# THE

# OBSERVATORY

## A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2005 November 11th at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

*The President*. Good afternoon, ladies and gentlemen. Let me start by reminding new Fellows, or long-serving Fellows who have not previously taken the opportunity, that they are warmly invited to sign the membership book after the meeting in the Society's apartments, in the less formal atmosphere of the drinks party. I will be very pleased to welcome them to the Society.

Before we proceed to the scientific part of the programme, it's my great pleasure to present the Fowler Awards for Geophysics and Astronomy. These are awarded to younger scientists, so it's very nice to be able to acknowledge the contribution that such people are making to the subject. I should also say that Mary Fowler is in the audience today. First, the Fowler Award for Geophysics is awarded to Dr. Arwen Deuss of the Earth Sciences Department at Cambridge. Dr. Deuss got a D.Phil. from Oxford in 2002 for work that included the first observation of an inner-core shear-wave, something seismologists have sought for over fifty years. Her novel contributions to seismology include observational work on the global properties of the upper-mantle discontinuities, as well as theoretical work on spectral methods used in low-frequency seismology studies. Arwen, congratulations. [Applause.]

The Fowler Award for Astronomy this year goes to Dr. Gordon Ogilvie of the Institute of Astronomy in Cambridge. He is a gifted young theoretical astronomer currently based at the Institute of Astronomy, with research links to DAMTP. He is carrying out theoretical research into clarifying and explaining the properties of accretion discs, and has already published around thirty papers in refereed journals. A remarkable feature of his research is his ability to construct and carry out precise mathematical calculations that reveal key features of complicated astrophysical processes. He has applied this skill to a variety of problems arising from observations of accretion discs, starting with magneto-rotational instabilities and going on to deal with waves, eccentricity, warps, outflows and, most recently, the interactions of discs and planets. Gordon is very clearly a researcher of great talent and promise. [Applause.]

It's excellent to be able to recognize and acknowledge the contribution of young people who are the future of the subject.

We have an interesting and slightly different scientific programme today. We are going to have a discussion on the Aurora programme, followed by the Harold Jeffreys Lecture. But first, we are going to have a short presentation by Dr. Derek McNally on 'Major lunar standstills at Stonehenge'.

*Dr. D. McNally*. I would like to continue the lunar theme of the earlier part of the day and present a short contribution on the Society's Stonehenge and Astronomical Heritage Committee's (SAHC, formerly the RAS Stonehenge Group; chairman Professor C. L. N. Ruggles) project to record lunar 'standstills' (a term introduced by Alexander Thom, a noted surveyor of megalithic monuments). I would like briefly to outline the consequences of the lunar 18·6-yr precessional cycle on the changing declination of the Moon — namely, that over this cycle the Moon's declination, north and south, can reach in excess of 28 degrees at the major standstill and just under 19 degrees at the minor standstill. A standstill is not an instant but rather an extended period of about 12 months, rendered still further imprecise by the lunar 9-arcminute nutation. The Moon is currently in the early stages of a major-standstill period.

The SAHC is recording the rising of the Moon at major and minor standstills. I have preliminary slides which show the moonrise of 2005 September 26. These illustrate that the Moon, at a major standstill, rose in the lintelled arch to the north of the summer-solstitial sunrise arch (the minor-standstill moonrise being in the corresponding arch south of the solar summer-solstitial rise position). They also show that even a low-budget endeavour can experience interference beyond the observer's control, and that fill-in flash is unnecessary to image the stones given the ambient light pollution at Stonehenge. I hope Fellows will take advantage of the standstill to see the spectacle of the Moon high in the sky at one part of its monthly cycle and two weeks later skimming across the treetops (*i.e.*, at about an elevation of ten degrees in latitude 51·5 degrees N).

*The President*. Thank you, Derek. Are there any questions or comments?

*Anon*. It is often said that Stonehenge was built for the observation of the midsummer sunrise, but of course, with English weather, that was not always possible. Is that absolutely ruled out?

*Dr. McNally*. It wasn't built for observation of the summer solstice. I think it may have been used at the solstices, but I don't think it was the summer one. The principal entrance to a church is at the west end, and on entering one is faced with the principal focus — the altar. So with Stonehenge one enters from the north-east and witnesses the spectacle of a setting winter-solstice Sun. Consequently, I think they were focussing on the midwinter-solstice setting. There are other monuments around the country that are also lined up on the winter-solstice setting, but then there are two up in Orkney, for example, which face each other; one faces the setting Sun and one faces the rising Sun. I think it's like fashions in astronomy — what we observe and where we send satellites and so on — and I imagine that in neolithic times there were just as many fashions as there are today.

*Anon*. May I add another point? In the south of Armenia, there is a similar monument to Stonehenge, which, uniquely, has holes bored in the stones for observation of the stars. Has the Society ever looked at the implications of this?

*Dr. McNally*. No. [Laughter.]

*Dr. N. Kollerstrom*. Diodorus of Sicily, the 1st-Century historian, alluded to a monument, apparently in Britain, which is widely supposed to have been Stonehenge. He said that a 19-year cycle was celebrated there. In addition, there are 19 stones in the inner bluestone horseshoe at Stonehenge. Is it likely that these are related to what you have just been discussing: the 18·6-year cycle of the lunar major standstill?

*Dr. McNally*. I wouldn't like to say, but I would agree that 19 is pretty close to 18·6. Against that, I've already pushed my luck with one coincidence in suggesting that the separation of the major and minor standstills at rising match this 12-degree spacing of the uprights of the lintelled circle, plus or minus a degree. So I'll pass on that one.

*The President*. Thank you very much indeed. We are now going to move on to a panel discussion on the Aurora programme. There are going to be four short presentations and then a period in which I hope we will get some discussion going between the panel members and the audience. I'll leave the speakers to run it and then just step in to referee occasionally.

*Mrs. Sue Horne*. Aurora is an optional programme of the European Space Agency (ESA), focussed on exploration of Solar System destinations that could eventually be explored by humans, *i.e.*, the Moon and Mars. In practice, the early part of the programme, which PPARC is considering joining, is for robotic exploration of Mars. PPARC is proposing to join the following elements of the programme. (*i*) The *ExoMars* mission (to be launched in 2011), which will search for traces of past and present life, characterizing the Mars biological environment, and improve our knowledge of the Mars environment and geophysics. UK scientists have registered interest in hardware involvement in nine instruments. (*ii*) 'Preparation for the Future': a programme to support scenarios for future missions and undertake critical technology development. The PPARC contribution to *ExoMars* is similar to the PPARC contribution to missions funded through ESA's science programme.

There is broader interest in Aurora than just the traditional PPARC community: 33 institutes have been identified that have an interest in Aurora-associated science; and 133 academics attended a NERC/PPARC comparative planetology meeting on 2004 September 15. PPARC has put forward a bid to OST for a balanced programme which includes the ESA subscription, instrumentation and technology development, science exploitation, knowledge transfer, and an outreach programme.

*Professor Monica Grady*. I am a planetary scientist and a fervent supporter of the Aurora programme. There are lots of reasons for my support: scientific and technological reasons, reasons related to education and training, to outreach; that it involves a huge community of people, and it is politically a good thing to do. But the main reason I am a supporter of Aurora is the science driver to answer the question that interests me and many others: is there life beyond the Earth, and specifically, is there life on Mars?

Mars is so like the Earth in many ways, but it has gone so wrong sometime in its history. We are still trying to grasp why and how and when it went wrong — what is the real difference between Mars and Earth? Is it because it is smaller than the Earth, that it does not have plate tectonics, that it has lost its atmosphere, or lost its water; that it is no longer active in the sense that Earth has a dynamic interaction between the atmosphere, hydrosphere, and lithosphere?

We have learnt a huge amount from the spacecraft which have been put into orbit around Mars, from those which have landed, and from meteorites which come from Mars. But taken all together, there are still a large number of things about the planet that we don't understand. We don't even know whether there is any carbon there. There is $CO_2$ in the atmosphere, but we have not found any carbonates in the soil. The amount of carbon found in meteorites is vanishingly small and potentially swamped by terrestrial contamination, so we have not yet managed to analyse whether there is any carbon, organic carbon, biological carbon, or life on Mars.

We will only be able to answer this fundamental question by sending missions to Mars, like the *ExoMars* mission planned for 2011. We will only be able to understand the planetary structure of Mars by sending seismic packages to Mars, by drilling into the surface, and by setting up networks of seismic packages. None of those questions is going to be answered by any of the missions in the current NASA programme.

I want to sum up and answer perhaps some of the political questions that non-planetary scientists might ask. Why should ESA do it alone, why should ESA not join with NASA and cut down the cost? The ESA programme is complementary to the NASA programme. When Aurora was first mooted, which was before Bush's grand-vision statement and the US Mars programme was proposed, there was a lot of criticism of Aurora because it was extremely challenging and expensive. It has been made a lot more realistic since those early days. The US programme has been trimmed dramatically as well: recent disasters have led to the pruning of NASA's budget; NASA has refocussed on human exploration and return to the Moon, so NASA's initiative in exploring Mars has receded. ESA therefore has the opportunity to invite the US to join ESA as it leads joint missions; indeed, part of *ExoMars* incorporates an important US instrument.

Where is the money coming from? Is this going to mortgage PPARC's future for many years? Is it going to prevent us building very large telescopes? No — this is a particular part of PPARC's programme for which they are asking for extra resource. PPARC's Science Committee and Council have said they will not go through with this unless there is significant extra resource in the programme. That extra resource is for Aurora, not extra money for PPARC's other programmes. So this is not money which can otherwise be spent on accelerators for particle physics or a large telescope.

We're asking for extra resource to train a new generation of scientists, engineers, and technologists; extra resource to inspire the next generation of school students; extra resource for our school-teachers; extra resource for our space industries, which make up a major part of our GNP; extra resource to enable knowledge transfer; but most importantly, extra resource through Aurora to look to our neighbouring planets to try and understand if there is life on those planets, was there life in the past, is it extant, extinct, dormant, or was it never there? If we find traces of life on Mars, the implications for life beyond, and the search for exoplanets, is much wider. If it's not there, and never has been there, it really may be showing us that perhaps Earth is truly unique, with all that that means for humanity. So, members of the congregation [laughter], my thesis is that we cannot afford to pass up this opportunity; we cannot afford *not* to be part of Aurora.

*Professor T. W. Hartquist.* Gosh Monica, if you'd had another five minutes, you'd probably have converted me! [Laughter.]

About a year ago I had the privilege to participate in the discussions of the Solar System sub-panel of the PPARC Astronomy Grants Panel. I was impressed and pleased to see how strong and vigorous the UK Solar System community is. It plays a vital rôle in each of numerous areas including polar ionospheric physics, magnetospheric and space-plasma physics, solar-corona research, dynamo theory, helioseismology, meteoritics, and planetary science. The breadth and depth of UK Solar System research are impressive.

My main areas of research include star formation, the hydrodynamics and magnetohydrodynamics of diffuse galactic and extragalactic sources, and molecular astrophysics. However, I have published on mesospheric and ionospheric problems, the solar corona, heating of meteoritic inclusions, and Saturn's rings, as well as other astrophysical and laboratory phenomena. I find a broad range of questions interesting, and that's precisely the reason that I am concerned about the impact that the funding of Aurora could have on science.

Let's consider some numbers. The PPARC Astronomy and Solar System annual grants budget is £24 million. That funds the purchase of computers and their management; the construction of instrumentation in many areas including optical astronomy, cosmic-microwave-background research, and polar-radar studies; operational costs of a good number of small and moderate-sized facilities; travel; visitors' costs; salaries of postdoctoral researchers. Thus, if invested wisely, £24 million per year can support a very diverse and excellent research portfolio.

Depending on whether one uses the numbers mentioned by Steve Miller or those given by Sue Horne, the UK commitment to Aurora is likely soon to be £25 million per year or somewhere around half that if averaged over a long enough period. The part of the Aurora programme to which these numbers apply covers only instruments to be launched in about the next half-dozen years. One is justified in asking how much top-class science the Aurora investment will buy compared to what the current funding of grants purchases.

In their talks, the other speakers state that Aurora would be funded by 'new' money from the government. However, in conversations that I had with various PPARC staff a year ago, I heard that the 'new' money would cover most but not all of the Aurora costs. Then unidentified 'lower priority' programmes were to be cut to provide the rest. This issue needs clarification. One might also wish to bear in mind PPARC's prediction that the funding allocated to cover 'full economic costing' is too little to maintain the work supported by grants at the current level. Furthermore, one might also worry whether Aurora cost overruns and delays would make other programmes and the grant budget even more vulnerable.

