI)_C$ (Table III). The accuracy of our measurements was ±0·03, ±0·02, ±0·017, ±0·02, and ±0·019 magnitudes in *UBVRI*, respectively. Unfortunately, in the analysis based on the 1995 campaign[9] our photometric data were not included: our observations of the 1995 secondary

TABLE I

Radial velocity data obtained by R. F. Griffin and A. Duquennoy

Heliocentric Date	*HJD* − *2400000*	*Velocity (km s*$^{-1}$*)* *prim.*	*sec.*	*Source*★	*Weight*
1988 Nov. 3·21	47468·71	−19·0	—	OHP	1
1988 Nov. 7·19	47472·69	−18·4	—	OHP	1
1988 Dec. 6·09	47501·59	−17·0	—	Cambridge-old	1/8
1988 Dec. 13·04	47508·54	−16·5	—	Cambridge-old	1/8
1988 Dec. 20·04	47515·54	−14·0	—	Cambridge-old	1/8
1989 Jan. 5·03	47531·53	−15·3	—	Cambridge-old	1/8
1989 Jan. 17·98	47544·48	−12·5	—	Cambridge-old	1/8
1989 Feb. 24·18	47581·68	−10·8	+6·1	ESO	1
1989 Mar. 26·84	47612·34	−8·9	+0·4	OHP	1
1989 Apr. 29·82	47646·32	−7·0	+0·6	OHP	1
1989 Oct. 30·16	47829·66	−0·7	−14·9	OHP	1
1989 Nov. 17·14	47847·64	+2·6	—	Cambridge-old	1/8
1989 Dec. 23·04	47883·54	+1·3	—	Cambridge-old	1/8
1990 Jan. 14·01	47905·51	+2·2	—	Cambridge-old	1/8
1990 Jan. 30·02	47921·52	+2·1	−18·4	OHP	1
1990 Feb. 12·14	47934·64	+1·5	−18·9	ESO	1
1990 Apr. 4·84	47986·34	+3·2	—	Cambridge-old	1/8
1990 Oct. 7·20	48171·70	+6·6	—	Cambridge-old	1/8
1990 Dec. 4·13	48229·63	+7·2	—	Cambridge-old	1/8
1991 Jan. 26·01	48282·51	+6·6	−22·6	OHP	1
1991 Mar. 13·85	48329·35	+6·5	—	Cambridge-old	1/8
1991 Apr. 3·87	48350·37	+3·7	—	Cambridge-old	1/8
1991 Oct. 29·16	48558·66	−30·4	+30·2	OHP	2
1991 Dec. 19·04	48609·54	−32·5	+33·5	OHP	2
1992 Jan. 14·01	48635·51	−29·9	+30·2	OHP	2
1992 Jan. 18·00	48639·50	−29·1	+30·3	OHP	2
1992 Jan. 24·06	48645·56	−28·4	+31·0	OHP	2
1992 Feb. 27·30	48679·80	−24·1	+22·3	DAO	2
1992 Apr. 22·85	48735·35	−18·5	+14·0	OHP	1
1992 Aug. 16·13	48850·63	−9·0		OHP	0
1992 Aug. 17·13	48851·63	−9·1		OHP	0
1992 Dec. 18·07	48974·57	−4·8		OHP	0
1993 Feb. 15·93	49034·43	−3·4		OHP	0
1993 Mar. 22·86	49069·36	−1·4	−16·8	OHP	1
1993 Apr. 20·83	49098·33	−0·4	−15·5	OHP	1
1993 Aug. 30·16	49229·66	+1·5		OHP	0
1993 Sep. 12·14	49242·64	+4·2	−10·6	OHP	1
1993 Nov. 6·34	49297·84	+3·3		OHP	0
1994 Jan. 3·05	49355·55	+6·4	—	OHP	1
1994 Feb. 19·88	49403·38	+6·1	−18·7	OHP	1
1994 Apr. 30·84	49473·34	+5·7		OHP	0
1994 Dec. 12·13	49698·63	−1·6	−17·0	OHP	1
1995 Jan. 5·06	49722·56	−4·4		OHP	0
1996 Jan. 1·08	50083·58	−10·1		OHP	0
1996 Mar. 31·87	50174·37	−6·4		OHP	0
1996 Dec. 16·07	50433·57	+1·5		OHP	0
1997 Jan. 26·03	50474·53	+2·8	−15·0	OHP	1
1997 Sep. 11·16	50702·66	+5·2		OHP	0
1997 Dec. 21·06	50803·56	+6·5	−15·9	OHP	1
2000 Jan. 9·02	51552·52	−1·5	−9·5	Cambridge	1
2000 Feb. 28·86	51603·36	−0·7	−15·9	Cambridge	1
2000 Apr. 6·85	51641·35	+1·3	−11·5	Cambridge	1
2000 Nov. 13·17	51861·67	+5·4	−15·4	Cambridge	1
2001 Jan. 7·02	51916·52	+5·6	−18·0	Cambridge	1
2001 Feb. 13·93	51954·43	+6·3	−20·0	Cambridge	1
2001 Nov. 14·20	52227·70	−2·7	−13·8	Cambridge	1
2001 Dec. 12·11	52255·61	−5·9		Cambridge	0

TABLE I (*concluded*)

Heliocentric Date	*HJD* *− 2400000*	*Velocity (km s⁻¹)* *prim.*	*sec.*	*Source**	*Weight*
2001 Dec. 22·05	52265·55	−9·2	−1·7	Cambridge	1
2002 Jan. 17·99	52292·49	−17·7	+18·2	Cambridge	1
2002 Jan. 24·97	52299·47	−20·2	+17·2	Cambridge	1
2002 Feb. 5·98	52311·48	−23·8	+26·0	Cambridge	2
2002 Feb. 16·90	52322·40	−27·5	+25·7	Cambridge	2
2002 Mar. 1·88	52335·38	−29·5	+31·2	Cambridge	2
2002 Mar. 9·90	52343·40	−31·3	+29·6	Cambridge	2
2002 Mar. 27·87	52361·37	−33·1	+34·7	Cambridge	2
2002 Apr. 6·86	52371·36	−33·0	+34·4	Cambridge	2
2002 Apr. 23·85	52388·35	−32·2	+35·2	Cambridge	2
2002 Oct. 24·18	52571·68	−13·2	—	Cambridge	1
2002 Nov. 7·20	52585·70	−12·3	—	Cambridge	1
2003 Jan. 6·01	52645·51	−8·3	−4·1	Cambridge	1
2003 Dec. 15·13	52988·63	+2·8	−14·6	Cambridge	1
2004 Feb. 27·94	53063·44	+4·0	−18·2	Cambridge	1
2005 Jan. 11·03	53381·53	+5·6	−18·6	Cambridge	1
2005 Nov. 25·16	53699·66	−26·4	+27·9	Cambridge	2
2005 Dec. 28·09	53732·59	−22·4	+23·5	Cambridge	2
2006 Feb. 20·93	53787·43	−17·0	+9·3	Cambridge	1
2007 Apr. 10·87	54201·37	+1·5	−12·2	Cambridge	1

*Sources:
OHP — *Coravel* at Haute-Provence Observatory.
Cambridge-old — the original radial-velocity spectrometer at Cambridge, with which Griffin first developed the cross-correlation method of measuring velocities (*ApJ*, **148**, 465, 1967).
Cambridge — *Coravel* instrument currently working at Cambridge Observatory.
DAO — instrument at Dominion Astrophysical Observatory.
ESO — a spectrometer similar to *Coravel*.

The velocities written between the columns for the primary and secondary have been reduced as if the system were single-lined and zero weights have been applied in those cases.

The data before 1992 Aug. 16 were published already by Griffin & Duquennoy in 1993, but new reductions have been applied to the whole set and these are given here.

TABLE II

Radial-velocity data obtained from Rozhen Observatory spectra

Date	*JD* *− 2400000*	*Velocity (km s⁻¹)* *prim.*	*sec.*	*Weight*
2005 Jan. 20	53391·31	+5·5 ± 0·8	−18·2 ± 1·3	1
2006 Apr. 14	52581·70	−12·5* ± 0·9		1

*This is a measurement of the blend of primary and secondary but unit weight was applied.

TABLE III

Mean wavelengths of the four photometric systems used by us

Detector: *Band*	*EMI 9558B* λ (Å)	*Burle C31034* λ (Å)	*SBIG:STL 11000* λ (Å)	*SBIG:STL 1001* λ (Å)
U	3708	3678	3676	~3600
B	4342	4467	4392	~4400
V	5436	5426	5343	5404
R	6391	6689	6319	6414
I	7420	8380	8020	8305

TABLE IV

UBVRI photometry of the 1995 primary and secondary eclipses collected with the EMI9558B photomultiplier

JD − 2400000	*ΔU*	*ΔB*	*ΔV*	*ΔR*	*ΔI*
49723·4524	0·010	−0·320	−0·758	−1·026	−1·247
49731·3870	−0·006	−0·340	−0·766	−1·020	−1·260
49741·4052	−0·011	−0·320	−0·767	−1·022	−1·241
49751·3617	0·004	−0·339	−0·743	−1·016	−1·232
49751·5129	−0·004	−0·330	−0·765	−1·029	−1·233
49754·2218	0·087	−0·233	−0·687	−0·954	−1·169
49754·2680	0·041	−0·256	−0·695	−0·950	−1·185
49754·3465	0·105	−0·243	−0·669	−0·938	−1·136
49754·4260	0·107	−0·209	−0·655	−0·942	−1·150
49756·2605	0·393	0·069	−0·416	−0·681	−0·937
49756·4032	0·411	0·084	−0·378	−0·662	−0·898
49757·2831	0·669	0·299	−0·188	−0·477	−0·774
49758·4480	1·084	0·702	0·114	−0·232	−0·488
49759·2654	1·410	0·911	0·290	−0·101	—
49761·2366	1·777	1·336	0·597	0·201	−0·106
49761·5347	—	1·191	0·507	0·097	−0·179
49762·4387	1·221	0·851	0·235	−0·137	−0·412
49763·5336	0·811	0·430	−0·082	−0·408	−0·682
49764·4510	0·507	0·185	−0·303	−0·599	−0·847
49765·4287	0·289	−0·033	−0·492	−0·782	−1·009
49766·2876	0·143	−0·174	−0·563	−0·846	−1·089
49767·3374	0·043	−0·279	−0·720	−0·966	−1·174
49769·4922	−0·038	−0·315	−0·761	−1·007	−1·214
49771·3802	0·004	−0·320	−0·749	−1·013	−1·221
50040·4597	—	−0·377	−0·782	−1·016	−1·229
50044·4597	0·005	−0·311	−0·720	−0·963	−1·171
50047·4604	0·006	−0·296	−0·686	−0·927	−1·151
50048·4013	—	−0·266	−0·679	−0·913	−1·086
50066·4347	0·025	−0·306	−0·741	−0·989	−1·232
50068·3413	—	−0·331	−0·734	−0·969	−1·215
50884·4409	−0·044	−0·355	−0·750	−1·028	−1·231
50895·3600	−0·040	−0·329	−0·764	−1·016	−1·259
51078·5412	−0·015	−0·311	−0·760	−1·022	−1·262
51196·3584	−0·025	−0·335	−0·756	−1·020	−1·239
51257·4235	−0·008	−0·350	−0·768	−1·028	−1·269

eclipse have poor time coverage as a result of bad weather. To fill the gaps in the light curves, new data were obtained covering the 2002 secondary eclipse. A single-channel diaphragm photometer with a cooled Burle C31034 photomultiplier and a set of five filters U, B, V, R_C, I_C were used. Their response curves were close to the standard Johnson–Cousins $UBV(RI)_C$ system (Table III). Additionally, two intermediate-band interference filters (FWHM $\approx$ 100Å), h (located at Hβ around λ = 4870 Å) and c (located in the continuum around λ = 4804 Å), were used. The accuracy of those measurements was ±0·028, ±0·021, ±0·018, ±0·017, and ±0·021 magnitudes in $UBVR_CI_C$, respectively. Data around the last secondary minimum of 2006 have been obtained with two new CCD detectors (SBIG: STL 11000 and STL 1001) with a new filter set. The mean wavelengths of those photometric systems are presented and compared with the two previous photometric systems in Table III. The accuracy was ±0·018, ±0·007, ±0·005, ±0·007, and ±0·008 magnitudes in the $UBVR_CI_C$ filters, respectively. HD 258848 was chosen as the comparison star and GSC 1332–0578 as a check star, both suggested by Terrell *et al.*[11]. Our original differential magnitudes (OW Gem – HD 258848) are presented in the Tables IV – VII) and as *UBVRI* light curves in Fig. 1.

TABLE V

Photometry of the 2002 secondary eclipse collected with the Burle C31034 photomultiplier

JD −2400000	ΔU	ΔB	ΔV	ΔR_C	ΔI_C	Δc	Δh
52520·5887	−0·011	−0·399	−0·750	−1·089	−1·405	−0·555	−0·605
52526·5821	−0·002	−0·401	−0·779	−1·114	−1·398	−0·549	−0·612
52528·5610	0·022	−0·395	−0·763	−1·120	−1·423	−0·569	−0·574
52530·5813	0·055	−0·406	−0·761	−1·090	−1·397	−0·521	−0·626
52535·5425	0·031	−0·412	−0·772	−1·100	−1·400	−0·560	−0·578
52537·5415	0·044	−0·382	−0·745	−1·084	−1·395	−0·549	−0·562
52542·6051	0·044	−0·379	−0·748	−1·087	−1·376	−0·560	−0·565
52550·5663	0·031	−0·388	−0·765	−1·088	−1·382	−0·564	−0·589
52552·5397	0·034	−0·386	−0·751	−1·095	−1·384	−0·562	−0·596
52558·6378	—	−0·353	−0·738	−1·054	−1·351	−0·530	−0·576
52567·4629	—	−0·335	−0·650	−0·926	−1·205	−0·475	−0·505
52568·4644	0·080	−0·325	−0·643	−0·939	−1·220	−0·442	−0·510
52572·5023	0·068	−0·331	−0·640	−0·940	−1·217	−0·441	−0·490
52574·5563	0·094	−0·307	−0·652	−0·934	−1·257	−0·432	−0·431
52581·4831	0·052	−0·383	−0·724	−1·057	−1·351	−0·511	−0·577
52584·4530	0·018	−0·420	−0·778	−1·112	−1·406	−0·555	−0·605
52584·6540	0·074	−0·391	−0·770	−1·086	−1·390	−0·571	−0·579
52585·4333	0·020	−0·401	−0·778	−1·106	−1·408	−0·532	−0·578
52585·5563	0·045	−0·388	−0·741	−1·084	−1·386	−0·524	−0·574
52586·5032	0·050	−0·392	−0·749	−1·076	−1·386	−0·549	−0·543
52603·4456	—	−0·364	−0·754	−1·115	−1·406	−0·534	−0·586
52615·4383	0·031	−0·383	−0·750	−1·091	−1·378	−0·547	−0·586
52618·5081	0·054	−0·389	−0·751	−1·107	−1·408	−0·547	−0·579
52714·3303	0·021	−0·388	−0·744	−1·097	−1·391	−0·584	−0·586
52889·6166	—	−0·357	−0·713	−1·068	−1·365	−0·533	−0·522
52890·6166	0·027	−0·367	−0·753	−1·068	−1·361	−0·502	−0·552
52904·5900	0·038	−0·398	−0·769	−1·096	−1·433	−0·517	−0·597
52922·5165	0·043	−0·382	−0·736	−1·086	−1·379	−0·559	−0·604
52935·5517	0·049	−0·384	−0·744	−1·083	−1·382	−0·541	−0·594
52950·4901	—	−0·423	−0·763	−1·142	−1·385	−0·596	−0·577
52985·3382	0·032	−0·378	−0·771	−1·101	−1·409	—	—
53008·5002	—	−0·378	−0·714	−1·086	−1·351	—	—
53026·5686	—	−0·421	−0·764	−1·125	—	—	—
53056·3132	0·051	−0·352	−0·738	−1·090	−1·381	—	—
53069·4435	0·059	−0·397	−0·749	−1·114	−1·373	—	—
53077·4511	0·051	−0·417	−0·769	−1·121	−1·383	—	—
53094·3720	0·009	−0·349	−0·779	−1·103	−1·403	—	—
53110·3685	0·029	−0·367	−0·758	−1·093	−1·428	—	—
53122·3420	—	−0·409	−0·760	−1·087	−1·344	—	—
53273·6200	—	−0·390	−0·741	−1·098	−1·382	—	—
53291·5840	0·015	−0·386	−0·771	−1·118	−1·407	—	—

TABLE VI

$UBV(RI)_C$ data obtained with the SBIG STL 11000 CCD camera. The columns with HJD+ denote the decimal part of the day

HJD	*HJD+*	ΔU	*HJD+*	ΔB	*HJD+*	ΔV	*HJD+*	ΔR_C	*HJD+*	ΔI_C
2453449	·4107	−0·131	·4380	−0·373	—	—	—	—	—	—
2453463	·3898	−0·128	·3955	−0·378	·4031	−0·716	·4066	−0·993	·4107	−1·329
2453477	—	—	·3587	−0·389	·3556	−0·725	·3684	−1·019	·3709	−1·339

TABLE VII

$UBV(RI)_C$ data of the 2006 secondary eclipse obtained with the SBIG STL 1001 CCD camera. The columns with HJD+ denote the fraction of the day.

HJD	*HJD+*	*ΔU*	*HJD+*	*ΔB*	*HJD+*	*ΔV*	*HJD+*	*$ΔR_C$*	*HJD+*	*$ΔI_C$*
2453648	·6597	−0·110	·6614	−0·381	·6622	−0·748	·6638	−1·032	·6644	−1·375
2453745	·6161	−0·152	·6129	−0·388	·6104	−0·750	·6185	−1·036	·6196	−1·379
2453760	—	—	·4652	−0·388	·4591	−0·751	·4784	−1·028	·4805	−1·382
2453789	·2514	−0·116	·2476	−0·381	·2451	−0·750	·2551	−1·027	·2582	−1·389
2453799	·3832	−0·137	·3861	−0·385	·3873	−0·743	·3887	−1·024	·3894	−1·366
2453801	·3573	−0·135	·3602	−0·376	·3612	−0·744	·3621	−1·022	·3630	−1·374
2453816	·3272	−0·135	·3267	−0·389	·3348	−0·746	·3307	−1·016	—	—
2453818	·3875	−0·147	·3889	−0·390	·3893	−0·754	·3898	−1·005	·3901	−1·337
2453819	·3655	−0·127	·3683	−0·368	·3700	−0·725	·3713	−0·983	·3723	−1·321
2453828	—	—	·3327	−0·328	·3281	−0·643	·3465	−0·896	·3492	−1·203
2453829	·2706	−0·103	·2652	−0·323	·2613	−0·647	·2741	−0·889	·2908	−1·194
2453829	—	—	—	—	·2931	−0·642	·2898	−0·884	—	—
2453831	·3043	−0·102	·3072	−0·335	·3007	−0·659	·3081	−0·894	·3089	−1·208
2453832	—	—	·3913	−0·342	·3922	−0·664	·3928	−0·904	·3933	−1·231
2453833	—	—	·3907	−0·350	·3796	−0·688	·3808	−0·948	·3819	−1·250
2453837	—	—	—	—	·3607	−0·673	—	—	—	—
2453844	—	—	·3568	−0·394	·3523	−0·738	·3603	−1·032	·3617	−1·368
2454025	·4877	−0·135	·4827	−0·386	·4812	−0·741	·4913	−1·035	·4922	−1·368
2454066	·4471	−0·144	·4389	−0·373	·4354	−0·738	·4406	−1·029	·4418	−1·376
2454120	·6090	−0·169	·6044	−0·425	·6031	−0·794	·6066	−1·043	·6075	−1·376
2454128	·3383	−0·136	·3435	−0·421	·3310	−0·770	·3469	−1·030	·3496	−1·371
2454188	—	—	·2808	−0·431	·2827	−0·773	·2847	−1·047	·2882	−1·383
2454207	·3560	−0·157	·3519	−0·449	·3497	−0·795	·3483	−1·047	·3466	−1·382

Period analysis

An O–C analysis was carried out for verification of the orbital period and for determination of the time between primary and secondary minima. The times of minima from our observations and the time of the secondary minimum from Williams[2] were obtained using Kwee & van Woerden's method[12]. The times of minima are presented in Table VIII. The moment of primary minimum with $E = 18$ was excluded from the analysis because of its obviously large error. The O–C residuals for the primary events were calculated from Williams & Kaiser's[4] ephemeris:

$$JD_{\min I} = 2415779{\cdot}0\ (\pm 0{\cdot}4) + 1258{\cdot}59\ (\pm 0{\cdot}03) \times E, \qquad (1)$$

and are shown in Figure 2. The best fit to these data gives the new ephemeris:

$$JD_{\min I} = 2415778{\cdot}98\ (\pm 0{\cdot}22) + 1258{\cdot}580\ (\pm 0{\cdot}011) \times E \qquad (2)$$

The O–C values for the secondary minima were calculated assuming a new period $P = 1258{\cdot}58$ days (Equation 2) and taking into account an initial value of the phase shift $\Delta\phi_{II} = 0{\cdot}23$ according to Williams[2], which corresponds to 289·473 days (Fig. 2). The residuals obtained for the six measured secondary minima give a mean (O–C) of $3{\cdot}25 \pm 0{\cdot}19$ days and hence the new phase shift is $\Delta\phi_{II} = 0{\cdot}23258 \pm 0{\cdot}00016$.

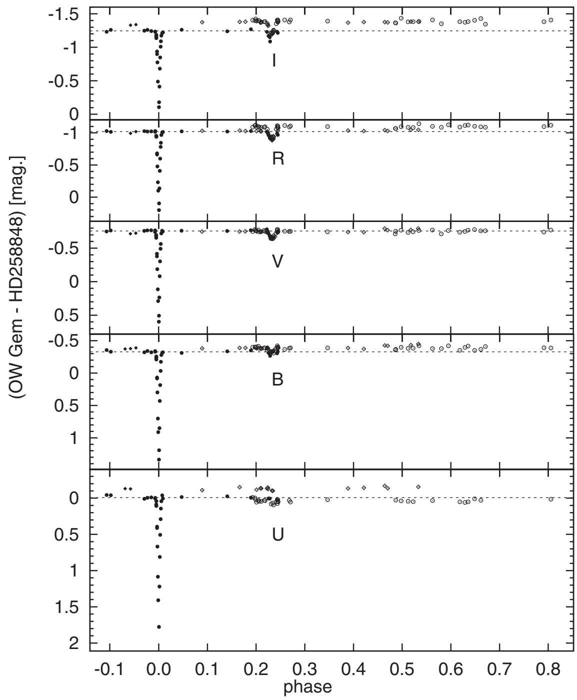

FIG. 1

The *UBVRI* light curves of OW Gem. Our original differential magnitudes are presented. The data were phased with a period of 1258·58 days. The horizontal dashed lines mark the average brightness outside of eclipse in the old photometric system with the EMI 9558B photomultiplier. Data collected with photomultipliers are represented by circles, and those with CCD by diamonds. Filled symbols are used for EMI 9558B or SBIG:STL 11000 and open for Burle C31034 or SBIG:STL 1001.

Preparation of photometric data

We have used five sets of photometric data for modelling. Four sets were obtained at Piwnice Observatory, two with photomultiplier detectors and two with CCDs. The fifth set of CCD data was obtained by Derekas *et al.*[7]. They reported the possible existence of asymmetry in the light curve of the primary eclipse. This effect is not visible in any other data. It seems to be an artefact. Figure 1 in Derekas *et al.*'s paper[7] shows that all of the VR_CI_C data points from the Szeged Observatory lie somewhat above the fit to the Piszkéstetö Observatory data points. Because of this we used only Piszkéstetö data to improve the primary-eclipse time coverage. As a consequence we have collected data from five different photometric systems, which are somewhat shifted in magnitude. Additionally, the depth of minima in particular bands depends on their mean wavelengths. The correction for the depth of secondary eclipse is very small in comparison with the primary eclipse. To transform the data to the homogenous systems without correction for depth of primary eclipse, we shifted the BVR_C Piszkéstetö CCD data (the star GSC 1332–0578 was the comparison star for these data) and the *UBVR* Toruń data obtained with Burle C31034 photomultiplier and STL CCDs to the data obtained in 1995 with the EMI 9558B photomultiplier (Table III). The *I* pass-band established with the

TABLE VIII

The moments of primary and secondary eclipses of OW Gem used for O–C analysis

E	*JD* − 2400000	*Error*	*O–C*	*Author*	*Ref*
			Primary		
0	15779·4	—	0·400	Kaiser	4
2	18295·8	—	−0·380	”	4
4	20812·5	—	−0·860	”	4
5	22072·5	—	0·550	”	4
9	27105·6	—	−0·710	”	4
9	27106·9	—	0·590	”	4
15	34658·0	—	0·150	Fuhrmann	4
16	35916·0	—	−0·440	”	4
18	38435·0*	—	−1·380*	”	4
25	47243·4	±0·5	−0·350	Kaiser *et al.*	4
26	48502·1	±0·4	−0·240	Williams & Kaiser	4
27	49760·857	±0·052	−0·073	Hager	13
27	49760·68	±0·03	−0·250	this work	—
27	49760·59	±0·02	−0·340	Kaiser *et al.*	9
29	52277·77	±0·01	−0·340	Kaiser *et al.*	9
29	52277·73	±0·2	−0·380	Derekas *et al.*	7
			Secondary		
25	47535·50	±0·91	2·547	Williams	2
27	50053·84	±0·71	3·727	this work	—
27	50053·2	±0·2	3·087	Kaiser *et al.*	9
29	52570·9	±0·1	3·627	Kaiser *et al.*	9
29	52570·30	±0·13	3·027	this work	—
30	53829·32	±0·20	3·467	this work	—

* excluded from our analysis

EMI 9558B photomultiplier differs considerably from both Johnson's *I* and Cousins' I_C, so the *I*-pass-band data obtained in the region of the primary eclipse were excluded from analysis. The *I* pass-band established with the STL 1001 CCD detector is quite close to the Cousins I_C pass-band, so this was adopted as the reference system for the infrared domain. The magnitudes from the transformed systems were shifted onto the reference systems by values presented in Table IX.