The other speakers address what is probably the most limited programme that one could associate with the name Aurora and what many Aurora advocates would like to see as just the first stage of a bigger programme. A single major space-based mission costs within a factor of two either way of one billion dollars. A longer-term robotic Aurora programme would involve numerous missions of various sizes and might cost an order of magnitude more than a single major mission.

Sometimes, the name Aurora has been used in connection with a programme that would ultimately put humans on Mars in the 2030s. In a report commissioned by the RAS, Close, Dudeney & Pounds recommended that the UK spend £150 million per year to support a manned space programme. (The RAS has publicized this report in such a manner that the Society appears to endorse the recommendation at least tacitly, though no debates and votes involving its membership or, more importantly, its professional membership have taken place. I do not view an invitation placed on the Society's web page to 'blog' about human space flight as a means of initiating a serious, informed debate.) The £150 million per year is an insignificant fraction of the total required international budget to put a base on the Moon, much less get humans to Mars. Some guesses of the total cost of the achievement of that latter goal are in the range of many hundreds of billions of dollars. For comparison, the UK 2004 GDP was about 1700 billion dollars.

The possibility of a mission to get humans to Mars and back has triggered considerable debate and worry in the States. An editorial in the 2005 March 11 issue of *Science* stated, "On 14 January 2004, President Bush announced a 'vision' for space exploration: a project that would take astronauts to the Moon to establish a base and then launch a manned probe to Mars. This announcement, strangely absent from the State of the Union address a week later and still undiscussed by Congress, had a major impact on the NASA budget. ... The 3M vision may be good for lunar and Martian research, but it is bad news overall for science."

According to a 2005 March 21 *New York Times* article, Robert Kirshner, a Harvard professor and the President of the American Astronomical Society, has said, "They are doing it totally from the top-down ... from the President, the political people. That does not make it evil, but that is not where the community of scientists saw the opportunity."

In a recent warning about the risks of astronomical and Solar System 'Overly Ambitious Facilities' published in the *AAS Newsletter*, Peter Foukal, a solar physicist, invited astronomers to recall the Superconducting Supercollider (SSC). He cited the negative effect that the "SSC debacle" had on experimental particle physics in the USA. This OAF's initial projected cost was about six billion dollars, a number that bears comparison to some given above.

Before this discussion, I sent emails to nine US-based scientists telling them of the upcoming debate. Eight of them, plus a former AAS President whom I did not contact, responded. I do not have time to summarize all of their statements, but they reflected concern. One response came from a friend who is European by birth. He noted, "Contrary to the UK or Europe in general, the attention span in the US can be short, and for example we did not live up to our promises on ISS (Space Station). ... If you argue for the spin-offs: making school kids excited or new high-tech stuff, I imagine that these goals could be better achieved by directly investing in schools and industry."

One year of UK funding for even the limited Aurora programme would pay off the student debts of a thousand physicists. According to a very unscientific survey I conducted with one third-year class, many students would be willing to commit to three or more years of teaching, if such a debt-relief package were offered.

The search for life is cited as one of the main justifications for Martian exploration. I am tired of those favouring expensive missions to Mars wrapping themselves in the flag of the origin of life and/or the distribution of life in the Universe. Science concerns how and why parts of the Universe became as they are. Processes and mechanisms are behind the how and why. The most cost-effective means of addressing many of the major processes would almost certainly involve additional investment in particular chemical and molecular biological areas and possibly the study of life in extreme terrestrial environments. If we want to be cost-effective in the investigation of astrophysical processes relevant to the origin of life, we should support the work of people like Gordon Ogilvie, who received a prize at this meeting, and his colleague John Papaloizou. They work on the theory of discs and planet formation and planetary migration. Exploration of Mars would tell us more about the specifics of what happened there, but I simply do not see it revealing as much about the important fundamental mechanisms as these much less expensive types of research would.

We need the RAS to promote further discussion. A huge problem is the lack of information, and the RAS can play a useful rôle in obtaining and disseminating information.

Questions requiring answers concern the identities of programmes that are vulnerable due to the initiation of even the limited Aurora programme, the potential consequences of delays and cost overruns of that programme for others, the exact nature of and cost estimates for more extensive versions of Aurora, and the possible consequences for other science if they were funded. The RAS publication *A&G* is an ideal vehicle for dissemination.

I am also concerned about how the government gets advice about science priorities. In the States, the National Academy advises the government in a way that is, at least in part, independent of the funding agencies. What rôle does the Royal Society play or should it play? In astronomy and Solar System science, it does not seem to contribute as the US Academy does in the decadal forward look. If the government is willing to invest tens of millions of pounds per year of 'new' money in science to strengthen UK participation in a key field, the scientific community must do its best to help the government make wise choices. Researchers should be able to identify a number of areas across the spectrum of UK science that would benefit from such investment and for which we could make attractive cases. I am speaking of bottom-up policy formulation conducted in a coordinated, rational fashion. I would really like to see large investments in science in the UK go into areas to ensure UK dominance. This would be better than spending to put the UK into a supporting rôle in an area in which it will eventually be a hostage to the attention span of a larger country.

*Professor S. Miller*. Good afternoon, my name's Steve Miller from UCL, and I think I am here because I chair the PPARC Solar System Advisory Panel. Clearly I am mainly a planetary scientist, but I have done work on cool stars, the ISM, stars in the early Universe, so I have wide-ranging interests too and I'm not one of those people who think that planets are absolutely everything. Tom has raised so many issues, and every single one of them has the smell of a red herring about it [laughter] — so let's try to look at some of them.

Tom said he has heard of a lot of definitions of Aurora. It's because UK scientists have been very vocal and very involved in the preparation stage of Aurora that we are defining Aurora: that is why ESA's Aurora programme is now an affordable, sensible programme of robotic investigations. Yes, there are those who would like Aurora to become human missions — in the future. That debate is on-going; what we are talking about is what Aurora is at the moment.

We have heard the criticism that if we have Aurora then everything else will shut down because it is so expensive. Well, first of all, Aurora is a programme of uplift — uplift of up to 25 million pounds per annum into PPARC's budget; uplift that has enormous support at all levels in the Solar System community, but also with the Minister for Science. Some of us were lucky enough to go to a meeting that PPARC organized at the Treasury a couple of weeks ago which spoke about the achievements of UK planetary science and about Aurora. Lord Sainsbury spoke there very warmly about Aurora. His last two sentences were: "I have here the end sentence prepared for me by the civil servants. It says that prior to the ESA ministerial meeting, I can't promise you anything. But I have written my own last sentence — it goes: I can assure you that I will be fighting for Britain to have the fullest and most leading involvement with Aurora possible." Now I have never heard a minister be so supportive of any science programme across any discipline as I heard from Lord Sainsbury. And I believe 'what it says on the tin', I believe that's what he is going to go for. I believe that is the way in which he sees this programme. Monica is quite right — if we say we don't want Aurora, we are not going to get any money for particle physics, we're not going to get it for cosmology, it's just not going to be there. Ministers are like huge oil tankers: once you get them moving, you don't turn them round on a sixpence. And if any of you would like to tackle Lord Sainsbury in full flood, and spin him around, go ahead, be my guest! [Laughter.]

My job as Chair of the Solar System Advisory Panel is to ensure we have a balanced programme across the whole of Solar System science — solar science, solar-terrestrial physics, solar-planetary physics, which is what I'm interested in. 'Rock-jocks' like Monica, well, they don't interest me so much. [Laughter.] It's my job and the panel's job to ensure there is a balanced programme, and if Tom wants to see a balanced programme he can look first at the strategy document that we put out in 2002, and in spring of 2006 he can look at the revised strategy document that we will be putting out that puts forward a balanced programme; and we'll put forward that programme next week to the Science Committee along with other advisory panels, who will ensure there *is* a balanced programme across PPARC. So there are checks and balances within PPARC — some of us think sometimes too many checks and balances! — that ensure that not one programme is allowed to trash everything else.

We heard these kind of calamitous statements when we joined ESO: everything will close down! Well maybe they will in the future, but I still hear that the *UKIRT* board is putting forward proposals to get them into some new instruments. *UKIRT* is still a flourishing telescope — and long may it flourish, I still make use of it. I don't tend to make use of ESO, but I think joining ESO was the correct decision. So this whole notion that one programme is somehow going to trash astronomy or the whole of particle physics is simply fantasy. There is no mechanism by which that could happen.

Tom warned about ambition, that there are people in Aurora with ambition. Well I hope that this meeting is full of people with ambition; if scientists don't have ambition, who on Earth is going to have? Of course we have ambition; part of it was to be an integral part of the Aurora programme. If there are people who have ambition to get the UK into human spaceflight programmes, then that cannot possibly come out of the science budget; but let them go out and fight for their ambition across the political spectrum. Culture, media, and sport: if there's something that 'culture' should be doing for science it's probably getting to do real 21st-Century exploration. The fears are unfounded; we will have a balanced programme, we will have an ambitious programme, thank heavens, we will have an exciting, dramatic programme. Just embrace Aurora, it's great: 25 million quid a year from Lord Sainsbury — I'm shopping there every day from now on! [Laughter.]

*The President*. Well, I'm afraid that the impassioned speeches have taken up pretty much all of the time available! [Laughter.] I do think we need to move on fairly promptly to the Harold Jeffreys Lecture, but we have time for one or two quick points, nothing longer.

*Professor I. W. Roxburgh*. Yes, mine is a question. This funding for the Aurora programme will produce an imbalance in the distribution of funding within astronomical sciences. The opposite was said, but it obviously will. My question is to PPARC: if Aurora is funded, which represents a 40% increase in the budget, we're told, will there be a transfer from the remaining £25 million out of Solar System science into astronomy, in order to rebalance the whole distribution?

*Professor Miller*. I'll answer that. This is a question for the Science Committee to look at, but where you draw balances is a matter of judgement. The PPARC Science Committee has four particle physicists and a particle astrophysicist, it has four astronomers, and Monica Grady is the sole Solar System representative. I'm not too concerned that astronomy, which I am also very passionate about, is going to suffer because of imbalances. We're talking about an opportunity really to bring up UK planetary science and that's a gain for the whole community.

*Professor Roxburgh*. My question was whether astronomy can benefit from it.

*Professor Miller*. Well, of course it can. Who is not interested in extrasolar planets?

*The President*. We can't have everyone standing up. Monica — sit down, we haven't got time! [Laughter.] We could clearly go on for hours and it would have been nice if we had more time for discussion, but we really do need to move on and hear this year's Harold Jeffreys Lecture, to be delivered by Professor Paul Silver from the Carnegie Institution of Washington. His title is 'Mantle deformation, continental evolution, and the Wilson cycle: paradoxes and proposals'.

*Professor P. Silver*. [It is expected that a summary of Professor Silver's lecture will appear in a forthcoming issue of *Astronomy & Geophysics*.]

*The President*. Thanks very much indeed, Paul, for a really stimulating lecture. Are there any questions or comments?

*Mr. M. F. Osmaston*. You're invoking viscous dissipation, which means you have got to have a force to do the work. Where are you getting your force? Ten years ago you said that the mid-ocean-ridge push was inadequate by a factor of about five, even to support the Andes laterally, so where is this energy and work you require coming from?

*Professor Silver*. Yes, that's a very good point. It's mantle convection, but there are three ways of doing that. One is the ridge-push force, which you referred to; another is slab pull, which doesn't work in Tibet any more because the slab is long gone; the third is basal shear on the base of the plate, and that's the one responsible, I think. As I said, we looked at the flow fields beneath the ocean basins. The mantle is moving at about 2–3 cm a year independent of plate motion. To the extent that continents are coupled into that, there's going to be a basal shear stress at the base of the continents and I think that's more than enough to provide the necessary force. The Indian plate, I would bet, is strongly coupled to mantle convection and that source of stress, that force, is enough to produce this deformation.

*Dr. A. Jackson*. Paul, in your preamble you showed an excellent agreement in the oceans between your observations and a model which was partly based on buoyancy-driven flow in the mantle. I just wondered, did you ascribe all of the velocity perturbations in the mantle to thermal variations? As you know, there are some schools of thought which think that part of the perturbations are compositional and indeed that the great African plume is not necessarily even rising.