Additionally, the depths of the secondary minima in the transformed systems were corrected onto the reference systems according to the expression:

$$m_{\mathrm{ref}} = \bar{m}_{\mathrm{ref}} + (m_{\mathrm{trans}} - \bar{m}_{\mathrm{trans}}) \times \alpha_{\mathrm{ref/trans}} \qquad (3)$$

where individual values of measured brightness are denoted by m, mean brightness outside of eclipses by $\bar{m}$ (see Table X), and the α parameter is the ratio of the depth minimum in the reference systems to the depth minimum in the transformed systems (see Table XI).

Values of the α parameter were obtained by fitting a 2nd-order polynomial to the observational depths of the secondary-eclipse data obtained with the Burle C31034 photomultiplier. All the brightness values were normalized to unity corresponding to the mean magnitudes outside the eclipse (Table X), as is required by the differential-correction (DC) procedure of the Wilson–Devinney code.

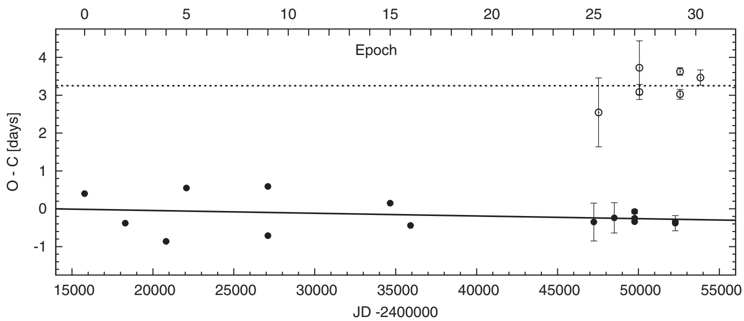

FIG. 2

The O–C diagram for the moments of the primary eclipse from the ephemeris given by Equation 1 (filled circles) and the secondary eclipse from the ephemeris given by Equation 2, with phase shift adopted as $\Delta\phi_{II} = 0 \cdot 23$ (open circles). The best fit for the primary eclipses (solid line) indicates a slightly shorter period. The best fit for the secondary eclipses (horizontal dashed line) was found assuming the new value $P = 1258 \cdot 58$.

TABLE IX

Values of shifts from transformed systems onto reference systems

		Ref. syst. EMI9558B			*Ref. syst. STL1001*
	U	*B*	*V*	*R*	*I*
Trans. syst.					
EMI9558B	—	—	—	—	−0·134
Burle C31034	−0·036	+0·052	−0·008	+0·076	+0·015
STL 11000	+0·128	+0·044	−0·041	−0·014	−0·041
STL 1001	+0·137	+0·062	−0·005	+0·013	—
Piszkéstetö	—	+0·979	+0·891	+0·860	—

TABLE X

Mean differential magnitude (OW Gem – HD 258848) outside the eclipses in particular photometric systems

	U	*B*	*V*	*R*	*I*
EMI9558B	−0·002	−0·336	−0·762	−1·020	−1·241
Burle C31034	0·034	−0·388	−0·754	−1·096	−1·390
STL 11000	−0·130	−0·380	−0·721	−1·006	−1·334
STL 1001	−0·139	−0·398	−0·757	−1·033	−1·375

TABLE XI

Ratio ($\alpha_{ref./trans.}$) of the depth of secondary minima in reference systems to the depth of the secondary minima in transformed systems

$\alpha_{ref./trans.}$	*U*	*B*	*V*	*R*	$\alpha_{ref./trans.}$	*I*
$\alpha_{EMI9558B/BurleC31034}$	1·019	0·951	1·002	0·954	$\alpha_{STL1001/EMI9558B}$	1·087
$\alpha_{EMI9558B/STL1001}$	1·020	0·980	1·009	0·996	$\alpha_{STL1001/BurleC31034}$	0·994

Modelling the system parameters — methods of solution

We have calculated a simultaneous solution to the photometric data described above and the velocity curves. We have used both the DC and light-and-velocity-curve (LC) programs of the 2003 version of the Wilson–Devinney code[14,15], where the radiative functions used are based on Kurucz's stellar-atmosphere models. This allows us to model giants or supergiants in addition to main-sequence stars. Hence the use of this version of the WD program is more suitable in the OW Gem case. On the other hand, in the 2003 version the previous effective-wavelength characterization of the pass-bands was replaced by integration over the actual pass-bands of the standard photometric systems. Twenty-five standard bands are available in the code. Five of our bands — *UBVRI* used in the analyses — adjust well for the $UBVR_CI_C$ standard bands defined in the program. Unfortunately, our *h,c* bands do not have equivalents among the standard ones, so we decided to omit them in our analyses.

The Levenberg–Marquardt algorithm used in the 2003 version of the WD program together with a properly selected value of the λ parameter (see, *e.g.*, Kallrath & Milone[16]; Kallrath *et al.*[17]) usually allows finding a solution in the parameter space even when a large group of free parameters for simultaneous iterations is used. However, the OW Gem orbit is characterized by high eccentricity, which amplifies correlations between the parameters. We have used the method of multiple subsets (MMS) recommended by Wilson & Biermann[18] for dealing with this problem. The method relies on disposing of the strongest correlation through separation of the most correlated parameters into different groups. In our case the groups have included two (seldom three) weakly correlated parameters. The use of the method does not significantly extend the time of calculation, but a problem appears in the form of unrealistically small values of the errors. We have obtained more plausible values of the errors by executing additional iterations with all the free parameters simultaneously. The solution was calculated with the aid of a program we wrote for 'semi-automatic' iteration, which made it possible to keep control over the result of each iteration by visual inspection of the evolution of parameters, errors, and residuals on the computer screen as a function of the iteration number. The criterion for a proper solution was set as obtaining the minimum sum of squared residuals over the domain of parameter space as well as (in some cases) visual inspection of the shape of the χ^2 surface.

Fixed parameters

OW Gem's photometric behaviour does not indicate the possibility of the occurrence of spots on the component's surface, so the system was treated as unspotted. Synchronous rotation has been assumed. It is impossible to find the temperatures of both the components in OW Gem's case using only the WD code, so the temperature T_1 of the hot component was fixed at 7100 K according to the F2 Ib–II classification[5]. The values of both stars' temperatures are below 7200 K, which is a theoretical upper threshold for convective envelopes. Such cases are characterized[19] by the theoretical values of a bolometric albedo $A = 0{\cdot}5$ and the exponent in the bolometric gravity brightening $g = 0{\cdot}32$. Any third-light contribution has been neglected. The derivatives of the orbital period and ω were assumed to be equal to zero. A nonlinear, logarithmic limb-darkening law was used. The theoretical coefficients *x,y* have been calculated according to the Van Hamme[20] tables for: $T_1 = 7100$K, $T_2 = 4950$K, $\log g_1 = 2{\cdot}2$, $\log g_2 = 2{\cdot}0$.

The free parameters' initial values

The initial values of $a \sin i = 1052\ R_\odot$, $V_\gamma = -5{\cdot}25$ km s^{-1}, and $q = M_2/M_1 = 0{\cdot}676$ have been adopted from Griffin & Duquennoy[5]. The values of the surface linear potentials of the stars were initially estimated as $\Omega_1 = 35$, $\Omega_2 = 24$ from the value of the radii $R_1 = 30\ R_\odot$, $R_2 = 35\ R_\odot$, and mass ratio. A temperature $T_2 = 4950$ K and an inclination $i = 89°$ have been taken from our simple model[10]. The large orbital eccentricity enables an estimation of the parameters e and ω when we have information about the time separation of the eclipses and about the duration of the phenomena. From there we have found the initial values of $e = 0{\cdot}5183$ and $\omega = 144{\cdot}04°$ using formulae 4·4·60 and 4·4·61 from Kallrath & Milone[16]. The parameter ϕ_0 was treated differently. This parameter is connected with the manner of the orbital solution and it is only a formal parameter of the WD code, where for circular orbits the phase of periastron is equal to 0·0 for $\omega = 90°$ by definition. If we want to get the actual value of the periastron phase with the WD code for an eccentric orbit, we have to take into account the value of the phase correction ϕ_0. This parameter can briefly be defined as the difference between the actual periastron phase for an eccentric orbit and the periastron phase that would be for a circular orbit with the same periastron longitude adopted. Three parameters, eccentricity e, periastron longitude ω, and ϕ_0, are dependent upon each other. For that reason parameter ϕ_0 cannot be treated directly as the other free parameters, but a search has to be made through a wide area of that parameter's space in order to find the global minimum and not just a local one.

The orbital-geometry solution

The solution was carried out in two basic steps. Both stages of the calculations were carried out for many values of ϕ_0 according to the procedure described below. The first stage aimed to determine the geometry of the orbit through estimation of the parameters: $a \sin i$, e, ω, V_γ, q, and L_1. The pass-band luminosity L_1 of the primary component is defined in detail in the manual of the 2003 version of the WD program. The changes in two parameters, the argument of periastron ω and the eccentricity e, have a strong influence on the light curves as well as on the velocity curves: the duration of both eclipses and their phase shift depend very strongly on them. This timing constraint puts a strong limitation on the values of e and ω. The WD program solves for the velocity curve and many light curves simultaneously[21], and this advantage has been used in the first part of the first stage, where the values of the parameters e, ω, L_1 were corrected. When a convergence was achieved, then in the second part of the first stage, the parameters $a \sin i$, V_γ, and q, which depend only on the velocity curve, were corrected, then both steps were repeated.

The second stage has the purpose of determining first of all the orbital inclination i together with the parameters depending only on the light curves: T_2, Ω_1, Ω_2, and L_1. Ω_1 and Ω_2 are linear functions of the true potentials on the equipotential surfaces of the stars[14]. A black-body approximation was used. In order to make the geometric solutions independent of the input values of the radii and the inclination, the first stage of the solution (searching for $a \sin i$, e, ω, V_γ, q) was carried out again. A further repetition of both stages did not show changes in the size of the errors, so it was considered that for the current value of ϕ_0 the final solution for the orbital parameters had been achieved. Each value of ϕ_0 relates to one value of the sum of weighted squared residuals ($\chi^2 = \Sigma(W \cdot Res^2)$), which

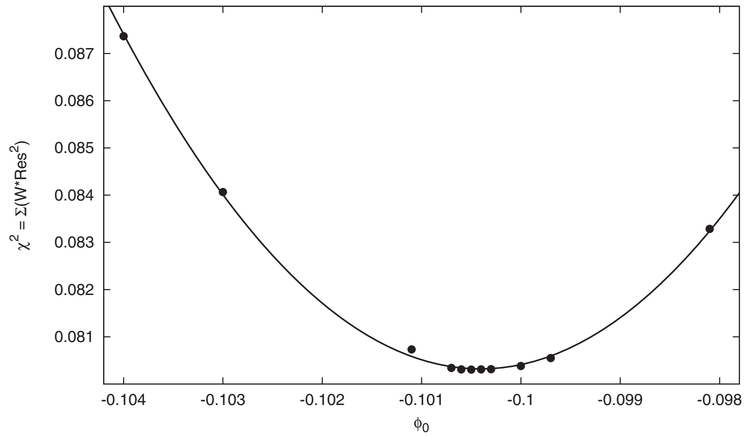

FIG. 3

Quality of fit obtained for different values of the ϕ_0 parameter. A polynomial was fitted and the minimum of the function was found.

represents the quality of the fit obtained. A second-order polynomial was fitted for $\chi^2(\phi_0)$ and the minimum of the function was found (Fig. 3). This way the final set of orbital parameters has been found and these values are compared with previous solutions in Table XII. The radial-velocity curves computed from our parameters (Fig. 4) differ slightly from those published earlier by Griffin & Duquennoy[5] and Terrell *et al.*[8]. A good timing of both minima gives better values for ω and e in comparison to previous solutions. However, these parameters force small changes in $a \sin i$, V_γ, and q in comparison to free fitting.

TABLE XII

Orbital parameters of OW Gem

	This work	*Griffin 2007***	*Terrell et al.*[8]	*G&D*[5]	*unit*
P	1258·58	1259·30	1258·59	1260·00	days
$a \sin i$	1030·0 ± 10·0	1035·9 ± 11·8	1044·4 ± 8·8	1052·0 ± 18·7	$R_\odot$
i	89·040 ± 0·028	—	89·09 ± 0·02	89·0 ± 0·1	degrees
V_γ	−5·10 ± 0·10	−5·21 ± 0·06	−5·18 ± 0·14	−5·25 ± 0·16	km s^{-1}
q	0·692 ± 0·011	0·687 ± 0·013	0·664 ± 0·002	0·676 ± 0·014	—
e	0·5286 ± 0·0006	0·5233 ± 0·028	0·51718 ± 0·00002	0·515 ± 0·011	—
ω	140·73 ± 0·12	140·3 ± 0·5	143·08 ± 0·02	140·2 ± 1·3	degrees
ϕ_0	−0·1004 ± 0·0001	—	−0·1030 ± 0·0001	—	—
$\Delta\phi_{II}$	0·23250	0·23655	0·23207	0·24123	—
$\delta\phi_{II}$*	+0·10	−5·00	+0·64	−10·89	days

* The differences between the observed and calculated phase shift of the secondary eclipse
** Personal communication

The final solution must give a formally larger standard deviation of the observational points than a free fitting to the radial velocities only, without any timing constraints. This is in contradiction to the solution by Terrell *et al.*[8], who obtained unrealistically low errors for e and ω, as pointed out by Griffin[22]. Moreover, a detailed inspection of Figure 1 in Terrell *et al.*[8] shows that almost all the points

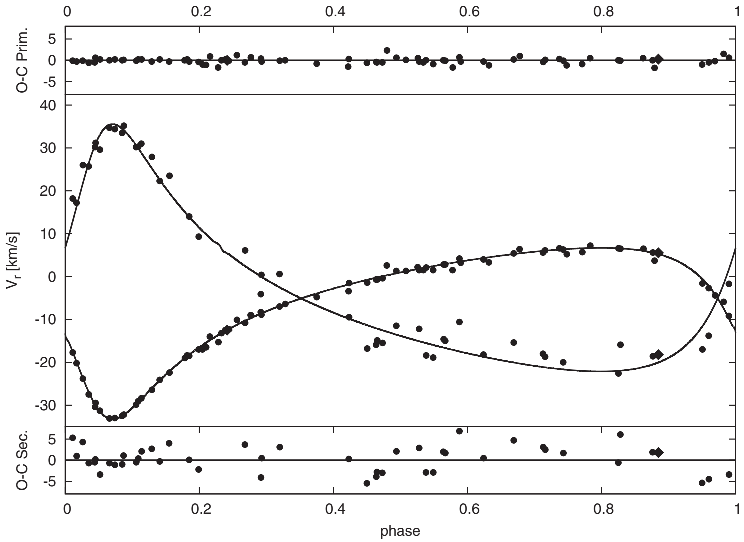

FIG. 4

Synthetic radial-velocity curves (lines) fitted to the Griffin & Duquennoy data represented by circles and our three points represented by diamonds. At the top and the bottom the residuals (O–C) are shown for the primary and secondary components, respectively, which demonstrate the quality of the fit.

which lie on the descending branch of the secondary minimum are above their synthetic light curves, while the points which lie on the ascending branch are located below their model. This disagreement in the timing has been noted previously by Griffin[22]. Values of e and ω obtained by Terrell *et al.*[8] differ significantly from other results and we have inferred that their solutions must have landed in a local minimum as a consequence of the use of an incorrect ϕ_0 value. Values of e and ω which Griffin (personal communication) has obtained with his new radial velocities are close to our values and consistent in size of errors. However, because of a lack of photometry in those solutions, the values of e and ω cannot give a good timing of the secondary minimum. The differences between the observed phase shift of the secondary eclipse $\Delta\phi_{\rm II} = 0{\cdot}23258$ and that derived from the orbital solution (Table XII) are $+2{\cdot}4$ hours for our solution, $+15{\cdot}4$ hours for Terrell *et al.*[8], and minus a few days for orbital parameters derived from radial velocities only. Given that we have obtained such a good timing from our analysis, we hope that our parameters are close to the true ones with realistic errors.

System components: solution for physical parameters

Knowledge of the orbital geometry allows us to proceed with a part of the modelling leading to exact information on the physical parameters of the components, *i.e.*, temperatures, radii, masses, and luminosities. At this stage of the solution a stellar-atmospheres approach has replaced the previous black-body approximation. The temperature of the hot component has had to be adopted. It is not possible to determine the temperatures of both components of OW Gem if we do not have at our disposal very-good-quality photometry (accuracy better than $0^m \cdot 01$) of both eclipses reaching deep into the ultraviolet and far-infrared domains. Thus we can obtain the ratio of temperatures only. Griffin & Duquennoy[5] have classified the hot-component spectral class as F2 Ib–II; that, according to Straizys & Kuriliene's spectral-class–effective-temperature classification[23], should have an effective temperature $T_1 = 7100$K. This value of the hot-component temperature was adopted in our calculations, the same as in other papers. However, we have compared the OW Gem spectrum with spectra of neighbouring spectral-class standards, and we have found that the accuracy of this classification is of the order of one subclass. By interpolation within the Straizys & Kuriliene spectral-class–effective-temperature classification we have estimated the uncertainty on the temperature of the hot component as $T_1 = 7100^{+150}_{-200}$ K, and for the corresponding temperature of the cool component *via* the temperature ratio as $T_2 = 4975^{+110}_{-140}$ K (see Table XIII). The error of our T_2 value given in the table is the error of the fit to the observational data and not the error of parameter determination.

The orbital inclination and radius of the primary component R_1 have been found by a search of the 'whole' χ^2 surface for their possible values. The method relies on the execution of many fits where the two desired parameters are first fixed and then they are changed in the following runs with the assigned resolution. Such a map of the χ^2 function usually allows one to find a global minimum, and so correct values of the parameters. The surface $\chi^2(i,R_1)$ has been obtained by calculating a grid of 130 values of χ^2 (Fig. 5), to which a 3rd-order polynomial was fitted. Later on, the minimum of the polynomial was found. In this way, the inclination i and the radius of the primary component R_1 have been determined, and subsequently the values of the radius of the secondary component R_2, its temperature T_2, and the luminosity L_2. The resultant value of the inclination is shown in Table XII. Table XIII presents our physical parameters in comparison with those of other authors. Fig. 6 demonstrates the quality of the fit to U and V light curves, and Fig. 7 demonstrates the variations in the $(B-R)$ colour index during the primary and the secondary eclipses of OW Gem.

TABLE XIII

A comparison of the physical parameters obtained by us, Terrell et al., and Griffin & Duquennoy

	This work	*Terrell et al.*[8]	*G&D*[5]	*unit*
T_1*	7100	7100	7100	*K*
T_2	4975 ± 20	4917 ± 110	4800	*K*
Ω_1	33·34 ± 0·21	35·15 ± 0·03	—	—
Ω_2	24·17 ± 0·15	24·19 ± 0·01	—	—
R_1	32·32 ± 0·22	30·9 ± 0·3	30 ± 3	$R_\odot$
R_2	32·56 ± 0·23	31·7 ± 0·3	35 ± 3	$R_\odot$
M_1	5·49 ± 0·21	5·8 ± 0·2	5·9 ± 0·3	$M_\odot$
M_2	3·80 ± 0·16	3·9 ± 0·1	4·0 ± 0·2	$M_\odot$
$(L_1/(L_1+L_2))_U$	0·949 ± 0·011	0·946 ± 0·008	0·945	—
$(L_1/(L_1+L_2))_B$	0·921 ± 0·007	0·924 ± 0·005	0·899	—
$(L_1/(L_1+L_2))_V$	0·851 ± 0·007	0·868 ± 0·006	0·834	—
$(L_1/(L_1+L_2))_R$	0·803 ± 0·008	0·815 ± 0·004	—	—
$(L_1/(L_1+L_2))_I$	0·757 ± 0·009	0·761 ± 0·005	—	—

* Adopted

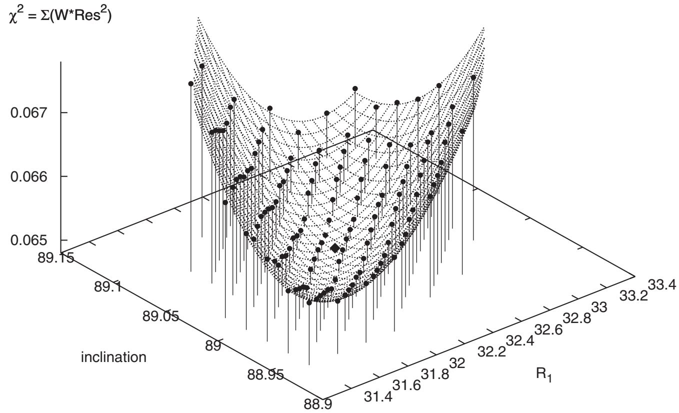

FIG. 5

Adjusting the polynomial $f(i,R_1)$ for the points on the χ^2 surface. The location of the minimum on this surface is denoted by a diamond. The radius of the primary component (R_1) is expressed in solar radius units and the inclination in degrees.

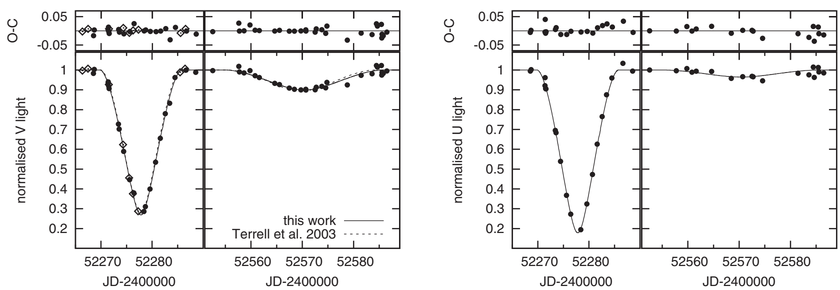

FIG. 6

V (left) and *U* (right) light curves of OW Gem normalized to 1 out of eclipse. The filled circles represent our measurements. The Derekas *et al.*[7] data are shown by diamonds. At the top are placed the (O–C) residuals, which demonstrate the quality of the fit. The dashed line represents the model of Terrell *et al.*[8] where the secondary minimum is shifted about 13 hours with respect to our model (solid line).

OW Gem's spectrum

We used the coudé spectrograph of the 2-m *RCC* telescope at the Rozhen Observatory (Bulgaria) to obtain spectra of OW Gem, with a resolving power of 15000, on 2005 January 20 ($\phi \sim 0{\cdot}88$) and 2006 April 14 ($\phi \sim 0{\cdot}24$). The spectral regions covered were 6620–6825 Å and 6470–6820 Å, respectively. In Fig. 8 the spectra of OW Gem are compared with spectra of HD 164136 (F2 II), HD 75276 (F2 Iab), and HD 159532 (F1 II). The spectrum of HD 164136 (ν Her)

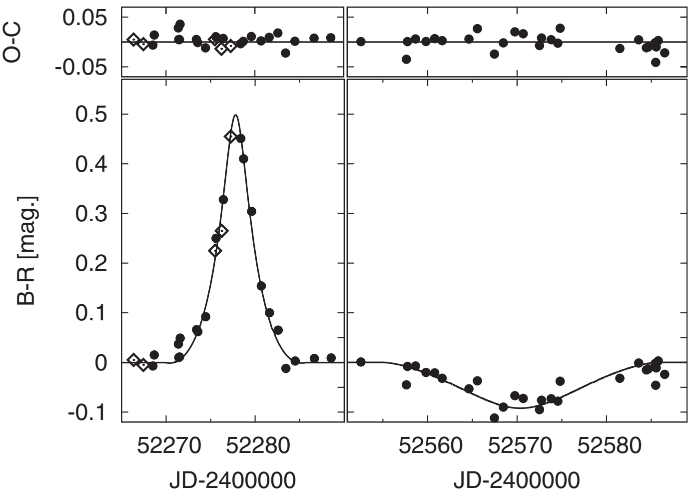

FIG. 7

The *B–R* colour index during the primary and the secondary eclipses of OW Gem. The filled circles represent our measurements. The Derekas *et al.*[7] data are shown by diamonds. At the top are placed the (O–C) residuals, which demonstrate the quality of the fit.

is from the Indo–U.S. library of coudé-feed stellar spectra[24], and the spectra of HD 75276 and HD 159532 are from the UVES library of high-resolution spectra[25]. Additionally, a spectrum of the eruptive variable (and possible stellar merger) V838 Mon (see Tylenda & Soker[26], and references therein), obtained at the Rozhen Observatory with the same resolution as the OW Gem spectra, is shown in Fig. 8.