*Professor Silver*. That's a very good point. I would argue that we know that it's rising because the flow field would be backwards if it were sinking. We can tell the difference between upwelling and downwelling, and it is upwelling; so I'd say that argues against the composition having a dominant effect.

*A Fellow*. At the western-most North American continental deformation, how has the strain changed and how has the gravitational potential energy changed in that region?

*Professor Silver*. We did a different experiment in western-most North America. In that case, the lithospheric-deformation model failed, so what we did was simply use the surface deformation field as a top boundary condition on the asthenosphere; we have the subasthenospheric mantle underneath. We did not go into the dynamics of the surface deformation field. There we have shear-wave splitting observations, which give us the asthenosphere component, and we just inverted for what was below, and you can do that uniquely. In that case, the lithosphere is too thin for us actually to see this component and our goal there was to try to estimate this sub-asthenospheric, essentially mantle, convection component, and we did a very good job.

*A Fellow*. For how many years has this been going on? Millions of years?

*Professor Silver*. The shear we're seeing in the asthenosphere is at least 30–40 million years old, something like that.

*The President*. May I ask a question? You talked about the direction of the shear where it's splitting; can you use the amount of splitting to test anything like rheology, for example?

*Professor Silver*. Yes, good point. I studiously avoided talking about this parameter called lag time — the time between the fast shear wave and the slow shear wave — which is essentially the product of the path length and the strength of the anisotropy. You can say something in principle, but you have to be able to separate out those two factors and it's actually very difficult to do. You either have to assume the strength or assume the path length. The only place we've been able to do that is in South Africa, because we can go down and grab samples out of the mantle (kimberlite nodules), so we know what the strength is. Then we can actually determine what the thickness was and make sure that it is consistent with the lithospheric contribution, which it turned out to be. But that's the reason why we studiously avoided it. We just used the orientation.

*The President*. I'm going to ask another question as well, one of the privileges of the Chair: last year James Jackson gave the Harold Jeffreys Lecture and he was also arguing against the jelly-sandwich model for rheology, but for a granulite facies basically going all the way through. That's actually got a stronger rheology than you were proposing, if I remember rightly?

*Professor Silver*. Yes, I suppose 'strong' and 'weak' are really relative and I guess he was arguing for a weak mantle. All I need for this — the basic observation — is coupling and the only way to get coupling is if the lower crust and the upper mantle are the same strength, or the same weakness. We don't really care, it's just that they have to be the same. It looks like the only way they can really be the same is if they are both weak. Now, the very top could be strong. If the gravitational potential energy is all being applied by this top layer and deforming everything else, that would work as long as the very bottom of the lithosphere is even weaker. However weak one makes the rest of it, this is still much stronger than the asthenosphere below and that's why I think it's still lithosphere; it's much stronger than the stuff below, otherwise you would not get these kinds of flow fields.

*Dr. G. Helffrich*. Paul, the testable consequence of your idea — that there is constant strain throughout the lithosphere down to the asthenosphere — is that you have seismicity continuously down through it. On the other hand, studies of the depths of seismicity in continental areas support the idea of the jelly-sandwich model where you have two depths at which seismicity actually occurs.

*Professor Silver*. I actually don't think seismicity is a very good diagnostic for strain and the reason is that what you need for earthquakes is slip on a fault; and you need an instability. If you lose the instability you won't get any earthquake. To use seismicity as a diagnostic for strain, you have to assume all these other things about the earthquake process.

*The President*. But James was actually also arguing that you could put the seismicity on the right side of the strength profile, so the seismicity was consistent.

*Professor Silver*. I guess your argument is that there's no seismicity in the mantle, and so he's saying the mantle is weak. Yes it's weak, but it can't be much weaker than the crust.

*The President*. Last question.

*Mr. M. Hepburn*. Can't you argue against that? When McKenzie was talking about this, it's not just seismicity that supports the theory of weakness in the lower part of the crust, it's also local gravitational isotropy. According to careful studies in Siberia, the big mountains there act as if there is gravitational isotropy at roughly the 17-km level, then there's a squashy bit where there are no earthquakes, then you get a stronger bit again in the upper part of the mantle. Surely all that's needed is to say that the strong bit of the mantle above the asthenosphere gets dissolved if it goes below a certain level, and this is supported by studies of the way in which olivine will crystallize out at particular pressures and is then no longer a stable phase at higher pressures. So under the Tibetan plateau you would expect it to be weak because it's not in the same form at that level; it's beginning to disappear.

*Professor Silver*. Well, I don't know. The seismic velocities suggest that the mantle isn't really weak at all.

*Mr. Hepburn*. No — at the top — but then you get the asthenosphere below it.

*Professor Silver*. Well the asthenosphere can be weak, that's no problem. In fact the asthenosphere has to be very weak for this to work at all, but I'm just talking about the lithospheric component. I think what you're talking about is the elastic thickness estimated by McKenzie.

*Mr. Hepburn*. Yes, but there's also the point that, broadly, the composition of the crust indicates something that melts at much lower temperatures than the olivine layer below it, so you would expect it to be strong.

*Professor Silver*. That's why I showed that jelly-sandwich slide about five times; because that's what everyone expects. What we're seeing, however, mechanically — I think observationally — is that and we have to figure out a way to go from that as a starting point to something that is more uniform. I should mention that off the plateau, there is weak coupling between the crust and the mantle, so all bets are off as long as you go off the plateau. Our argument would be that that area is much less deformed than the Tibetan plateau, so this jelly sandwich is still there and in fact the crust is effectively falling off the plateau, so it really depends on how much deformation is actually taking place.

*The President*. I think we really need to stop there before it gets into a personal discussion, which you are very welcome to continue over drinks. We have the drinks party starting now across the way. I think we should finish by thanking Paul once again for a most interesting talk. [Applause.]

## MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2005 December 9th at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

*The President*. It is one of my great pleasures to announce the annual awards. Council was busy yesterday and agreed to make the following awards for 2006, starting with the most prestigious awards, the Gold Medals. The Gold Medal for Astronomy is awarded to Professor Simon White, director of the Max Planck Institute for Astrophysics in Garching, Germany. The Gold Medal for Geophysics is awarded to Professor Stan Cowley of the University of Leicester. The Herschel Medal of the Society is awarded to Professor Govind Swarup, former director of the National Centre for Radio-Astrophysics and Institute for Fundamental Research, and currently honorary scientist at the Indian National Science Academy. The Jackson-Gwilt Medal of the Society goes to Dr. Keith Taylor, California Institute of Technology, Pasadena. The Chapman Medal of the Society is awarded to Professor Steve Schwartz of Imperial College, London. The Fowler Award for Geophysics is awarded to Dr. Claire Parnell at the University of St. Andrews and, unusually, we are making an award jointly for Services to both Astronomy and Geophysics to Dr. Brian Marsden at the Harvard-Smithsonian Center for Astrophysics. The Fowler Award for Astronomy is awarded to Dr. Serena Viti at University College London. [Applause.] Finally, the following Associates of the Society have been elected. On the the astronomy side, Professor Ewine van Dishoeck, Professor of Molecular Astrophysics at Leiden University, Dr. Francoise Genova, Director of CDS, Strasbourg, and Professor James Liebert, Steward Observatory, University of Arizona. On the geophysics side we have Professor Oddbjorn Engvold, Department of Theoretical Astrophysics, University of Oslo, Professor Tuija Pulkkinen, Chief of Space Research and the Meteorological Institute, Helsinki, and finally Professor Sami Solanki of the Max Planck Institute for Solar System Research, Katlenberg-Lindau.

My next job is to announce the winners of the 2005 RAS–Blackwell Prize. The winner of the Blackwell Prize is Dr. Philip Livermore of the Department of Applied Mathematics, University of Leeds, for his thesis entitled ‘Magnetic stability analysis for the geodynamo’, and the runner-up is Dr. Trudy Hoogenboom, who also did her research at the University of Leeds in the School of Earth and Environment, for her thesis entitled ‘Gravity and topography signatures of Venusian coronae.’ [Applause.]

Some other very pleasant announcements to make concern the biennial Queen’s Anniversary Prizes for higher and further education, which recognize and reward outstanding contributions that universities and colleges in the United Kingdom make to the intellectual, economic, cultural, and social life of the nation. I’m pleased to congratulate Liverpool John Moores University for receiving the prize for the work of the Astrophysics Research Institute. Focussing on its development of the world’s largest robotic telescopes, the prize rewards the Institute for significant public outreach work with the National Schools’ Observatory, and recognizes this innovation and success of its undergraduate astrophysics degree and distance-learning courses. [Applause.]

I'm also pleased to announce that Professor David Gubbins of the University of Leeds has been awarded the Institute of Physics Chree Prize and Medal for his contribution to our understanding of the dynamics and evolution of the Earth's core through his work in kinematic dynamo theory, thermodynamics, and palaeomagnetism. [Applause.] A collaboration of European research teams led by Professor Andrew Lyne of the University of Manchester has been awarded the European Commission's prestigious 2005 Descartes Prize for research. [Applause.] And finally, I'm pleased to congratulate Professor Stan Cowley for having been awarded the Julius Bartels Medal of the European Geosciences Union. [Applause.]

It's now time to move to the advertised programme. Firstly, I'd like to welcome Professor Michael Rowan-Robinson to talk about 'Understanding the red-galaxy population: new results from the *SWIRE–Spitzer* legacy survey'. It's a particular pleasure to welcome him here because, unless you do something terribly dastardly at the election next May, he succeeds me as the next President. [Laughter.]

*Professor M. Rowan-Robinson*. [The speaker said he wanted to discuss the potential of the *SWIRE* legacy survey, the largest of the surveys which are being carried out by the *Spitzer* mission. The *SWIRE* surveys consists of six areas totalling 50 square degrees at all *Spitzer* wavelengths from 3·6 to 160 microns and which overlap with other related surveys. Five of the six areas have already been catalogued with the sixth appearing next week and the final catalogue next year. Each of these fields has been chosen to coincide with other interesting areas of research.

*SWIRE* contains some 1·5 million galaxies, so photometric-redshift techniques are essential and in order to check the models it is necessary to have some spectroscopic redshifts available. In addition to optical photometric data, 3·6 and 4·5-micron *Spitzer* data are used; these sharpen up the redshift considerably, the r.m.s. of $(1+z)$ being 5%. This is a factor two better than using the Hubble Deep Field (HDF) with optical data only. Although QSOs do not have so many features, their redshifts can also be obtained.

The speaker said it was surprising how many high-redshift galaxies appear in this survey. Some 10% have $z > 2$ and 4% have $z > 3$ in the *SWIRE* data, and 20% of the galaxies are detected at 24 microns due to dust emission. Even here it is possible to make good predictions at longer wavelengths. One-third of the 24-micron objects are modelled as having dust tori. Of the highly luminous dust tori, one-third of the sample is type 1 and two-thirds type 2, whilst 5% are hyper-luminous. There are some red QSOs but no large population. The spectral-energy-distribution (SED) models can be validated because there is mid-IR *Spitzer-IRS* data for 50 objects in the N1 and N2 fields. The SEDs of sub-millimetre galaxies vary greatly in the far IR and sub-mm — there is not a homogeneous population fitted by a single SED. Several objects are fitted quite well by cirrus emission and this may just be cool dust in galaxies which are undergoing star formation — it is probably not due to mergers.

X-ray data in *SWIRE* areas corresponding to Lockman fields were then shown. All these objects are type-1 QSOs and *SWIRE* shows that they have dust tori in the IR. An interesting discovery is that of five Compton-thick AGN, found from the X-ray hardness ratio, two look like AGN and three like galaxies; the most extreme object has $z = 2 \cdot 5$ and is modelled as an extreme gas torus where the ratio of the infrared to optical luminosity is 100. It looks like an unreddened type-1 QSO in the optical but is very heavily extinguished.

This may be explained by optical light being scattered off the backside of the torus.

Turning to the subject of source counts, the speaker said that *SWIRE* is best for this down to the limit of the survey. Fainter counts are available from very deep surveys carried out by guaranteed-time teams. Old models worked quite well at 70 microns but not at 24 microns and models have been changed to fit those data. Flattening-off of the evolution at low redshift is seen so the form of evolution had to change. The model works very well at 160 microns without further fitting.