In both spectra of OW Gem the radial velocities were measured (Table II). In the spectrum on 2006 April 14, obtained during the secondary eclipse, we measured the radial velocity of the primary component only.

Griffin & Duquennoy[5] classified the primary component of OW Gem as an F2 Ib–II star. We do not have enough spectral observations to make a detailed spectral classification of both components. However, in Fig. 8 it is obvious that the lines in the OW Gem spectrum on 2006 April 14, dominated by the primary component, are very similar to those in the F2 Iab spectrum of HD 75276. The most remarkable difference between the spectrum of the OW Gem primary and the spectra of ν Her (noted by Griffin & Duquennoy) and HD 159532 is the rotational velocity, which is $v \sin i = 28$ km s^{-1} for ν Her and $v \sin i = 105$ km s^{-1} for HD 159532 (Snow *et al.*[27]). Based on the 2005 January 20 spectrum only, we cannot say anything about the secondary companion's spectral class.

The above spectral regions were chosen with the aim of checking on the presence of a weak Li I 6708 Å line in the spectrum of the secondary component suggested by Griffin & Duquennoy[5]. Those authors measured an equivalent width of about 10 or 15 mÅ for this lithium line in the composite spectrum. Our 2006 April 14 spectrum (just during the secondary eclipse) is dominated by the primary

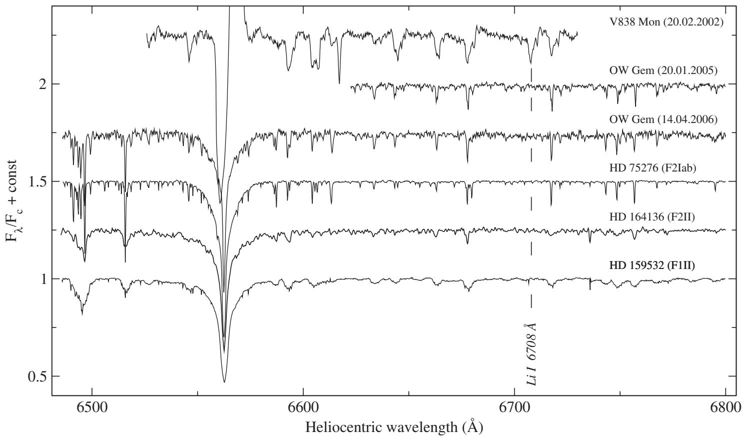

FIG. 8

A comparison of our OW Gem spectra with the spectra of HD 75276, HD 164136 (ν Her), HD 159532, and the possible merger V838 Mon. The spectra of the comparison stars are shifted to the heliocentric wavelengths of the OW Gem spectra. The spectra of HD 75276 and HD 159532 are degraded to the OW Gem spectral resolution. The Hα emission component in the spectrum of V838 Mon is truncated for clarity.

component. If the lithium line is present then it should be detectable in the 2005 January 20 spectrum (about 5 months before the primary eclipse) in which most of the absorptions are double. The quality of our spectra is good enough to identify and measure such weak absorptions. As can be seen in Fig. 8, in both spectra there are several faint features with equivalent widths of the order of 10–15 mÅ in the vicinity of the lithium line. However, none of these faint features disappears in the spectrum on 2006 April 14, as we would expect if the weak lithium line were present only in the secondary spectrum. Furthermore, the radial-velocity measurements show that all of these faint features are far from the expected lithium-line position for both stars. Therefore, we cannot confirm the presence of the Li I 6708 Å absorption line in either the primary-component spectrum or in the secondary-component spectrum.

To explain the unusual evolutionary status of the components, Eggleton[28] suggested that OW Gem is a former triple star in which the F supergiant is a merged remnant of a close sub-binary. He pointed out that it is very difficult to confirm that a particular star is or is not the result of a merger. A merger remnant could be an unusually rapidly rotating star[28] but it is obvious from Fig. 8 that this is not the case for the OW Gem primary component. After the merger (if that is what happened), a relatively strong Li I 6708 Å absorption line is present in the spectrum of V838 Mon (Fig. 8). As was noted above, this line is missing in the spectrum of the OW Gem primary component. Hence, we can consider the slow rotation and the lithium-line absence only as an indication that, if the primary component in OW Gem *is* a merger remnant, the merger event took place a long time ago.

Conclusions

The full set of orbital and physical parameters for OW Gem has been obtained with independently collected photometric data. Slightly better values of the parameters $e = 0 \cdot 5286$ and $\omega = 140° \cdot 73$ were obtained in our work in comparison to the previous analysis. Both the eccentricity and the periastron argument calculated in this paper give a better fit to the observations, especially to the best timing of the secondary minimum. This was made possible by using new data including three secondary minima. Our results underline the advantage of the simultaneous analysis of light and velocity curves. The new model has supplied a better estimate of the radii of the OW Gem components, by using good-quality multicolour photometry as well as a more reliable temperature ratio of the stars. However, we were not able to change significantly the values of the masses and the mass ratio of the components, confirming once again the unclear evolutionary status of the system, in which two massive stars with considerably different masses ($\sim 6\,M_\odot$ and $\sim 4\,M_\odot$) are placed in a very brief stage of supergiant evolution. A confrontation with the solar-metallicity evolutionary tracks from Girardi *et al.*[29] is presented in Fig. 9. The less-massive star is about 200 million years old. The more massive star is at least 100 million years evolutionarily younger. During this time it should have finished its evolution as a supergiant. The current evolutionary status of the system stands in contradiction to evolutionary models of the stars in binary systems and cannot be explained either by loss or transfer of mass[5]. In order to explain the observed parameters of OW Gem, we should perhaps revise the theory of stellar evolution. Another possibility is that a merger took place. Eggleton[28] has suggested that a triple system (close sub-binary $4\,M_\odot + 2\,M_\odot$ with a short period of about 2 days and the third component $4\,M_\odot$ on a wide orbit) can turn into a binary system. The lack of lithium-line detection in the present spectra is an indication that the merger event would have had to have taken place a long time ago.

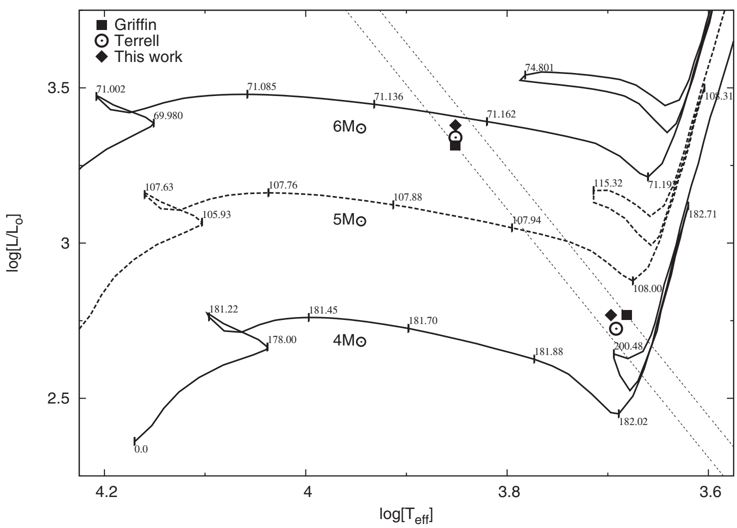

FIG. 9

Both OW Gem components shown on the Girardi *et al.*[28] solar-metallicity evolutionary tracks. The diagonal dashed lines are lines of constant radius for 30 $R_\odot$ and 35 $R_\odot$. The evolution time in millions of years is denoted by short vertical lines with appropriate numerical values.

It seems that future attempts at modelling the optical light and velocity curves will not result in significant changes in our knowledge about the physical parameters of the system. Nevertheless, the status of OW Gem still remains unexplained and an important case for understanding the evolution of binary stars. In particular, the depths of the primary and secondary eclipses deep in the ultraviolet and far-infrared can give the best direct calibration of surface temperature for F and G supergiants. For binary stars it is at present possible to obtain angular separations of a few milliarcseconds by optical interferometric observations (see, *e.g.*, ref. 30), and thus gives the opportunity for verification of the distance to the OW Gem system.

Acknowledgements

We especially thank Professor R. F. Griffin — who, together with Dr A. Duquennoy, has discovered the unusually interesting nature of the OW Gem system — for his permission to use the radial-velocity data collected by them since their paper in 1993. We are also deeply grateful to him for helpful and kind discussions. We are very grateful to Dr B. Roukema for his language corrections and for N. Biernaczyk, S. Frąckowiak, P. Oster, K. Rumiński, E. Świerczyński, M. Więcek, P. Wirkus, K. Wojtkowska, M. Wojtkowski, and P. Wychudzki for their contribution to collection of photometric data. This study was supported by MNiSW grant No. N203 018 32/2338, grant UMK No. 340-A, and partly supported by the Polish–Bulgarian Academies of Science exchange.

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REVIEWS

Space is a Funny Place, by Colin Pillinger (The Cartoon Museum, London, and through Professor C. Pillinger, PSSRI, Open University), 2007. Pp. 211, 25 × 18·5 cm. Price £15 (+ £2·50 postage & packing) (hardbound; ISBN 978 0 953 72639 4).

The process of getting instrumentation into space and onto other Solar System bodies is fraught with problems and often carries significant risk of failure. Many of us working in space science and astronomy have found that humour is an important defence, helping us deal with difficult periods in our projects. Colin Pillinger clearly subscribes to this view. His excellent *Mars in their Eyes* arose from an exhibition of Mars-inspired cartoons at the Cartoon Museum in London, serving a dual purpose as a catalogue for the event. *Space is a Funny Place* is a second and similar publication by Colin, collecting together some delightful cartoons with links to space and astronomy. However, this new book is much more ambitious than the first, linking the cartoons into a commentary on the first 50 years (and more) of space exploration.

Compared to a raft of publications timed to take advantage of the anniversary of the launch of *Sputnik*, this is one of the most interesting and thought provoking. For those not familiar with the story, the text is an excellent, compact, and accessible introduction to some highlights of the last 50 years, including the race to the Moon, the exploration of Mars, and the search for extra-terrestrial life. For the expert in these matters this book is a wry, offbeat, and amusing re-visitation of parts of our history. Colin is not afraid to expose his personal disappointments, including the failure to communicate with *Beagle 2* after its attempted landing on Mars in 2003 December, with some very funny, but bittersweet, cartoons on that subject.

You will not find this volume in your local bookshop and, unless you live in London, you will have to go to some trouble to obtain a copy by contacting Colin directly. However, it will be well worth the effort. It will lighten your life. — MARTIN BARSTOW.

Star Maps: History, Artistry, and Cartography, by Nick Kanas (Springer Praxis Publishing, Heidelberg), 2007. Pp. 382, 24 × 16·5 cm. Price £19·50/ $34·95 (paperback; ISBN 978 0 387 71668 8).

San Francisco Bay Area author, Nick Kanas, is an avid collector of celestial maps and charts. He has taken his many years of collecting expertise and condensed it into a marvellous book on this fascinating aspect of enjoying the night sky. His book is filled with 207 colour and black-and-white images of celestial maps from all ages. The surviving celestial maps from Mesopotamia, Egypt, China, India, and other ancient cultures influenced Greek, Roman, and Islamic sky watchers who in turn produced their own representations of the night sky. Once knowledge of these earlier maps became known to Renaissance European cartographers, the art and craft of representing the night sky on paper reached a high point of refinement. Their maps have become highly prized collectors' items, not only for showing the heavens, but for their artistry as well.

From the opening chapter, devoted to explaining the difference between celestial and cosmological maps, to the last chapter covering modern maps and atlases, you will find something fascinating on almost every page. The author describes the maps for each period and writes about how particular map styles were developed over the years and the relationships between them. Where a map or chart is illustrated, he discusses details shown on the map and gives you a good understanding of the map's place in cartographic history. As you progress through the ages, you can see how one age influences the work of later eras. I found this to be a very fascinating aspect of this comprehensive work. I have read a number of books on the history of celestial cartography, but none with the same depth and wealth of information on this important part of the history of astronomy.

One of the appendices lists celestial cartographers in alphabetical order and includes information on the works each individual produced. This is certainly a very handy part of this book. The first appendix gives you tips on collecting celestial maps and what pitfalls to avoid.

Mr. Kanas presents a vast and valuable body of knowledge on this subject and has done so in a lucid manner that I found very easy to follow and a real joy to read. Even though the small size of the book meant that images of the maps would be small, they are reproduced to such a fine point that details on all of them remain easily readable. I highly recommend this book to students of the history of astronomy or anyone interested in observing the night sky. — ROBERT A. GARFINKLE.

The Astronomical Almanac for the Year 2009 (The Stationery Office, London) 2009. Pp. 580, 26 × 18 cm. Price £40·00 (hardback; ISBN 978 0 11 887342 0).

The 2009 edition of *The Astronomical Almanac* sees the implementation of the resolutions passed at the 2006 General Assembly of the International Astronomical Union in Prague. In particular, these include the recommendations of the IAU Working Group on Precession and the Ecliptic. This completes the process of implementing relevant IAU resolutions from the 2000 and 2003 General Assemblies which have been incorporated into the editions of 2006, 2007, and 2008. Those resolutions now give the user access to a consistent set of well-defined, high-precision precession, nutation, and sidereal-time/Earth-rotation-angle algorithms enabling calculations, both CIO and equinox-based, to be performed at a precision of better than a tenth of a milliarcsecond. Section B provides detailed information on the practical implementation of these resolutions: the use of CIO-based intermediate places and equinox-based apparent places to generate conventional hour angles and declinations.

Much of the content is similar to previous years. Sections covering 'Phenomena', 'Time Scales & Coordinate Systems', 'Sun', 'Moon', 'Planets and Pluto', 'Minor Planets', 'Comets', as well as reference material on 'Stars and Stellar Systems' and 'Observatories' are provided. Indeed, the reference material in Section H has undergone some expansion and improvement. This edition has also attempted to address the thorny issue of whether Pluto is planet or not. This has been circumvented to some extent by referring to "The Planets and Pluto"!

It is worth noting that this book is a joint publication of the Astronomical Applications Department of the US Naval Observatory and HM Nautical Almanac Office, now part of the UK Hydrographic Office in Taunton. Let us hope that HMNAO has now found a more secure home in which it can provide the astronomical community with the reference almanac it has come to expect.

The Web companion to this publication, *The Astronomical Almanac Online*, has seen the deployment of the latest improved version of the site covering information for 2009. This is mirrored at both http://asa.usno.navy.mil and http://asa.hmnao.com. Throughout the book, the reader will find references to auxiliary material which may be found on those web sites.

Inflation has impacted on the cover price of this book. It is now £40·00. However, for those who require the information it contains or are writing software that they need to verify, this book is still a valuable addition to the astronomer's bookshelf. — DAVID STICKLAND.

A Walk through the Southern Sky: A Guide to Stars and Constellations and their Legends, 2nd Edition, by M. D. Heifetz & W. Tirion (Cambridge University Press), 2007. Pp. 109, 24·5 × 19 cm. Price £12·99/$22·99 (paperback; ISBN 978 0 521 68945 8).

Ideal for the back-garden star-gazer (or wherever s/he can get far enough away from light pollution to see down to 4th or 5th magnitude), this handbook helps one navigate through the visible stars and constellations without needing a telescope, and provides sufficient background information and historical anecdotes to whet the appetite of all but the most persistent armchair astronomer. It does not replace *Norton* or even compete with it; it is introductory where *Norton* assumes some background knowledge, simplifies the constellations by omitting the faint members, and makes the heavens look deceptively easy to recognize, whereas in *Norton* the charts can appear rather 'busy' and confusing to the beginner.

A few years ago I reviewed in these pages (**125**, 49, 2005) the third edition of the earlier of the Heifetz–Tirion 'Sky Walks' (*A Walk Through the Heavens*), which was angled at recognizing the northern sky but with some penetration south of the equator, and I kept the northern counterpart beside me as I perused the volume for the southern sky. The latter might be described as a companion volume to the northern one; however, apart from the central section being devoted to star maps and constellations not covered in the northern one, the introduction and general texts on stellar distances and magnitudes, visual acuity tests, planet ephemerides, and constellation lists are very similar if not fully identical, while the substantial section retelling the legends of the constellations is comprehensive in the southern volume but limited to northern ones in the earlier volume. To that extent, someone wanting the full information but living in the north would have to buy both volumes and knowingly purchase duplicate material. The authors could be criticized for having two bites at the same cherry, and had the northern-based material from the first volume now been incorporated into a comprehensive 'Walk Through the Skies' I feel that the resulting product might be a more satisfactory contribution to one's library than the pair of separate volumes. As it is, a few of the star charts in the southern-sky volume are drawn for a horizon of 25° S and portray constellations northwards to some 65° N.

There is definitely a niche in general literature for books like these, and the authors provide exactly what is needed. The descriptive charts are plentiful, uncomplicated and clearly labelled, and anticipate most queries that could arise during casual viewing. If I have a problem with the presentation of the charts it is their tendency to oversimplify; the stars are depicted with a lack of extinction right down to the horizon, the latter being conveniently shaped so as not to occult any of the objects that one needs to locate. The same difficulties that I cited in the northern volume are encountered here: describing a line connecting two stars as a "gentle curve" but without specifying its direction of bend can be ambiguous, and may not be very easy to follow when translating from flat chart to celestial sphere.

Each chart is labelled with direction, date, and time, and although the horizon is represented attractively as silhouettes of objects like trees and fences, they are not the same ones even if the direction of view is the same. I thought the authors had missed an opportunity there, since by creating a fixed landscape with features that relate to geographical direction the student might develop the habit of associating real features with the local compass points and noticing how the stars appear to move relative to them.

One practical point that both volumes have missed is advice on how to read a book in the dark. Being armed appropriately can make all the difference between persevering successfully and giving up after discouraging attempts.

As my review of the earlier volume mentioned, couching the star legends in language that has been stripped of all references to original violence or ill-will yields a particularly insipid product, a kind of babes' milk. Many of the legends actually grew out of wars and rumours of wars among the gods, and I was slightly irritated by texts so dumbed-down that even children would find them boring. However, intercomparing tales from different cultures and races did make for a little more interesting reading.

The textual material that also appeared previously in the northern volume has been reformatted and presented here in a more attractive font, and some of the errors (noted by more than one reviewer) have been corrected. The only serious gaffe that I noticed concerns Pluto, which is everywhere referred to as a "Planet". Few of the thinking public were unaware of the 2006 debacle that took Pluto down a notch, and an oversight of that nature, though trivial in itself and possibly deliberate, can colour one's judgement of the reliability of the whole. Even if explaining the differences between a planet and a dwarf planet may not lie within the compass of the book, it would be worth adding enough words to get the facts right.

Of course, under the sort of really clear skies depicted here, when stars can be seen in their true colours right down to the horizon, even the familiar constellations are so thickly peppered with faint members as to make them hard to identify. Charts such as these are no real replacement for learning one's own way about by frequent viewing, but they are a good incentive to get out of that armchair and have a good try. For those who normally see the southern skies, this volume is an excellent way to get started. — ELIZABETH GRIFFIN.

Making Beautiful Deep-Sky Images, by G. Parker (Springer, Heidelberg), 2007. Pp. 192, 23·5 × 15·5 cm. Price £19·50/$34·95/€26·95 (paperback; ISBN 978 0 387 71352 6).

Whether one participates in on-line chat rooms or various other on-line forums, something that becomes quite obvious is the tremendous interest by amateur astronomers in pursuing astrophotography. This venture into previously sacred ground has been facilitated by the digital age involving camera equipment, where not only are results available for immediate review but their appearance as attachments to forum messages allows critical feedback, exchange of ideas and tips, as well as suggestions for better techniques during image acquisition and processing.

A recent trend has seen the publication of books on the field of astrophotography by well-recognized astrophotographers including Rob Gendler, Jerry Lodriguss, Ron Wodaski, Thierry Legault, Stefan Seip, and others. The latest entry to this list is the book by Greg Parker, Professor of Photonics at the University of Southampton, who is a self-confessed 'newbie' in the field ("I started imaging literally one year ago" and "the bulk [of the images] will have been taken within 18 months of starting the hobby", pp. 170–171).

Although I had doubts about a book dealing with the potentially complex field of astrophotography written by someone with only 12–18 months' experience, I must confess I was generally pleased. The author suggests that "with so little 'real' imaging experience", it may seem odd that he is writing a book on the topic but counters that minor and silly issues related to the field are now both fresh in his mind and available for transmission to newcomers, which probably would not have been the case had he waited a few more years prior to embarking on this project. Personally, I would have to disagree with this suggestion, for the greatest contribution that any author can make when writing a book and passing on knowledge is his experience, which, presumably, will document lots of pitfalls, errors, solutions, and tricks related to technique so that the newcomer will not have to painstakingly reinvent the wheel himself.

Parker's book is comprised of fourteen chapters spanning 155 pages and two appendices which make up a further six pages. The first chapter is simply a very extensive documentation of problems with his equipment setup, which I believe does a lot more damage than good to the reader. Aside from describing problems that are *very* specific to his particular and somewhat eccentric set-up (*e.g.*, Hyperstar and the alt-az mount leading to focussing and field-rotation problems), there is too much detail, which simply distracts the user (*e.g.*, problems with the NexStar keypad, drilling holes to his mount, soldering a capacitor to his camera's circuit board, *etc.*). To make matters worse, he then describes the sale or removal of some parts in this imaging train and its replacement with new equipment (*e.g.*, the use of a piggy-backed Tak Sky 90), which begs the question as to the real significance of the earlier discussion surrounding the equipment which was eventually removed or relegated to alternative use (*e.g.*, NexStar as a guider for the Tak Sky 90). It is certain that a newcomer will not attempt imaging at $f/1 \cdot 85$ and will inevitably have different equipment, thus making the first chapter of the book of questionable value and perhaps a turn-off. I was very disappointed to note that "Noel Carboni actually processes all of my images nowadays" (p. 9), which should further distance a newcomer looking for advice, suggestions, and tips.

As disappointed as I was with the first chapter, I was delighted with the following three chapters where Parker encourages the newcomer to learn their local sky in relation to light pollution, seeing, and transparency; his discussion of the pros and cons of telescope designs (*e.g.*, refractor *versus* SCT), sub-exposure and total exposure times, sky-glow limits and narrowband imaging; and computer issues related to data acquisition and processing. However, there are some points that deserve discussion. Parker notes that refractors can be cumbersome to use for visual work when the objects are high in the sky. This is not true since a 90-degree mirror diagonal solves any such problem. Also, the author is concerned about the performance of (SBIG) CCD cameras which have the unique ability both to image and guide simultaneously in narrowbands such as Hα; but this is something that can be circumvented rather easily by binning (*e.g.*, 2×2 or 3×3) and by using, if necessary, slightly longer guide times, which is a trivial matter with any high-quality mount. Further, these cameras also have large guide chips based on the TC-237 chip, which makes the identification of a suitably bright guide star painless. Finally, it would have been nice to see an alternative formula for the computation of sampling (arc-seconds per pixel) which does not involve trigonometric functions, for the sake of simplicity and without any sacrifice of accuracy.

Chapter 6 is the reader's first real exposure to astrophotography, for the author does a very good job in identifying candidate objects — wide- and narrow-field, for each of the four seasons — which are not very challenging but ideal for providing a good basis for the reader's first attempts at deep-sky work.

Chapter 7 is effectively a continuation of the earlier chapter but it concentrates on the physical aspect of preparing for actual imaging of the deep-sky (such as calibration, test exposures, guide settings, *etc.*). However, there are some disturbing omissions: FWHM is immediately mentioned but there is no discussion as to what it means and how seeing may affect its measurement from exposure to exposure; a comment is made about programming the evening's imaging session (*e.g.*, 100 five-minute sub-exposures) but there is no discussion of monitoring the environmental temperature ("Now all you need do is go away", p. 59) and how it is certain to impact on focussing during the evening (a temperature change of two degrees can easily change the proper focal point); and the discussion surrounding the 'proper' exposure for our target mentions using a cursor to look at specific pixel values when, in reality, our best friend is the histogram! Perhaps my greatest disappointment in the whole book lies in this chapter and the complete absence of discussion surrounding flat frames and, especially, dark frames so that thermal noise, differential sensitivity between pixels, and vignetting can be corrected.

Chapter 8 is dedicated to the Hyperstar imaging system which the author uses, and it is, in my opinion, unfortunate that a complete chapter was dedicated to such an infrequently used (Fastar) configuration, which has been discontinued by the manufacturer! As noted earlier, the Hyperstar system is also associated with indirect problems and challenges, and there is no rationale for its inclusion in a book aimed at the newcomer and their desires to pursue beautiful images of the deep sky. For example, the author admits that with the Hyperstar in place, one must focus within an accuracy which is less than one-tenth (!) the width of a human hair.

Chapter 9 outlines a typical set-up where a wide-field refractor is piggy-backed onto a longer-focal-length instrument so that the main scope can be used on the intended target with the other scope being used as a guider. Although the Hyperstar system is mentioned yet again, the general principles remain the same.

Chapter 10 is dedicated to image processing, and the author does confess a lack of experience and furthermore mentions that all but one of the sample images in the book have been processed by someone else. What is frustrating is the discussion of the automated tools which are available *via* Noel Carboni's PHOTOSHOP. These pre-programmed utilities not only hinder one's learning process in relation to image processing but they can be misleading. For example, mention is made of the fact that an artificial flat can be produced from the image files using PHOTOSHOP and one of Carboni's utilities — this is most unfortunate since good technique involves taking real flats either at the start or the end of the imaging session, with artificial flats being used only as a last resort when flats were not taken for some unexpected reason (*e.g.*, the sudden arrival of clouds or rain causing an immediate termination of the session, which can be addressed anyway by using dome flats). The chapter continues with a process flow and provides a link to a sample file — unfortunately in JPG format! — which the author makes available and is used in the step-by-step description for bringing out faint nebulosity. Praise is due for the final section of this chapter, which concentrates on the construction of mosaics, and actual sample images (again in JPG format!) are provided to complement the text. In both cases where step-by-step instructions are given, the author provides sample screen images to support the text further.