Research student Tom Babbedge has calculated 3·6- and 24-micron luminosity functions from *SWIRE* data and there appears to be good consistency with other surveys. To see what is happening at longer wavelengths the speaker said that 24-micron data can be used to model the SED, which can then be used to predict the 70-micron flux, which in turn can then be checked with *Spitzer* data. There is a bit of scatter, but essentially there is good agreement.

Looking to the future, the speaker mentioned the *Astro-F* mission working at 90 and 160 microns, and due to be launched next year. *Herschel* will be launched in 2008 to examine the 350-micron régime and will see luminous galaxies out to $z = 5$.]

*The President*. Thanks very much. We have some time for a few questions.

*Mr. M. F. Osmaston*. Are you confirming the previously-seen fall-off in QSO numbers with redshifts between 3 and 5 and beyond that?

*Professor Rowan-Robinson*. I wouldn't say that. We find plenty of quasars with photometric redshifts out to 5 and even 6. However, the accuracy of those photometric redshifts is not very high, and I wouldn't say at the moment that we are seeing any particular evidence of a drop-off in density; nor am I saying that there is any inconsistency with what's been done before. We haven't really looked very systematically at the evolution of luminosity function for different types of objects though we are working on that now.

*Professor D. Lynden-Bell*. I'm going to ask a rude question. Why, when you are plotting a colour against the luminosity, do you not do it the same way round as you would for an H–R diagram?

*Professor Rowan-Robinson*. H–R diagram? What's an H–R diagram? [Laughter.] What's the relevance of an H–R diagram?

*The President*. Maybe that's a good point at which to move on to our next speaker! [Laughter.] Thank you very much, Professor Rowan-Robinson. [Applause.] Our next speaker is Professor Jay Gallagher from Wisconsin, who will be talking about '*HST*/*ACS* observations of NGC 346: spectacular star formation in the Small Magellanic Cloud'.

*Professor J. S. Gallagher*. Well, I will explain what an H–R diagram is! [Laughter.] The Small Magellanic Cloud (SMC) represents an important opportunity to study star formation on galactic scales in a system with very different conditions than are found in the Milky Way: lower metal abundances, a diffuse and disorganized ISM, and weak gravitational tides. The SMC resembles a dwarf spheroidal galaxy with star formation going on in the middle associated with interactions in a complex, frothy ISM. This talk reports results from on-going collaborative research on the SMC that is taking a unified approach to studying the Magellanic Clouds across wavelengths and stellar populations. New instruments such as *Spitzer*, the *Australia Telescope Compact Array* (*ACTA*), *VLT*, *Gemini*, *SALT*, and *HST* would all extend traditional techniques in galactic astronomy beyond our Galaxy. For proper-motion studies we can look forward to *GAIA* and *SIM*. We want to use chemical abundances to allow us to study the flow of gas in and out of the SMC and to understand how stellar populations behave compared to those in the Milky Way.

One of the major *HST* programmes, led by A. Nota, is to study the stellar contents and ionized gas in major SMC star-forming regions. Since the SMC is nearby and suffers relatively little interstellar extinction, its stellar populations are accessible to deep imaging with *HST* and spectroscopy from the ground. Thus we can observe deeply into massive star-forming complexes, which would be heavily obscured in the Milky Way. Our *HST/ACS* optical images of sites of recent star formation in the SMC turn out to be well-matched in resolution, scale, and depth with the *GLIMPSE* inner-Galaxy survey by the *Spitzer Space Telescope*. This allows us to make a comparison with similar (but highly obscured) young star clusters in our Galaxy.

The Nota *et al*. project obtained *HST/ACS* imaging of NGC 346, the largest SMC H II region, as well as the smaller NGC 602 complex. Our study of NGC 346 is the furthest along (Nota *et al*., *ApJL*, in press). The combination of $V$, $I$, and H$\alpha$ images beautifully displays the young stars as well as surrounding older stellar populations. Despite differences in size and metallicity, the ionized gas in NGC 346 resembles that in galactic H II regions such as Orion, with young O stars clustered away from nearby clouds. Most of the optical emission comes from photoionized gas near the cloud surfaces, producing the characteristic bright edges with embedded 'pillars of creation' where less-massive protostars may lurk.

The NGC 346 Hess diagram, stellar density on an $M_V$ *vs*. $V$–$I$ colour–magnitude diagram, shows a complex array of stars. A well-populated, very young main sequence is present at intermediate and higher stellar masses, but below about 3 $M_\odot$ a second, redder sequence appears. These are candidate protostars, and are found mainly in sub-clusters located near molecular-cloud peaks and around the central collection of OB stars. The properties of the candidate young stellar objects is consistent with star formation extending over the past 3–5 million years in a region covering about 70 pc in extent. In this case we are seeing star formation occurring across a rather large spatial scale with a considerable degree of coherence.

We are working to put these results into the context of evolution in an irregular galaxy. Under these conditions, collisions between interstellar clouds could be important factors in star formation that would lead to large-scale star-formation events in the compressed cloud–cloud interfaces. Another possibility is for star formation triggered by gas in shells produced by earlier star-forming events. The marvellous *ACTA* H I maps show the SMC to be rich in gas shells and super shells, while two nearby supernova remnants could be more immediate triggers in this case.

The *ACS* photometry of stars in the NGC 346 region also turned up an historical footnote. A fainter star cluster with a regular structure lies near the projected line of sight to NGC 346. Our Hess diagrams show that this cluster has an age of ~4·2 Gyr and extends over much of our field of view. Rich star clusters with this age are rare, albeit less so in the SMC than other galaxies, but this one seems to be quite impressive. We are exploring how we might place constraints on the formation and evolution of this remarkable stellar fossil as an added bonus from our project.

*The President*. Has anyone a quick question?

*Professor R. J. Dufour*. Do your data show the metallicity change between the

4·5-billion-year-old cluster stars and the younger stars?

*Professor Gallagher*. Because the 4·5-billion-year-old cluster wasn't recognized as an interesting cluster, there are no metallicity data. But we want to look at that. In fact the intermediate-age project is to measure metallicities *versus* ages of star clusters in the SMC. Just so you know, getting star clusters right in the middle, half of the age of the Galaxy, is a nice thing to happen as it gives you leverage on the evolution. They are rare and at this redshift it's really important.

*The President*. Thank you. One more quick question.

*Mr. K. Barker*. Is the SMC metal-poor because gas is streaming out if it?

*Professor Gallagher*. Actually the models that one likes suggest it is metal-rich because gas is streaming into it. But you have to ask from where the gas is streaming in. It is impossible to make a consistent picture of the Small Magellanic Cloud in which the gas is only internal to that galaxy. You need both loss and inflow to make standard models work.

*The President*. One last question.

*Dr. Y. Tsamis*. What's the projected distance on the sky between the centre of this region and this other cluster?

*Professor Gallagher*. They are very close. It's less than a hundred parsecs, about 40–50 pc projected. I think you might be suggesting that there is perhaps a gravitational perturbation between this big cluster and NGC 346? It is possible.

*The President*. Right, I think we probably should stop there. Thank you very much for that. [Applause.] We will move on now to one of the highlights of our monthly meetings, the 2005 George Darwin Lecture, which will be given by Professor Joe Silk from Oxford University on the 'The dark side of the Universe'. [It is expected that a summary of Professor Silk's talk will appear in a future issue of *Astronomy & Geophysics*.]

*The President*. Thank you very much, Professor Silk. Does anyone have any questions?

*Mr. H. Regnart*. Can you tell me the extent to which dark matter may cluster or concentrate in the centre of galaxies? And whether massive black holes, for example, in galactic centres are thought to be part of baryonic matter because presumably that is what they came from?

*Professor Silk*. Well, what we know pretty much for sure is that one can actually measure the black holes in the centres of many galaxies. Presumably they are made of baryons; we are almost certain but we can't prove they are. I would even say they must be, but it really doesn't change things very much. There was a cloud of dark matter which, we believe, had some concentration from which the baryons condensed to make the Milky Way and other galaxies too. Most likely though, when you go into the inner galaxy, it's all baryon dominated. In our Galaxy, anyway, the dark matter is a very small fraction of the inner galaxy.

*Professor J. Beckman*. Would it be possible that excess *EGRET* sources in the middle of galaxies represent dark-matter decay at a rate sufficient to account for the absence of dark matter cusps at the centre of galaxies?

*Professor Silk*. It's annihilating, we think. But the annihilation rate is so weak that it doesn't get rid of it. A typical particle will take millions of Hubble times to annihilate with another, so it really doesn't count significantly.

*Mr. C. J. Lintott*. Do you have a feel for how many supernovae are needed to provide necessary feedback and what this translates to in terms of star-formation rate and when this would have had to happen?

*Professor Silk*. I think that it's pretty clear that the standard mixture of stars with the initial mass function taken from the one measured and advocated to be universal will certainly give you enough supernovae to blow the gas away and give you strong feedback in dwarf galaxies. It fails significantly in the Milky Way and in more massive galaxies, and so I think that at that point you might need something else, which may be exotic, such as a supermassive black hole. The data could also be advocating a population of early massive stars but, although all of those are in the bounds of possibility, the only way to test this is to look into the very deep Universe with very big telescopes until you actually see them.

*Mr. R. Barber*. The case for dark energy seems not to be very convincing. At $z = 4$, the error bars are quite large and the number of observations is not very great. Also it must be the case that, looking back in the early Universe, which is quite different from today — lower metallicity, for instance — there are physical reasons why the observed supernovae are less bright than we might expect them to be. How do you rate the chance that in due course people might revisit this?

*Professor Silk*. As far as the dark energy goes, what we observe is that it's only become important recently, so between now and redshift $z = 1$ is the period where acceleration changes. As you look further back, there is no evidence of it at all. So it's a very recent phenomenon. There the error bars are small provided you interpret supernovae as being reliable distance indicators, and that's an issue that worries many of my colleagues. It is a significant look-back time in the past; things could be different, there could be intrinsic effects. I don't think we really have completely sorted out that issue. All I can add, though, is that if it were the supernovae that were the only indicator, it would certainly not be so convincing, but we have the microwave background and we have the large-scale structure. Between them, they indicate the need for dark energy. Dark matter cannot explain the flatness of the Universe: we need something else. The only culprit seems to be dark energy.

*The President*. Any other questions or comments?

*Dr. M. Cropper*. Are there any other inputs from, for example, when the electro-weak symmetry-breaking occurred or when gravity symmetry-breaking occurred, that could have caused inflationary effects?

*Professor Silk*. Electro-weak inflation? Or even a late phase transition? I think people speculate that a very late inflation, the one observed to be associated with dark energy, is associated with a mass scale that coincidentally agrees more or less with the inferred mass of the neutrino. It is possible that other things could be acting there too. There are simply no theories to my knowledge.

*Professor I. P. Wright*. What proportion of dark matter do you think could just be ordinary matter that we haven't yet seen?

*Professor Silk*. I think that's a difficult one to answer because you have to have faith in our indicators of the baryonic matter. Given the fact that we have these three different ways of measuring the baryon fraction, we get a lower limit on the baryons. There certainly is ten times more dark matter. I think you really have to think of ways to produce weird types of baryons that would not be counted in these experiments. But that's not impossible.

*The President*. One last question!

*Dr. D. McNally*. In October, Tim de Zeeuw showed that there was at least one galaxy that had been recently detected which didn't seem to require dark matter to sustain its rotation. You just said that in our Galaxy the dark-matter content might not be very great. Why should dark matter have lacunae like this around the Universe?

*Professor Silk*. The trouble is that this is not a simple issue. For example, when you measure rotation curves to look for dark matter you have to make assumptions that the orbits are circular. So every now and then, if you have a sample of a hundred galaxies you'll get one or two with some bizarre property, where you're getting a biassed result, and so one has to be very wary about this. There are various arguments that may be to do with the light of dim stars, for instance. On the other hand, it's also true that there are theories which can explain the rotation curve as well as the conventional Einstein theory, such as modified gravity theories, and so on. Then there are the theories with no dark matter at all. We are by no means in the clear. The vast majority of galaxies show very strong evidence of dark matter.

*The President*. Thank you once again. [Applause.] The next monthly A&G Ordinary Meeting will be held on Friday, January 13 next year. I'd like to finish by wishing everyone a happy Christmas and a very prosperous New Year.

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# THE VISIBLE AND NEAR-INFRARED ATMOSPHERIC EXTINCTION AT THE CANARY ISLANDS' INTERNATIONAL OBSERVATORIES — PAPER II

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We present the results of five years of multicolour monitoring of the visible and near-infrared extinction at Teide Observatory (TO, Tenerife) and the Roque de los Muchachos Observatory (RMO, La Palma) as part of an on-going photometric calibration programme. Results are presented in $BVRIJHK_{short}K$ for a total of 156 nights of observations made in photometric conditions between 2000 and 2005, extending our previously published results in time and into the near-infrared. The median extinction in $V$ is found to be 0·132 mags./airmass, similar to the values found by previous studies. In the near-infrared the extinction is found to be strongly dependent on the passband used, with the highest median extinction (0·118 mags./airmass) in $J$, in which water-vapour extinction is significant, and the lowest (0·046 mags./airmass) in the extremely clean $H$ window. We find a median extinction in $K_{short}$ approximately half that in the standard broadband $K$ filter. We examine the variation of the extinction with wavelength as a function of the level of extinction and confirm previous results that suggest that the wavelength dependence of the extinction peaks in $I$. We also find that there is a deep minimum in the wavelength dependence in $H$, the band least affected by increases in the aerosol component of the atmospheric extinction.

*Introduction*

The photometric extinction is one of the fundamental parameters used to define the quality of an astronomical observatory. In the Canary Islands a long-term programme of monitoring of the visible extinction has been one of the by-products of the work of the *Carlsberg Automatic Meridian Circle* (*CAMC*) at the RMO. The *CAMC* results have shown that conditions are generally excellent, although there are occasional isolated epochs of wind-blown Saharan dust (*calima*) that are generally limited to the summer months. Various studies have been carried out, both of the overall atmospheric extinction and the effects of an enhanced aerosol component[1–4] that have shown that the aerosol component does not normally affect the photometric quality of a night in the sense of making a night non-photometric. However, multicolour photometry taken with the *Jacobus Kapteyn Telescope* (*JKT*)[1] has suggested that the aerosol component of the extinction in the Canary Islands, which is composed of a mixture of wind-blown dust (both volcanic and — mainly, but not exclusively — surface dust from the African continent), sea salt, and smoke in variable proportions, is not exactly grey, showing a maximum in the *I* band. Similarly, an examination of the near-infrared extinction[2] and a comparison of the infrared extinction at the TO and the visible extinction measured by the *CAMC*[3] has shown that, while the conditions at the TO and RMO are strongly correlated and the visible and near-infrared extinction are strongly correlated, it is evident that not all bands respond in identical fashion to an increase in the aerosol component of the extinction. This is an important result that we confirm and will examine in more detail in this paper.

Here, we examine the greyness of the aerosol component in more detail with the benefit of data covering a broader wavelength range from 0·44–2·21 microns. We will see that the results broadly confirm previously published data. However, the study published by Kidger[3] was, in part, severely affected by the greatly increased background aerosol component resulting from the major eruption of Mount Pinatubo in 1992, which logically has different dust properties to *calima*, whereas the results reported here are unaffected by major episodes of wind-blown volcanic dust and thus are closer to the normal observing conditions in the Canary Islands.

*Methodology*

Visible observations were taken on 19 nights with the 1-m *JKT*. Of these, ten nights used the SiTE-1 detector and nine used the SiTE-2 detector. A standard filter set was used for all observations at the RMO, *B27*, *V30*, *R37*, and *I44* filters being employed on all nights except the first two when the *V33* filter, which has almost identical characteristics to *V33*, was used. The characteristics of the detectors can be found at the url: http://www.ing.iac.es/Engineering/engweb6a.htm, and of the filters at the url: http://www.ing.iac.es/~quality/filter/newfiltdoc.html. The remaining visible observations — 104 nights — were taken with the 0·82-m *IAC-80 Telescope* (*IAC-80*) at the TO, using the standard CCD camera with a standard filter set. Filter data for the *IAC-80* can be found at the url: http://www.iac.es/telescopes/tcs/filtros.htm#FiltrosIAC80. The response curve of the *IAC-80*'s Thomson chip is available in Martín-Luis[4].

The methodology of the visible observations has been covered in some detail by Kidger *et al.*[5], while additional details are given by González-Pérez *et al.*[6]. As precise and well-calibrated photometry was the aim of our observations, large numbers of Landolt stars[7,8], observed at frequent intervals and at the widest possible range of airmass, were used to calibrate the data, which were reduced using standard IRAF packages. Results from nights found on reduction to be non-photometric or highly doubtful were rejected. A median of 48 measures of individual stars in a median of four different Landolt fields (excluding repeat observations of an individual field) were used to calibrate the data each night*; taking observations of multiple fields allows us to define the photometric zero-point of the night with the maximum accuracy, while the widest possible range of airmass is needed to solve for the extinction. This allows us to determine a complete solution to the photometric calibration for each night (see section §3 of ref. 6).

Note that we find that a linear regression between the instrumental magnitude and the airmass gives us a good fit to the data in all filters; although the presence of strong molecular bands may cause curvature in the regression line, it is evident that the effect is not found by us to be strong enough to be significant in any filter (the extinction is always fitted interactively in the reduction). The slope of the regression line gives us the extinction in magnitudes per airmass and the intercept the photometric zero point (the amount that must be added to the measured instrumental magnitude to give the magnitude of a star outside the atmosphere).

All nights were examined for the possible presence of a temporal term in the extinction and one was added as necessary. (N.B. For the highest quality of data reduction it is almost always necessary to include a temporal term of extinction, particularly in the near-infrared where humidity changes ensure that >95% of nights give a better fit to the calibration data if a temporal term is included.)

The median error in the calculated extinction in $V$ for a single night in our data is found to be 0·011 magnitudes/airmass, with the median error in the calculated extinction for a single night in all four visible bands in the range 0·011–0·014 mags./airmass.

The infrared observations were taken on 41 nights using the *CAIN* infrared camera on the 1·52-m *Carlos Sánchez Telescope* (*CST*) at the TO. *CAIN* has a 1–2·5 micron *NICMOS*-type detector and is equipped with 'standard' near-infrared filters. A description of the instrument can be found at the url: http://www.iac.es/telescopes/cain/cain_datos_tecnicos.html. The same $J$ and $H$ filters were used on all nights. On 28 nights the $K_{short}$ filter was used; this covers the best part of the 2-micron window and has a long-wavelength cut-off at shorter wavelength than the standard $K$ filter, thus reducing the thermal background and the region of reduced transparency close to the long-wavelength end of the 2-micron window. On the remaining 13 nights a standard $K$ filter was used. The filter passbands are shown at the url: http://www.iac.es/telescopes/cain/3.2.html. The fact that we use both the $K$ and the $K_{short}$ filter allows us to compare the atmospheric conditions in both bands. The median error in the determination of the extinction on a single night varies from 0·011 mags./airmass in $H$, to 0·023 mags./airmass in $K$.

*Note that the median number of stars used is influenced by the fact that there was a significant proportion of nights where only half the night was scheduled for observations in our programme or, alternatively, for operational reasons half or less of the night could be used. If only full nights of observing are considered, the median number of stars used to calibrate would be about 25% higher.

In the near-infrared we use the extensive listing of infrared standard stars for Teide Observatory tied to the zero-point defined by Vega[9] to calibrate our data. The precision of these measurements and their link to the Vega scale has been verified by Cohen *et al.*[10], who find that the zero-point of the photometric scale of the stars is accurate to $\leq 0 \cdot 1\%$. For this calibration process we only use the subset of fainter stars in our sample that are limited to $K > 7$ and can be observed on the linear section of the camera. We note that the photometric scale defined by the stars by Kidger & Martín-Luis[9] has been checked against the widely used *UKIRT* Faint Standard list[11] and Arcetri list[12] and the scales are found to have an identical zero-point to $\leq 1\%$[13]. A method similar to that for the visible observations was used, with large numbers of calibration measures of different stars being taken at the widest possible range of airmass and at multiple epochs through the night; a high-airmass star and a low-airmass star were always taken at the start of observations and at a minimum of one other time during the night. As with visible observations, every effort was made to take at least one standard star as close to the telescope horizon limit ($2 \cdot 3$ airmasses) as possible, with at least one further star being observed at ~2 airmasses during the night. Further details of our infrared observing method can be found in González-Pérez *et al.*[6].

TABLE I

*Number of nights of observation in each filter reported in the text.*

| Filter | $B$ | $V$ | $R$ | $I$ | $J$ | $H$ | $K_{short}$ | $K$ |
|---|---|---|---|---|---|---|---|---|
| No. nights | 125 | 125 | 125 | 125 | 41 | 41 | 28 | 13 |

*Results*

The number of nights of data in each filter are shown in Table I. Note that we have smaller quantities of data in the infrared than in the visible and this should be borne in mind when assessing the statistical results in this study. However, the number of nights of data in the $J$ and $H$ bands is large enough for the results to be strongly significant provided that no important selection effects are involved. Note that the restriction at TO in dusty conditions is that observations may be carried out provided that Mount Teide is visible when backlit in the afternoon; this semi-empirical criterion means that observing will continue even in conditions of moderate to strong dust in suspension, so there is no obvious strong intrinsic bias in our statistics due to selection effects of not observing in non-optimum conditions†.

The extinction data are shown in graphical form in Fig. 1. To reduce confusion, the data are plotted for just the $V$ and $H$ bands. We present a statistical summary of the data in Table II, where we give the median, upper and lower quartiles, upper and lower deciles, and number of points for each band. The split of the data at $2 \cdot 2$ microns into broadband $K$ and $K_{short}$ reduces the amount of data in each band and thus considerable caution must be applied

†As our observing programme involves the observation of relatively bright stars at all Right Ascensions, we can observe at all epochs of the year, even with dust and at full moon, provided that conditions are photometric. As our programme has no strong scheduling constraints it was thus subject only to telescope availability, with more nights being scheduled when there were fewer time-critical programmes. Our internal project rules were that nights with dust were to be regarded as photometric and used as such, although with a strong recommendation to take extra calibration measures as a guard against variable extinction during the night.

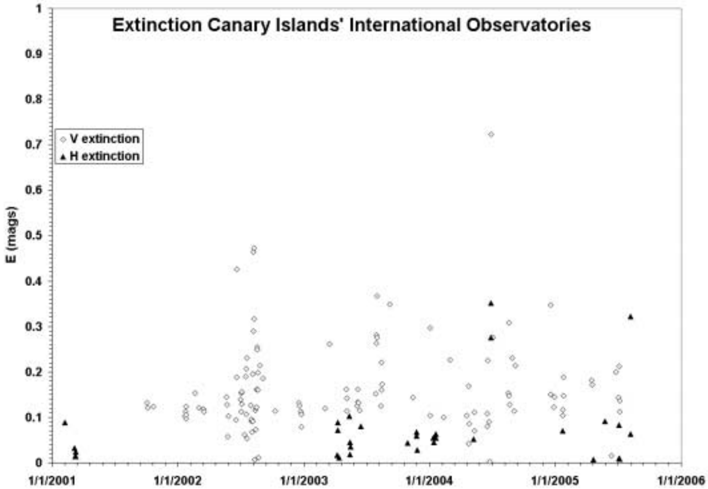


FIG. 1

The variation of the calculated extinction in our data in $V$ (open diamonds) and $H$ (filled triangles). Only these two bands are presented to reduce confusion in the graph. In both, the confinement of the majority of the data to a relatively narrow band of values is evident, as is the correlation between the visible and the infrared extinction.

TABLE II

*The statistics of the extinction in each filter for the data reported in the text. For each filter we give successively the upper decile (i.e., 90% of measurements have this value or smaller), the upper quartile (75% of points have this value or smaller), median, lower quartile, and lower decile, respectively.*

| | Upper-decile Extinction | Upper-quartile Extinction | Median Extinction | Lower-quartile Extinction | Lower-decile Extinction |
|---|---|---|---|---|---|
| $B$ | 0·353 | 0·277 | 0·220 | 0·200 | 0·178 |
| $V$ | 0·276 | 0·189 | 0·132 | 0·107 | 0·074 |
| $R$ | 0·255 | 0·146 | 0·098 | 0·073 | 0·053 |
| $I$ | 0·235 | 0·111 | 0·064 | 0·036 | 0·002 |
| $J$ | 0·234 | 0·142 | 0·116 | 0·098 | 0·070 |
| $H$ | 0·091 | 0·072 | 0·045 | 0·023 | 0·010 |
| $K_{short}$ | 0·115 | 0·089 | 0·050 | 0·024 | 0·024 |
| $K$ | 0·313 | 0·115 | 0·077 | 0·042 | 0·011 |

when dealing with the statistics; in particular, where small numbers of data points are involved the decile may have little or no significance.