Chapter 11 can be summarized with the well-known expression "One picture is worth a thousand words", for over 40 stunning colour photographs are provided covering the complete spectrum of deep-sky objects with descriptive text supplementing each photo. It is unfortunate that many photos with associated text often take up only half of each page when the photos could have been enlarged and/or additional images could have been presented.

Chapter 12 was a pleasant surprise, for it discusses how one can take matters a step further and be different from the mainstream. Parker describes the use of more powerful equipment provided that the financial resources are available, the creation of mosaics for high-resolution results involving large deep-sky objects, the pursuit of less-recognized and/or ignored objects, supernova hunting, and asteroid searches.

Something which the reader will notice immediately while reading Parker's book is the frequent reference to on-line (internet) resources. Although I was disappointed that issues such as polar alignment and collimation were only briefly addressed and the reader was redirected to an on-line reference — it would have been nice to have had much more on these critical issues within the book — it generally has a beneficial effect that is reflected in Chapter 13 where various on-line references are presented. Similarly, Chapter 14 provides a list of 14 books which give a broad perspective on astronomy and can open doors to the newcomer with general information on astronomy as well as potential targets.

The final two sections of the book are welcome appendices where the author describes the concept of apparent diameter, which is most important for any imaging session, and the various catalogues that are available for each group of deep-sky objects (such as clusters, nebulae, *etc.*).

As suggested above and throughout the discussion, the book by Parker is one man's venture into the world of astrophotography. Although there may be too many examples related to personal problems and a zeal for referencing the Hyperstar system, the book provides a good introduction to astrophotography that would be ideally suited to someone contemplating this addictive aspect of amateur astronomy. — ANTHONY AYIOMAMITIS.

David Levy's Guide to Observing Meteor Showers, by D. H. Levy (Cambridge University Press), 2007. Pp. 128, 24·5 × 17 cm. Price £18·99/$26 (paperback; ISBN 978 0 521 69691 3).

Meteor astronomy has made great strides in the past twenty years. A standardized, globally-employed, visual meteor-observing technique, allows amateurs to pool and analyse data statistically to provide credible, professionally-acceptable information on major shower activity regularly now. Automated CCD video systems, mostly run by amateurs, monitor the skies every clear night over parts of the inhabited northern hemisphere particularly, revealing details on minor meteor showers with an accuracy unmatchable visually. Even the 'Cinderella' amateur method of automated radio observing has provided valuable routine data on meteor activity by day and night for over a decade. Professionally, one key improvement has been developing theoretical meteoroid-stream filament models, to allow the first precise predictions of strong meteor activity.

Of these, Levy seems aware of the latter mainly, preferring to advocate casual meteor-counting with the naked-eye, ideally as part of a group, for amateur meteor enthusiasts. His text has little useful advice for those keen to go further and make scientific visual meteor observations, while the video and radio techniques pass unremarked. Using two old sources and some recent radar data, he lists 128 meteor showers he claims should be observable visually (incredibly, without giving a defined radiant position for any). This ignores the worldwide efforts of hundreds of observers and the few dedicated analysts since 1988, who have worked diligently to identify the 35 or so showers and shower complexes that are genuinely visually-observable presently.

The book is at its best in its more autobiographical passages, where Levy's enthusiasm comes through, although much of the discussion has useful and interesting points in places. Sadly, it is let down further by some abysmal editing. Minor errors and oversimplifications abound. SI and non-SI units feature rarely, but almost randomly, and are only once converted (p. 4). The twenty-seven photographs do not always complement the text, and two of the fifteen meteor images are duplicated, in one pair (Figs. 5·3 & 7·1) as positive and negative, carelessly attributed to different people. Misquoting Milton (p. 48) should surely be an offence in a CUP book!

Properly revised, edited, and corrected, with better illustrations (including some explanatory diagrams) and a lower price, this could be a handy meteor-observing primer. At present, Levy has been ill-served by a publisher who even managed to give the copyright date wrongly. — ALASTAIR MCBEATH.

Total Solar Eclipses and How to Observe Them, by M. Mobberley (Springer, Heidelberg), 2007. Pp. 216, 23·5 × 18 cm. Price £19·50/$29·95/€24·95 (paperback; ISBN 978 0 387 69827 4).

As I write this review, an annular eclipse of the Sun has recently occurred over Antarctica and a total eclipse of the Moon is about to take place over Europe, Africa, and the Americas. Consequently, the arrival of this book was well-timed and relevant. Indeed, the total eclipse of the Sun on 2009 July 21/22, which yields a maximum duration of totality in excess of six minutes and forty seconds, the longest since the memorable eclipse of 1991 July 11, may have stimulated the publication of this book.

This book is one of Springer's *Astronomy Observing Guides* which, in my experience, have proved to be useful additions to my astronomical library. The book is split into two sections, one dealing with eclipse mechanisms, statistics, and tracks, and the other covering the planning of eclipse trips and the observing of these events with a wide selection of equipment ranging from the naked eye to personal Hα solar telescopes.

The book contains thirteen chapters, starting with fairly standard material on why eclipses occur and the structure of the solar atmosphere. Following this are chapters dealing with eclipse tracks and the motion of the Moon's shadow, the effects of the lunar limb on predictions, and the wide variety of observational phenomena caused by totality. Eclipse tracks and information for annular and total eclipses are provided for the period 2008 to 2028.

The second half of the book covers topics such as planning trips, observing eclipses safely, photography, videography, and the use of more specialized techniques such as observing with Hα filters. There are some excellent examples of what can be achieved with digital photography, but I do feel that these highly processed images may give people an exaggerated idea of what can be achieved with the average digital camera and telephoto lens. There are interesting sections covering the personal experiences of a number of observers as well as items about those who take their eclipse chasing very seriously indeed.

The cover describes the book as having "all the information an eclipse chaser needs …" and, for the most part, it achieves that aim. My only grumble was that HM Nautical Almanac Office's *Eclipses Online* web site, http://www.eclipse.org.uk, did not make it into the appendix of useful information. All in all, the book is a useful addition to the eclipse chaser's library and will be a good reference for those who are considering their first eclipse trip. The book is well illustrated, gives good practical advice, and at less than £20 is very good value for money. — Steve Bell.

The Far Side of the Moon: A Photographic Guide, by C. J. Byrne (Springer, Heidelberg), 2008. Pp. 232 + CD ROM, 28·5 × 31·5 cm. Price £29·50/ $39·95/€32·95 (hardbound; ISBN 978 0 387 73205 3).

This is the companion to Charles Byrne's earlier book, *Lunar Photographic Atlas of the Near Side of the Moon*. Using images from all five *Lunar Orbiters*, plus the *Clementine*, *Apollo*, *Luna*, *Zond*, and *Nozomi* missions, the atlas covers the whole of the Moon's far side. The images have been skilfully 'cleaned up' to remove some of the photographic defects in the originals, and they are beautifully reproduced as well as being annotated. Each area of the far side is carefully described, and the text fully matches the high standard of the photographs.

There are also other comments of great interest. It has long been known that there are important differences between the Earth-turned and the far hemispheres of the Moon, and Byrne attributes this mainly to an ancient impact producing what he terms the Near Side Megabasin, far larger than the well-known South Pole–Aitken Basin. There have been earlier suggestions of huge basins now difficult to define because of later impacts, but Byrne takes matters further, and produces evidence which would be very hard to refute.

It is clear that the author has undertaken a tremendous amount of research, and the result is a book which must surely remain the standard in the foreseeable future. There is no other work which covers the subject so comprehensively or so well, and it should be included in every astronomical library. — Patrick Moore.

Planets and Life: The Emerging Science of Astrobiology, edited by W. T. Sullivan III & J. A. Baross (Cambridge University Press), 2007. Pp. 604, 25·5 × 19 cm. Price £80/$150 (hardbound; ISBN 978 0 521 82421 7), £40/$75 (paperback; ISBN 978 0 521 53102 3).

Astrobiology is a burgeoning field and many textbooks seem to be appearing at the moment. This volume is best described as "hefty", coming in at around 600 pages. As is now common, the aim is to cover many scientific disciplines, including astronomy, biology, geology, chemistry, and planetary science, and the 28 chapters have a multitude of authors. The target readership ranges from senior undergraduates to experienced scientists wishing to dip into the subject, and the book has emerged from a graduate-certificate programme in astrobiology offered by the University of Washington.

For the reader in a hurry, I have to say this isn't the best textbook I've seen. The chapter authors were encouraged to describe their topics so that most of the material could be understood by anyone, which in some cases has led to a rather rambling discourse. The level of detail sometimes obscures the overall point, or the aim of having all specialist language explained is not met, or some of the case examples seem more suited to a specialized paper. I think this could make it a frustrating read for a student text, where the reader would like to get a clear overall picture in a short amount of time — few students take more than one degree module in astrobiology. On the other hand, it is a good book to dip into for information and be confident that a question will be covered somewhere. Thus it would be good to have on the bookshelf of a working astrobiologist. I also liked the general level of explanation and the use of simple equations to illustrate basic points — not easy to achieve in a multi-disciplinary work.

The book is very well illustrated (in black and white) and the diagrams and tables are informative. There are good long reference lists for each chapter, pretty well up to date for this 2007 volume. Stylistically I found the pages a bit overfilled, including printing down very close to the bottom of the page, and some of the very many footnotes could perhaps have gone into the text or a glossary at the back. The appendices don't quite succeed in giving such basic information all in one place, but I did like the inclusion of 'Astrobiological destinations on planet Earth'!

Overall this might not be my first pick as an introduction to the field, but as an encyclopedia of the subject it works well, and at £40 (in paperback) it is a very good deal. — JANE GREAVES.

Fitness of the Cosmos for Life: Biochemistry and Fine-Tuning, edited by J. D. Barrow, S. C. Morris, S. J. Freeland & C. L. Harper Jr., (Cambridge University Press), 2007. Pp. 501, 25·5 × 18 cm. Price £65/$125 (hardbound; ISBN 978 0 521 87102 0).

This book is a collection of 21 articles from 24 contributors, based on a symposium held at Harvard University in 2003 October to celebrate the 90th anniversary of the publication of L. J. Henderson's *The Fitness of the Environment*. The symposium was made possible through the support of the Templeton Foundation.

The articles, developed since the symposium, address the broad question of the extent to which the Universe is biocentric and fine tuned for life, mainly life as we know it, based on carbon and liquid water. The anthropic principle is implicit or explicit pretty well throughout. The contributors' expertise and standpoints cover a wide range of sciences, plus the history of science, philosophy, and theology. The articles are divided among four parts, the titles of which give some idea of the scope of the book: I 'The fitness of "fitness": Henderson in context'; II 'The fitness of the cosmic environment'; III 'The fitness of the terrestrial environment'; and IV 'The fitness of the chemical environment'. There is also a preface, 'The improbability of life', by G. M. Whitesides.

The editors hope that the book will stimulate thinking and new investigations among scholars and scientists concerned with big questions such as "why can and does life exist in our universe?" The target readership is scientists, academics, and others, working in a range of disciplines.

I found this book comprehensive, and, on the whole, readable. It is well referenced, and adequately indexed. A few contributions require a fairly substantial knowledge of biochemistry or physics if the reader is to benefit fully from the text, but I would nevertheless recommend it to readers who are neither scientists, nor academics, nor theologians. After all, this book addresses an important issue, and within a few decades we should have a much better picture of how rare — or how common — carbon–liquid-water life is in our region of the Galaxy, and perhaps a more secure science-based view on the likelihood of life with a chemically different basis. — BARRIE W. JONES.

Planetary Systems and the Origins of Life, edited by R. Pudritz, P. Higgs & J. Stone (Cambridge University Press), 2007. Pp. 315, 25·5 × 18 cm. Price £65/$130 (hardbound; ISBN 978 0 521 87548 6).

This is a valuable contribution to a welcome new series of astrobiology books published by Cambridge University Press. The subject area is timely and the first three books in the series, of which this is the latest, are carefully edited collections of articles with a common theme. It would not be accurate to describe this book as conference proceedings. Rather, it is a conference-inspired collection. The editors have organized review papers from an overview conference on astrobiology at McMaster University so as to create a volume that is well structured and suitable for teaching, and appropriate to inform research students and seasoned researchers. A field like astrobiology is necessarily multidisciplinary and so there is a real need to introduce high-level researchers in one area to what they need to know about other related sciences.