The median is preferred in statistical treatment of the data as it is less affected than the mean by either skewing of the data distribution, or by the presence of a small number of outlying values. Similarly, the quartiles give a two-tailed measure of the expectation range of the data that is more informative than the standard deviation, which is one-tailed and thus gives no information about the skew of the distribution.

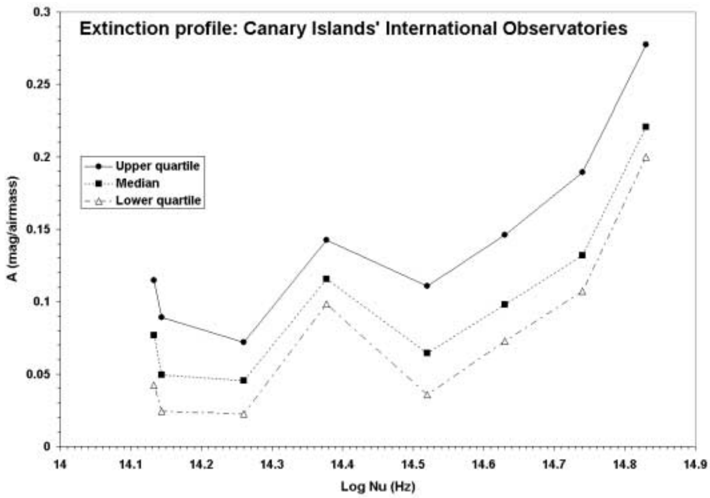


FIG. 2

The median profile of the extinction at the Canary Islands' International Observatories against the logarithm of the passband frequency. The graph is obtained by plotting the median value obtained for each filter. The upper and lower quartile values are plotted for comparison; these define a region within which the 50% expectation value for the extinction lies. Note the peak in the *J* window, which is affected by the presence of narrow water-vapour bands within the window, and the significant difference between the extinction in broadband *K* and the narrower $K_{short}$ filter that only uses the best part of the 2-micron window.

The data are represented graphically in Fig. 2, where we show the frequency dependence of the values of the extinction found in Table II. The total atmospheric extinction is a combination of various components: the Rayleigh-scattering component, which shows a strong frequency dependence with greater scattering at higher frequencies; the intrinsic absorption component, from atmospheric absorption features, which may be effectively constant (*e.g.*, strongly saturated lines of atmospheric gases such as $CO_2$, or the oxygen absorption lines), or time variable on short ($H_2O$) or longer timescales (*e.g.*, $SO_2$, $O_3$, $CH_4$); and the aerosol component. To a first approximation, in the majority of the bands investigated here, the Rayleigh and the aerosol components are dominant, although the variable level of water vapour in the atmosphere has an influence in the extinction in the *J*, *K*, and *I* windows in decreasing order of importance. We thus expect to see a frequency-dependent fall in the measured atmospheric extinction towards low frequencies, with the values tending to an asymptotic limit defined by the aerosol extinction (in the absence of significant atmospheric absorptions). This is what is seen broadly in the data, although there is an important maximum in the *J* window (1·2 microns) in which there are many narrow water-vapour absorptions, making this the least clean of the three main near-infrared windows.

The results give a median extinction of 0·132 mags./airmass in *V* and 0·045 mags./airmass in *H*. These values are close to the ones reported in previous studies (0·127 mags./airmass in *V* in Paper I (ref. 5)) and imply that in *V* the median level of atmospheric extinction consists of approximately equal amounts of Rayleigh scattering and of aerosol extinction. Note that the extinction in *V* tends asymptotically to the limit defined by Rayleigh scattering, with the lower quartile extinction being 0·107 mags./airmass and the lower decile 0·074 mags./airmass.

A concern in our data is the effect that uneven seasonal sampling may have on the calculated statistics; as we have more data in the summer months when telescope time on the *IAC-80* is undersubscribed, there is a possibility that our statistics may be biassed if the extinction is systematically different during the summer. To check this possibility we have divided the data by season and compared the statistics from season to season. To this end we define January to March inclusive as winter, April to June as spring, *etc*. The results are shown in Table III. In *V*, *R*, and *I* we find a slight increase in the median extinction in the summer months; no increase is detectable in *B*, or in the infrared bands, although we caution that there is insufficient infrared data in the summer to come to firm conclusions. The lower quartile of extinction values is stable throughout the year, thus there is the same expectation of very low extinction in the visible at any time of the year. Only in the upper quartile is a strong seasonal effect seen. While there is no significant difference in the value of the upper quartile of the visible extinction between the autumn, winter, and spring, the value of the upper quartile is between 50% larger and double, according to band, in the summer. In other words, the principal difference in the summer is that the extinction values are more strongly skewed to large values, although the central value of the distribution of values remains virtually unchanged.

A further test of the validity of our statistics is to compare the global statistics with the nights when we have simultaneous *BVRIJHK* data at Teide Observatory. On five nights we have simultaneous extinction data in the visible

TABLE III

*Seasonal extinction statistics for each filter. For the purposes of this table, winter is defined as the months from January to March; spring, April to June; summer, July to September; and autumn, October to December.*

| | *B* | *V* | *R* | *I* | *J* | *H* | *K* | $K_{short}$ |
|---|---|---|---|---|---|---|---|---|
| Winter | | | | | | | | |
| Upper quartile | 0·229 | 0·150 | 0·099 | 0·080 | 0·135 | 0·061 | | 0·088 |
| Median | 0·210 | 0·118 | 0·082 | 0·053 | 0·109 | 0·050 | 0·024 | 0·082 |
| Lower quartile | 0·188 | 0·105 | 0·075 | 0·041 | 0·099 | 0·026 | | 0·068 |
| *n* | 19 | 19 | 19 | 19 | 10 | 10 | 1 | 9 |
| Spring | | | | | | | | |
| Upper quartile | 0·259 | 0·164 | 0·120 | 0·090 | 0·214 | 0·087 | 0·092 | 0·103 |
| Median | 0·228 | 0·120 | 0·093 | 0·060 | 0·122 | 0·049 | 0·050 | 0·083 |
| Lower quartile | 0·201 | 0·088 | 0·061 | 0·011 | 0·107 | 0·035 | 0·026 | 0·078 |
| *n* | 32 | 32 | 32 | 32 | 18 | 18 | 10 | 8 |
| Summer | | | | | | | | |
| Upper quartile | 0·336 | 0·231 | 0·208 | 0·163 | 0·149 | 0·074 | 0·314 | 0·040 |
| Median | 0·234 | 0·156 | 0·112 | 0·085 | 0·070 | 0·010 | 0·225 | 0·030 |
| Lower quartile | 0·205 | 0·115 | 0·083 | 0·045 | 0·032 | 0·000 | 0·136 | 0·010 |
| *n* | 57 | 57 | 57 | 57 | 7 | 7 | 2 | 5 |
| Autumn | | | | | | | | |
| Upper quartile | 0·248 | 0·138 | 0·114 | 0·070 | 0·142 | 0·066 | | 0·097 |
| Median | 0·210 | 0·124 | 0·104 | 0·062 | 0·133 | 0·052 | | 0·059 |
| Lower quartile | 0·193 | 0·114 | 0·081 | 0·037 | 0·094 | 0·044 | | 0·046 |
| *n* | 15 | 15 | 15 | 15 | 6 | 6 | 0 | 6 |

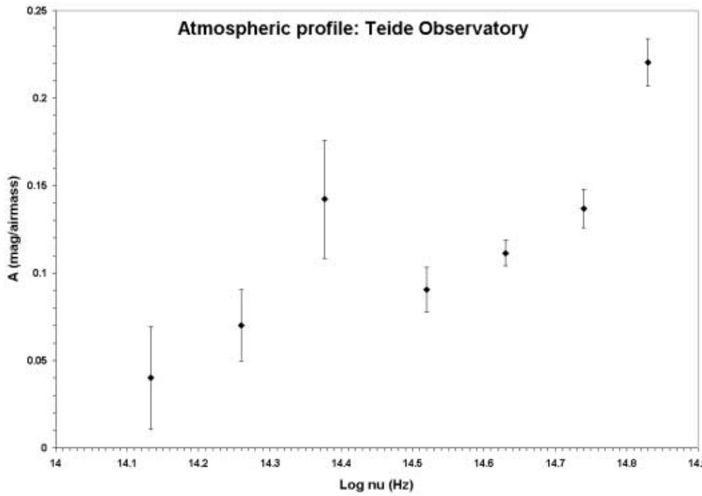


FIG. 3

The median extinction in each band for the five nights when we have simultaneous *BVRIJHK* extinction data from the *IAC-80* and *CST*. The error bars are the median error in the extinction determination for each band for these five nights of simultaneous data. Note that the form of the frequency dependence in the simultaneous data is identical to that found in Fig. 2 from the median of all data, simultaneous and non-simultaneous, as is the level of the curve.

and near-infrared taken on the *IAC-80* and the *CST*, which are separated by approximately 50 m and are at virtually the same altitude and thus can be considered to have an identical atmosphere. In Fig. 3 we show the atmospheric extinction profile that we obtain by taking the median calculated extinction in each band for the five nights of simultaneous data. The error bars shown are the median error in the determination of the extinction for each band in those five nights.

Both the shape of the curve and its frequency dependence and the absolute level of the curve are close to those obtained from the median of all data presented in Fig. 2. This leads us to believe that the data that we present here represent an almost unbiassed sub-set of the global atmospheric conditions in the Canary Islands.

One of the most interesting questions about the conditions in the Canary Islands' International Observatories is the dependence of the extinction on the level of the aerosol component and the greyness of this component. This is examined in Fig. 4, where we plot the difference between the upper and lower quartile of the extinction (lower curve) and the upper and lower decile (upper curve) as a function of frequency. The differences in each band and their variation as a function of frequency are an indicator of the greyness of the aerosol component of the extinction. If the aerosol component is grey we would expect to see that the extinction curves at different levels of aerosol component would be parallel and horizontal. We see that at the quartile level (*i.e.*, 50% expectation range of values) the curve is almost flat, thus at this level the extinction is grey and hence an increase or decrease in one band is correlated to a similar increase or decrease in the others. In contrast, the decile curve (the 80% range of expectation of values) shows a strong wavelength dependence, with a maximum

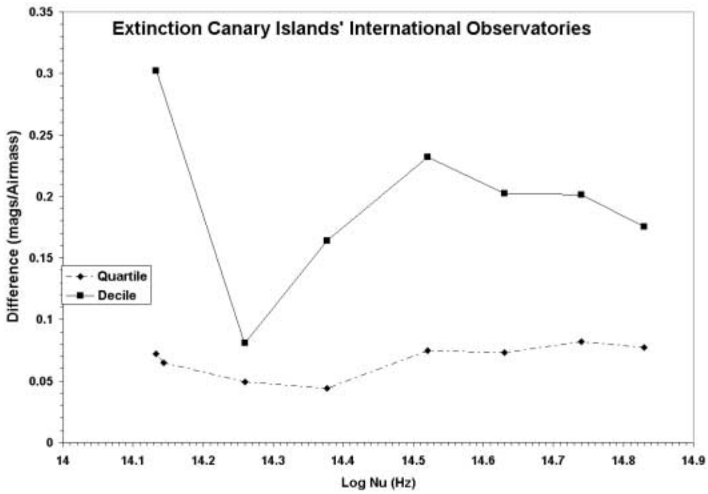


FIG. 4

The difference between the upper and lower quartile of the extinction (lower curve) and the upper and lower decile (upper curve) as a function of frequency. This graph is an indicator of the greyness of the extinction. We see that at the quartile level (*i.e.*, 50% expectation range of values) the curve is almost flat, thus at this level the extinction is grey and so an increase or decrease in one band is correlated to a similar increase or decrease in the others. In contrast, for the decile curve (the 80% range of expectation of values) a strong wavelength dependence is evident with a maximum in the *I* band. This curve confirms the suggestion first made by Stickland *et al.*[1] that the increase in the extinction at the RMO shows a colour dependence with a peak at *I* that is correlated with the typical size of the aerosol grains. Note also that the extinction in *H* is strongly confined to a relatively narrow band of values and thus this band is relatively little affected by increases in the aerosol component of extinction. The peak in $K_{short}$ is of uncertain significance due to the rather small number of measurements in this band.

in the *I* band and a deep minimum in *H* that indicates that at 1·6 microns the atmospheric extinction is strongly confined to a narrow range of values. The smoothness of this curve demonstrates that the dependence is not due to random statistical fluctuation and confirms the suggestion first made by Stickland *et al.*[1] that the aerosol component of the extinction in the RMO shows a colour dependence with a peak at *I* that is correlated with the typical size of the aerosol grains. We caution, though, that the apparent peak in $K_{short}$ is of uncertain significance due to the rather small number of measurements in this band, although it is consistent with the expected thermal signature of a warm atmospheric aerosol component in epochs of high extinction.