The book is substantial and divided into three parts. The first deals with planetary systems and the origin of life, and covers extrasolar planets, atmospheres, terrestrial-planet formation, and how you get from protoplanetary discs to things like genetic codes and cells. The second part focusses upon known forms of life on Earth (and by way of contrast, potentially on Mars); Lynn Rothschild on how microbial life has adapted to extreme environments that may be analogous to those to be found elsewhere in the Solar System; and articles on gene histories, metazoans, and a range of biological issues concerning habitat and ecosystem stability. The third part focusses on life in the Solar System, possible forms of life on Mars, Titan, and Europa, and how to determine observationally whether any of the hypotheses about their presence or absence might be correct. The editors span the disciplines of biology, astronomy, and planetary sciences in a way that has produced a volume that is uniform in style, accessible, and useful for students and workers with an astrobiological leaning, whatever subject specialism they work in. Recommended to readers throughout the Solar System. — JOHN D. BARROW.

The Oxford Companion to Cosmology, by A. Liddle & J. Loveday (Oxford University Press), 2008. Pp. 343, 24 × 16 cm. Price £35 (hardbound; ISBN 978 0 198 60858 5).

Oxford Companions are a long-established tradition. The one to English literature is where you look if you want to know what someone means by calling you Micawberish. And you might look in the music one to find out whether any of Verdi's operas have happy endings. In this one, you can look for items you might encounter in reading something else about cosmology from Abell (clusters) to Zwicky (Fritz). It begins with a seven-page overview of the standard hot Big Bang and ends with a somewhat longer index (both generally very good). There are pictures (Friedmann, Gamow, Hoyle, *etc.*; Einstein gets two) though not much conversation. The single most annoying aspect is that all page numbers occur on inside corners, so that one cannot flip through pages looking for something said to be on page 184 but must open the book flat to discover that one is referred to "extra dimensions" for a treatment of Kaluza–Klein theories. The articles dealing with topics on which the authors are particularly expert, for instance, inflation and dark matter, are extensive and authoritative. Indeed the range of expertise displayed is very impressive, given that they seem to have done it all themselves, no colleagues being thanked for comments or contributions.

Now, of course, the rest of my remarks are going to be complaints, which also run from Abell (who didn't just examine the Palomar Observatory Sky Survey plates but also took more than half of them; Al Wilson, who did nearly all the rest, didn't precisely "assume" H = 180 km/sec/Mpc — that was the Humason–Mayall–Sandage number from 1956, used by everybody at the time) to Zwicky (who could not have been the first to notice that galaxies cluster from his *Zwicky Catalogue*, since he had measured the velocity dispersion of the Coma cluster more than 30 years before; and whose colleague Baade indeed came to Pasadena, to Mt. Wilson, or to Santa Barbara Street as you wish, but to Caltech only to attend seminars, if that).

In between, I was hoping for an explanation of anti-de-Sitter space, which is not there, and I think the description of de Sitter space is not clear enough that the constant density is identically zero*. Also not there, Hilbert and Brans-Dicke; and other good friends like Bondi and Gold are hidden in other articles, though Hoyle gets his own (with reference to, I think, the less interesting of two biographies published in the last couple of years). A list of oddities includes (*i*) expression of surprise at the pronunciation of arXiv that doesn't mention that's a 'chi' not an 'ex', (*ii*) the statement that Friedmann came to cosmology late in life, though the numbers are there to tell you he was 32 and so would have been eligible for every young scientist's prize now given, (*iii*) a complete capitulation to the forces of political correction in speaking of "gravitational waves" (historically something in the Earth's atmosphere) rather than "gravitational radiation", which doesn't even make the index, and (*iv*) placing Gamow's first nucleosynthesis paper in 1946 (*Ohio Journal of Science*, 1935).

Could some of these things be described as "things I might have done differently?" No, because never in the world would I have attempted anything of the sort, and that the authors have done so is admirable! — VIRGINIA TRIMBLE.

Modern Canonical Quantum General Relativity, by T. Thiemann (Cambridge University Press), 2007. Pp. 819, 25·5 × 18 cm. Price £75/$140 (hardbound; ISBN 978 0 521 84263 1).

Writers on the philosophy of time often refer to what have been the two faces of time. One face is time as one coordinate among many in the block universe of Einstein, the other face is time as a parameter labelling what happens from time to time. This fundamental dichotomy is reflected in physics where, roughly speaking, the first face corresponds to Lagrangian dynamics and the second to Hamiltonian mechanics. Nevertheless, in almost all of physics, there is no real conflict between these two aspects of time; they can be shown to be equivalent. This is far from obvious when it comes to unifying General Relativity and quantum mechanics, quite possibly because one seems better adapted to the first face and the other to the second face. It is not surprising, therefore, that current approaches divide into those, like String/M theory and its precursor supergravity theory, which are fundamentally covariant in outlook, and what is often called Loop Quantum Gravity, which is quintessentially Hamiltonian in spirit. Since neither project has yet reached completion, it is too early to say whether one face is to be preferred over the other.

* In a translation, kindly provided by a Dutch-speaking student and forwarded by Karl Glazebrook, of a newspaper article (*Altemeen Handelsblad*, 1930 July 9), de Sitter describes as empty a universe whose name "which due to personal reasons, I need to conceal." He thereby joins the select company of gentlemen who spoke of "my father's criterion" and "the diagrams".

The present mammoth tome, of over 800 pages, is the most complete account to date of the Hamiltonian approach to the quantization of General Relativity. It is mathematically challenging and not to be embarked upon by someone unacquainted with, or unwilling to learn very quickly, General Relativity, differential geometry, functional analysis, operator algebras, *etc.*, *etc.* True, the author offers brief introductions, but I think it can safely be said that the typical reader of *The Observatory* is likely to find this book pretty heavy going.

Should such a typical reader bother at all with the book, or order it for their library? It certainly constitutes a major contribution to the subject, perhaps *the* major contribution in book form. More importantly, both String/M theory and Loop Quantum Gravity are beginning to emerge from a long period of internal development to a point at which they can begin to make suggestions about the relevance of quantum gravity to astronomical, or more properly cosmological, observations. The main, and perhaps the only, direct application of quantum gravity to the observable world is to the physics of the very early Universe, to what happened before inflation and what were the initial conditions of the Universe that allowed inflation to take place — if indeed that is the correct explanation for the spectacular stream of data we have recently seen and are about to see from satellite-borne and ground-based observations. It is also possible that the upcoming initiation of the *LHC* accelerator at CERN will have indirect relevance for quantum gravity, perhaps by seeing evidence for supersymmetry, an essential component of String/M theory, but not necessary in Loop Quantum Gravity.

If the exciting possibility of links like these between the worlds of the very small and the very large are realized, then theorists will have to delve much deeper into the structure of quantum gravity than hitherto. This book is a magnificent and comprehensive introduction to one possible avenue. It has no rival. — GARY GIBBONS.

Introduction to High-Energy Astrophysics, by S. Rosswog & M. Brüggen (Cambridge University Press), 2007. Pp. 355, 25·5 × 18 cm. Price £35/$65 (hardbound; ISBN 978 0 521 85769 7).

This might well be called the golden age of high-energy astrophysics. We have a flock(?) of missions, including *XMM–Newton*, *Chandra*, *RXTE*, *Swift*, *Integral*, *Suzaku*, and *Agile*, in addition to detectors of various kinds on many other spacecraft. There are also an increasing number of ground-based experiments probing cosmic rays and neutrinos. While a seemingly endless number of conferences are held, there are few clear guides to the basics. This book therefore comes as a very welcome addition to the field.

Striking a balance between just enough theory and not too much is a difficult task for any textbook. I feel the authors have it about right in this slim but well-structured tome. Beginning with emission processes, the authors provide an introduction to the various types of object that populate the Universe. Each chapter can, however, be read in its own right. Obviously there are other books that provide more detail than can be included here, but this is an excellent starting point. Each chapter comes with a reading list.

My own research interests lie towards the later chapters on gamma-ray bursts and active galaxies, but I accept that leading up to them using supernovae, neutron stars, and the binary-star zoo makes sense. It can be argued that our understanding of what is actually going on in real life decreases as one progresses through the chapters. I'm sure the authors intend that this book should stimulate the reader to go out and discover more.

This is a textbook and so has questions, some of which would stretch the most able undergraduates, but that should not put off those who hoped never to have to do an exam-type question ever again. It could easily form the basis of more than one undergraduate course and should be required reading for anyone working in high-energy astrophysics. The style and layout are clear and all for a reasonable price. Highly recommended. — PAUL O'BRIEN.

THESIS ABSTRACTS

EVOLUTION AND NUCLEOSYNTHESIS OF ZERO-METALLICITY AGB STARS

By Herbert Ho Bun Lau

This work describes the evolution and nucleosynthesis of the first generation of low/intermediate-mass stars, also known as zero-metallicity stars or Population III stars. Detailed models from the STARS code are presented and analysed.

Such stars played an important rôle in early-Universe nucleosynthesis because they were the first production site of metals. Because of the absence of metals, in particular carbon, nitrogen, and oxygen, some nuclear reactions could not occur in those stars. They were considerably hotter and their evolution was very different from that of higher-metallicity stars.

This work first describes the evolutionary code, STARS, used to model those stars. Modifications needed are described and explained. There are numerical issues because of their extremely low CNO abundances throughout their evolution. Higher resolution is particularly needed, especially around regions where carbon is produced.

The models from our code were first compared with other works. In general, the results agree relatively well up to the early asymptotic giant branch; for example, the occurrence of H–He core flash and surface abundances after second dredge-up. Some possible explanations for the quantitative differences are discussed.

The evolution of 5-$M_{\odot}$ and 7-$M_{\odot}$ stars was modelled through all the thermal pulses. Their pulses are much weaker than those of higher-metallicity stars of the same mass. The pulses grow weaker and eventually stop, after which the core grows much faster. Carbon then ignites degenerately at the core before the cessation of burning in the envelope. This causes a thermonuclear runaway and the stars explode as supernovae of type 1·5. With consideration of the lower mass-loss rate of zero-metallicity stars, supernovae are a probable fate for zero-metallicity, high-mass AGB stars.

Observed carbon-enhanced metal-poor stars were probably formed by mass transfer in binary systems. The work also shows that HE 0107-5240, for example, could have been formed by mass transfer from a 7-$M_{\odot}$ star. — *University of Cambridge; accepted 2008 February.*

Simple Four-Mirror Anastigmatic Systems with At Least One Infinite Conjugate

By Andrew Rakich

This thesis describes an analytical approach to the optical design of four-mirror anastigmatic optical systems. In all cases investigated here the object is at infinity. In the introduction, the field of reflecting, or 'catoptric', optical-system design is discussed and given some historical context. The concept of the 'simplest-possible reflecting anastigmat' is raised in connection with Plate Diagram analysis. It is shown that four-plate systems are in general the simplest possible anastigmats, and that four-plate systems comprised of four spherical mirrors are the last family of 'simplest-possible reflecting anastigmats' for which the complete solution set remains unknown.

In Chapter 2, third-order aberration coefficients in wavefront measure are derived in a form that is particularly suitable for Plate Diagram analysis. These coefficients are subsequently used to describe the Plate Diagram, and to detail the application of the Plate Diagram to the survey of all possible solutions for four-spherical-mirror anastigmats. The Plate Diagram technique is also generalized to investigate its use as an optical-design tool. In the example given, a generalized Plate Diagram approach is used to determine solutions for four-mirror anastigmats with a prescribed first-order layout and a minimum number of conicoids. In Chapter 3, results are presented for the survey of four-spherical-mirror anastigmats in which all elements are required to be smaller than the primary mirror. Two novel families of four-spherical-mirror anastigmats are presented and these are shown to be the only examples of four-spherical-mirror systems that exist under the given constraints.

Chapter 4 gives an example of the application of Plate Diagram analysis to the design of an anastigmatic system with a useful first-order layout and a minimum number of conicoid mirrors. It is shown that systems with useful first-order layouts and only one conicoid mirror can be obtained using this method. In Chapter 5, results are presented of the survey of all remaining four-spherical-mirror anastigmatic systems, that is, systems in which elements are allowed to exceed the diameter of the entrance pupil, which includes systems with concave and convex primary mirrors. A wide variety of solutions are presented and classified according to both the underlying geometry of the solutions and the first-order layouts. Of these systems only one has been reported in previously published literature. The results presented in this thesis complete the set of 'four-plate' reflecting anastigmats, and it can now be said that all possible solutions for four-spherical-mirror anastigmatic systems have been determined. — *University of Canterbury; accepted 2007 April.*

Here and There

OH NO HE DIDN'T

[Pogson] recognized … that stars of the traditional sixth magnitude were some one hundred times brighter than those of the first. — *Journal for the History of Astronomy*, **38**, 376, 2007.

ADVICE TO CONTRIBUTORS

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

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

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

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

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