The results in Fig. 4 have interesting implications for observations in epochs of an enhanced aerosol component of the extinction. They suggest that even in epochs of high visible extinction the conditions at 1·6 microns are only affected to a comparatively small degree and thus the limitation of observations to this band is indicated for periods of high extinction. In contrast, the *I* band and possibly the 2-micron window are more severely affected by enhanced aerosol extinction than others, and thus we suggest that observations in these bands should be discouraged during periods of *calima*.

However, it is important to reaffirm that, despite popular belief to the contrary, nights with *calima* are not, *per se*, non-photometric. We find that a good fit can be obtained to the extinction even when the visible extinction is 3–4 times its normal level, provided that elementary rules of photometric calibration are followed: measure sufficient calibration stars at the widest possible range of airmass and ensure that the measures are well enough distributed in time to allow the temporal term in the extinction to be determined. This last is particularly important in conditions of calm when the dust layer may settle during the night, or when dust is arriving or dispersing, but is good practice on any night.

### *Conclusions*

We have presented visible and near-infrared extinction data for 156 nights of observations at Canary Islands' International Observatories, taken between 2000 and 2005. The results confirm and expand the conclusions of previous studies. Only a small increase in the median visible extinction is detected during the summer months and the expectation of a low level of atmospheric extinction is no different in winter and summer months. The best atmospheric conditions are found in *H* in the near-infrared and, at epochs when the aerosol component of extinction is not enhanced, in *I*. In contrast, *J* and *B*, respectively, give consistently the highest extinction in the near-infrared and the visible.

We find that the aerosol component of extinction at the Observatories is not grey but instead shows a strongly peaked frequency dependence with its maximum around one micron. This suggests that observations around these wavelengths are not advisable in conditions of strongly enhanced extinction as they will be more seriously affected than measurements at shorter wavelengths. In contrast, the extinction in the *H* band at 1·6 microns shows only a relatively weak correlation with the visible extinction, thus, even on nights when the visible extinction is much greater than normal, there is only a small increase in the extinction in *H*, so observing in this band rather than *J* or *K* is strongly recommended during nights of greatly increased aerosol extinction.

However, even though the aerosol component of extinction is not grey, this has no effect on the photometric quality of the night. Even during epochs of *calima* the conditions will be photometric provided that the dust is not strongly layered and no cloud is present. Even a level of extinction 3–4 times normal does not affect the photometric calibration provided that the standard rules for a good photometric calibration are followed and sufficient calibration stars are observed to ensure that the temporal term of the atmospheric extinction can be calculated reliably.

### *Acknowledgements*

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# HAMILTON'S ECCENTRICITY VECTOR GENERALIZED TO NEWTON WONDERS


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The vectorial velocity is given as a function of the position of a particle in orbit when a Newtonian central force is supplemented by an inverse-cubic force as in Newton's theorem on revolving orbits. Such expressions are useful in fitting orbits to radial velocities of orbital streams. The Hamilton–Laplace–Runge–Lenz eccentricity vector is generalized to give a constant of the motion for these systems and an approximate constant for orbits in general central potentials. A related vector is found for Hooke's centred ellipse.


## *Introduction*

In a central orbit a particle with position $(r,\tilde{\phi})$ has $r^2 d\tilde{\phi}/dt = \tilde{h}$ constant. Newton[1] pointed out that if $\phi = \tilde{\phi}/n$, with $n$ any constant, then $r^2 d\phi/dt = h$ is also constant. He then enquired what extra radial force would be needed to make a new orbit with $\phi$ replacing $\tilde{\phi}$ but with the $r(t)$ unchanged. Evidently $h = \tilde{h}/n$ so the old radial equation of motion $d^2r/dt^2 - \tilde{h}^2 r^{-3} = \tilde{F}$ will change to $d^2r/dt^2 - h^2 r^{-3} = F$.

Thus the extra radial force required is $F - \tilde{F} = -(n^{-2} - 1)\, \tilde{h}^2/r^3$, which is outward or inward according as $n$ is greater or less than one. If the new motion were observed from axes that rotate (non-uniformly) at the rate $\boldsymbol{\Omega} = (n^{-1} - 1)\, \dot{\tilde{\phi}}$, then one would see the particle perform the original orbit unchanged. Thus from fixed axes the whole orbit may be thought of as revolving with angular velocity $(n^{-1} - 1)\, \dot{\tilde{\phi}} = (1 - n)\, \dot{\phi}$. This is Newton's theorem on revolving orbits. Newton used it to demonstrate the accuracy of the inverse-square law in the Solar System; notice, however, that the theorem holds for orbits subject to any central force $\tilde{F}$. Chandrasekhar[2] gives an elegant discussion but see Lynden-Bell & Lynden-Bell[3] for the true shapes of Newton's orbits in uniformly rotating axes.

Hereafter we specialize to an inverse-square law supplemented by an inverse-cubic force, so the potentials considered take the form

$$\psi = \mu r^{-1} + \frac{1}{2} K r^{-2},$$

where $\mu$ may be thought of as $GM$ and $K$ is a constant, which in galactic orbits is normally negative though in Hartree-Fock atoms it is positive. The equation of motion in fixed axes is

$$d^2\mathbf{r}/dt^2 = -(\mu r^{-2} + K r^{-3})\,\hat{\mathbf{r}},$$

where 'hats' denote unit vectors.

Comparison with Newton's theorem on revolving orbits suggests that we think of the angular momentum $\mathbf{h}$ as $n^{-1}\tilde{\mathbf{h}}$ and $K$ as $(n^{-2}-1)\,\tilde{h}^2 = (1-n^2)h^2$. If we go to axes rotating non-uniformly with angular velocity $(n^{-1}-1)\,\tilde{\mathbf{h}}/r^2$, the orbital equations relative to the rotating axes must reduce to those under the action of the inverse-square law alone. In practice it is $h$ and $K$ that are known, so in terms of those $n^2 = 1 - Kh^{-2}$. Should $K$ be greater than $h^2$ we would be in trouble, but under such circumstances the inverse-cube attraction overcomes the centrifugal repulsion so orbits spiral into the origin. Otherwise in the non-uniformly rotating axes with $\boldsymbol{\Omega} = (1-\sqrt{1-Kh^{-2}})\,\mathbf{h}/r^2$ we recover, writing a dot for time derivatives relative to the rotating axes,

$$\ddot{\mathbf{r}} = -\mu r^{-2}\hat{\mathbf{r}}. \tag{1}$$

We have indeed laboriously checked that with this $\boldsymbol{\Omega}$ the $2\,\boldsymbol{\Omega}\times\dot{\mathbf{r}} + \dot{\boldsymbol{\Omega}}\times\mathbf{r} + \boldsymbol{\Omega}\times(\boldsymbol{\Omega}\times\mathbf{r})$ terms cancel out with the $-K\hat{\mathbf{r}}/r^3$ force term.

*The orbit seen in the rotating axes*

From (1)

$$\mathbf{r}\times\dot{\mathbf{r}} = \tilde{\mathbf{h}},$$

a constant, and

$$\ddot{\mathbf{r}}\times\tilde{\mathbf{h}} = -\mu\hat{\mathbf{r}}\times\left(\hat{\mathbf{r}}\times\frac{\dot{\mathbf{r}}}{r}\right) = \mu\dot{\hat{\mathbf{r}}},$$

thus

$$\dot{\mathbf{r}}\times\tilde{\mathbf{h}} = \mu(\hat{\mathbf{r}} + \tilde{\mathbf{e}}), \tag{2}$$

where $\tilde{\mathbf{e}}$ is a constant vector *fixed in these rotating axes*. Since both the other terms are perpendicular to $\tilde{\mathbf{h}}$, $\tilde{\mathbf{e}}$ must be too, so it lies in the plane of the orbit. Dotting with $\hat{\mathbf{r}}/\mu$ we find setting $l = \tilde{h}^2/\mu$,

$$l/r = (1 + \tilde{\mathbf{e}}.\hat{\mathbf{r}}) = 1 + \tilde{e}\cos\tilde{\phi}. \tag{3}$$

This is the equation of a conic of eccentricity $\tilde{\mathbf{e}}$ and semi-latus-rectum $l$, and $\tilde{\phi}$ is the angle in the rotating axes measured from perihelion. Thus $\tilde{\mathbf{e}}$ points toward perihelion in the rotating axes and it will inevitably rotate relative to fixed axes. Since the axes rotate at the rate $(n^{-1}-1)\tilde{h}/r^2=(1-n)\dot{\phi}$ we find

$$\tilde{\mathbf{e}}=\mathbf{e}\cos[(1-n)\phi]+\hat{\mathbf{h}}\times\mathbf{e}\sin[(1-n)\phi], \tag{4}$$

where $\mathbf{e}$ is in the fixed absolute direction to the perihelion at which $\phi=0$ and its magnitude is the eccentricity $|\tilde{\mathbf{e}}|$. For fixed axes this comes from Hamilton[4]. For the contributions of Laplace, Runge & Lenz see Goldstein[5].

*The orbit seen from fixed axes*

The equation of the orbit (3) is put into fixed axes by writing $\tilde{\phi}=n\phi$, $l=n^2h^2/\mu$:

$$l/r=1+e\cos n\phi. \tag{5}$$

The velocity in fixed axes will be

$$\mathbf{v}=\dot{\mathbf{r}}+\boldsymbol{\Omega}\times\mathbf{r},$$

where from (2)

$$\dot{\mathbf{r}}=\frac{\mu}{nh}\hat{\mathbf{h}}\times(\hat{\mathbf{r}}+\tilde{\mathbf{e}}).$$

Thus using (4) for $\tilde{\mathbf{e}}$ and $\boldsymbol{\Omega}=(1-n)\dot{\phi}\hat{\mathbf{h}}=(1-n)\mathbf{h}/r^2$ we find our expression for $\mathbf{v}$:

$$\mathbf{v}=\frac{\mu}{nh}\left\{\hat{\mathbf{h}}\times\hat{\mathbf{r}}\left[1+\frac{l}{r}(n^{-1}-1)\right]+\hat{\mathbf{h}}\times\mathbf{e}\cos[(1-n)\phi]-\mathbf{e}\sin[(1-n)\phi]\right\}, \tag{6}$$

or using (5) to eliminate $l/r$ in favour of $\phi$,

$$\mathbf{v}=\frac{\mu}{nh}\left\{\hat{\mathbf{h}}\times\hat{\mathbf{r}}\left[n^{-1}+(n^{-1}-1)\,e\cos(n\phi)\right]+\hat{\mathbf{h}}\times\mathbf{e}\cos[(1-n)\phi]-\mathbf{e}\sin[(1-n)\phi]\right\}. \tag{7}$$

To get the radial velocity along any line of sight $\hat{\mathbf{l}}$, one merely uses $\hat{\mathbf{l}}.\mathbf{v}$ and then corrects for the Sun's motion.

*The eccentricity vector constant of the motion*

Equations (6) or (7) give us the velocity in terms of a conserved constant vector $\mathbf{e}$ pointing toward the initial perihelion. To get $\mathbf{e}$ itself we need to invert this equation so $\mathbf{e}$ is expressed as a function of $\mathbf{v}$, *etc*. To do this we note that the final two terms in the $\{\}$ in (6) yield $\mathbf{e}$ if we first cross multiply them by $\times\,\hat{\mathbf{h}}\cos[(1-n)\phi]$ and add the result to the same two terms $\times\,(-\sin[(1-n)\phi])$. But from (6) those final two terms are equal to

$$\frac{nh}{\mu}\mathbf{v}+\hat{\mathbf{r}}\times\hat{\mathbf{h}}\left[1+\frac{l}{r}(n^{-1}-1)\right];$$

hence $e^2$ is the square of this, which yields $e^2=1+2l\mu^{-1}\varepsilon$, where $\varepsilon=\frac{v^2}{2}-\frac{\mu}{r}-\frac{K}{2r^2}$, and

$$\mathbf{e}=\left\{\frac{nh}{\mu}\mathbf{v}\times\hat{\mathbf{h}}-\hat{\mathbf{r}}\left[1+\frac{l}{r}(n^{-1}-1)\right]\right\}\cos[(1-n)\phi]-$$
$$\left\{\frac{nh}{\mu}\mathbf{v}+\hat{\mathbf{r}}\times\hat{\mathbf{h}}\left[1+\frac{l}{r}(n^{-1}-1)\right]\right\}\sin[(1-n)\phi], \qquad (8)$$

which gives the new vector constant of the motion. We remind the reader that $\phi=\cos^{-1}(\hat{\mathbf{e}}.\hat{\mathbf{r}})$ and $n=\sqrt{1-Kh^{-2}}$. We may eliminate $l/r$ in terms of $\phi$ and $e$ by using (5). Notice that (8) is actually an implicit equation because $\phi$ is not known until $\hat{\mathbf{e}}$ is known. However, by taking $(x,y)$ coordinates in the plane of the motion and measuring a new $\phi'$ from the $x$ axis, we may write $\phi=\phi'-\phi_0$, where $\phi_0$ is the azimuth of $\hat{\mathbf{e}}$. With $\phi'$, $\mathbf{v}$, $\mathbf{h}$, $\mathbf{r}$, $n$, $l$ all known it is then possible to find a $\phi_0$ from the components of $(8)/e$. So the implicit equation may be solved for $\hat{\mathbf{e}}$. There will be very many solutions unless we restrict $\phi_0$ to be in the range $-\pi/n$ to $+\pi/n$ and $\phi'$ in the range $-\pi$ to $+\pi$. I gave a discussion earlier[6] of the type of forces that leave the magnitude of the eccentricity unchanged but slew its direction.

*Approximating orbits in other potentials*

Suppose we are given a potential $\psi(r)$ and an orbit within it is defined by its pericentre at $r_p$ and its apocentre at $r_a$. Then since $\dot{r}$ vanishes at these points the angular momentum of the orbit is given by

$$h^2=2\,\frac{\psi(r_p)-\psi(r_a)}{r_p^{-2}-r_a^{-2}}$$

and the energy of the orbit is given by

$$\varepsilon=\frac{h^2}{2r_p^2}-\psi(r_p)\,.$$

Writing $r=u^{-1}$, the angle between perihelion and aphelion is then

$$\Phi=\int_{r_a^{-1}}^{r_p^{-1}}\left\{2h^{-2}\left[\varepsilon+\psi(u^{-1})\right]-u^2\right\}^{-1/2}du\,.$$

This may be compared with the angle given by the orbit (5), which is $\pi/n$. Hence we may define the $n$ of the approximating orbit by $n=\pi/\Phi$.

We shall make the angular momenta of the two orbits equal and define the eccentricity by

$$e=\frac{r_a-r_p}{r_a+r_p},$$

which is also in conformity with (5).

Evidently the $K$ of our approximating potential is already known *via* $n$ since $K = h^2(1 - n^2)$. To specify the approximating potential we still need $\mu$, which we fix by making the perihelion distances equal, which, *via* the eccentricity, implies the aphelion distances are equal and hence

$$\mu = (h^2 - K)(r_p^{-1} + r_a^{-1})/2 \ .$$

Thus we have an approximation scheme giving $n$, $K$, $h$, $e$, $\mu$ for any orbit specified by $r_p$ and $r_a$ in any known potential $\psi(r)$. We expect the $\mathbf{e}$ defined by (8) to be approximately constant for such orbits.

*The eccentricity vector for the harmonic oscillator*

Newton[7] explained the connection between the Keplerian ellipse and that generated by the two-dimensional harmonic oscillator. Here we follow Chandrasekhar's preferred path[2]. Set $x + iy = z$ in the plane of the orbit. The equation of the harmonic oscillator is then $\frac{d^2z}{dt^2} + \omega^2 z = 0$ and its energy is $\varepsilon = \frac{1}{2}\left(\frac{d\bar{z}}{dt}\,\frac{dz}{dt} + \omega^2 \bar{z} z\right)$. Its angular momentum is $h = \frac{1}{2i}\left(\bar{z}\,\frac{dz}{dt} - \frac{d\bar{z}}{dt} z\right)$. Now consider the mapping of the complex plane $Z = z^2$. Following Newton we ask whether the mapped path considered with a new time $\tau(t)$ can be an orbit under a new central force. Evidently the angular momentum of the $Z$ orbit is

$$\frac{1}{2i}\left(\bar{Z}\frac{dZ}{d\tau} - \frac{d\bar{Z}}{d\tau}\,Z\right) = \frac{1}{i}\,|z|^2\left(\bar{z}\,\frac{dz}{d\tau} - \frac{d\bar{z}}{d\tau}\,z\right) = 2\,\frac{dt}{d\tau}\,|z|^2 h,$$

so if the angular momenta are to be equal then $\frac{d}{d\tau} = \frac{1}{2|z|^2}\,\frac{d}{dt}$. Chandrasekhar (in error!) omits the factor 2. Now using the $z$ equation of motion

$$\frac{d^2Z}{d\tau^2} = \frac{1}{4\,|z^2|}\,\frac{d}{dt}\left(\frac{2}{\bar{z}}\,\frac{dz}{dt}\right) = \frac{-1}{2z\bar{z}^3}\left(\frac{d\bar{z}}{dt}\cdot\frac{dz}{dt} + \omega^2 \bar{z} z\right) = \frac{-\varepsilon}{z\bar{z}^3} = \frac{\varepsilon Z}{|Z|^3},$$

but the final expression shows us $Z$ is a motion under an inverse-square law with a force constant $\mu = GM = \varepsilon$, where $\varepsilon$ is the energy of the simple harmonic orbit. Thus under the mapping the simple harmonic centred ellipse becomes the Kepler eccentric ellipse. However, the latter has a conserved Hamilton eccentricity vector so what does that vector become under the inverse transformation from Kepler's to Hooke's ellipse? In the notation of this section, vectors in the plane of the motion are complex numbers. Since $d^2Z/d\tau^2 = -\mu Z/|Z|^3$ we follow our well-trodden path and multiply by $h$ and integrate to find

$$h\; dZ/d\tau = i\mu(Z/|Z| + e),$$

where $e$ is the (complex) constant of integration. To transform this into the $z$ plane we write $d/d\tau = \frac{1}{2|z|^2}\,\frac{d}{dt}$, $Z = z^2$, and $\mu = \varepsilon$. So

$$-\left(\frac{ih}{\varepsilon}\,\frac{1}{\bar{z}}\,\frac{dz}{dt} + \frac{z}{\bar{z}}\right) = e. \tag{9}$$

I had to differentiate this extraordinary expression and show the result to be zero before I believed that it was indeed a constant of the motion! In the original $Z$ space $e$ pointed toward perihelion. After the transformation $Z = z^2$, $e$ is still the same complex number so is unchanged but now the perihelion in $z$ space will be at half the angle to the real axis. This suggests that we should be considering a new vector $\tilde{e}_p$ pointing to perihelion and with the property that $e = \tilde{e}_p^2$. This $\tilde{e}_p$ will then be a constant of the motion too and furthermore the $\pm$ ambiguity in its definition reflects the fact that the Hooke ellipse has two perihelia in opposite directions. There is an intrinsic difficulty in the transformation that we have to place a cut in the complex $Z$ plane to define $\sqrt{Z}$ properly. It we place this cut arbitrarily then the direction of that cut intrudes into the resultant formulae. It is much more sensible to take the cut to be defined physically. Taking the cut along the real axis and that toward perihelion along $e$ has advantages. Then both $e$ and $\tilde{e}_p$ are real. Rewriting our equation to give us the velocity we have

$$\frac{dz}{dt} = \frac{i\varepsilon}{h}\,(z + e\bar{z});$$

we may now rewrite this in vector form

$$\frac{d\mathbf{r}}{dt} = \frac{\varepsilon}{h}\left\{\hat{\mathbf{h}} \times [\mathbf{r} + \tilde{\boldsymbol{e}}_p.\mathbf{r}\tilde{\boldsymbol{e}}_p + \tilde{\boldsymbol{e}}_p \times (\tilde{\boldsymbol{e}}_p \times \mathbf{r})]\right\},$$

which gives the velocity in terms of the vector $\tilde{\boldsymbol{e}}_p$ that points to perihelion and the position vector $\mathbf{r}$ in the orbital plane orthogonal to $\hat{\mathbf{h}}$. To find the magnitude of $e$ and $\tilde{e}_p$ we return to equation (9). The general solution to the harmonic equation is $z = pe^{i\omega t} + qe^{-i\omega t}$ where $p$ and $q$ are complex numbers. In terms of $p$ and $q$, $h = \omega(p\bar{p} - q\bar{q})$ and $\varepsilon = \omega^2(p\bar{p} + q\bar{q})$. Putting these expressions into equation (9) we find

$$e = \frac{-2pq}{p\bar{p} + q\bar{q}};$$

choosing the real axis along $e$ means that $p = Pe^{i\chi}$ and $q = -Qe^{-i\chi}$ with $P$ and $Q$ real and positive, then $z = (P - Q)\cos(\chi + \omega t) + i(P + Q)\sin(\chi + \omega t)$ and

$$e = \frac{2PQ}{P^2 + Q^2} = \frac{a^2 - b^2}{a^2 + b^2} = \frac{e_\star^2}{2 - e_\star^2},$$

where $a$, $b$ are the semi axes and $e_\star$ is the 'eccentricity' of the centred Hooke ellipse. Thus the magnitude of $\tilde{e}_p$ is given by

$$\tilde{e}_p = \frac{e_\star}{\sqrt{2 - e_\star^2}}.$$

Of course $\tilde{e}_p$ could be generalized to cases in which the linear Hooke law is supplemented by an inverse-cube repulsion, by following the method given in this paper.

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# POLARIZED STANDARD STARS

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From the observing log of the Flower and Cook Observatory, now defunct, there are presented the means of all the filtered polarization measures of 19 stars that were believed to be of constant polarization and constant orientation of the electric vector. Whilst a few of these do not confirm published results from other groups, the data for these objects are not strong enough to conclude that they are polarization variables.

## *Introduction*

In a recent communication, Koch & Clarke[1] summarized and interpreted the measures of nominally zero-polarization stars observed at the Flower and Cook Observatory (FCO) over a 27-year interval. A single reflector was used for all the observations but the optically-active components of the polarimeter as well as the detectors were altered several times. These changes have been described in the aforesaid reference.

## *Non-null standard stars*

A relatively small number of supposedly constant-polarization stars — polarized by interstellar scattering — were selected from the literature of the 1950s through the 1970s and from private communications. These were observed hardly at all at the beginning of the FCO programme in 1969 when all of the close-binary-programme targets were only feebly polarized. As more, and typically more distant, close binaries and other targets were added to the observing programme, it was necessary to calibrate the natural polarization scale more carefully and the 19 objects in Table I were observed as opportunity offered. Some of the stars lack red magnitudes and for some of them no polarization measures have ever been published for particular filters. The observing procedure was the same for those objects as for the null standards. In retrospect, it would have been better to concentrate much more on those targets but inertia was too strong.

The calibration procedure incorporating both null and non-null standards has been described by Koch & Clarke. The values in Table I result from linear regressions of the FCO natural parameters upon the weighted published means, and typically these fits were reasonably well defined for each interval over which the polarimeter remained mounted on the telescope without interruption. After it became clear that a few of the supposed standards were not yielding their historical values, they were culled from the regressions and the calibration process was iterated without them. Those stars were then subjected to the calibration as if they were programme objects. The table gives 202 linear and 38 circular normalized Stokes-parameter means. As the detector 'sees' the star, $v > 0$ for a counter-clockwise rotation of the electric vector. Each parenthesized datum is the $1\sigma$-error of the least significant figure of the preceding entry.

*Discussion*

The errors in the table represent the dispersion of the individual weighted observations and were not calculated from photon statistics. There is an asymmetry, not seen for null standards or for polarized programme targets, in the distribution of these internal errors: those for $q$ are larger than those for $u$ more commonly than the reverse possibility. The effect does not depend on magnitude, filter, algebraic sign of either normalized Stokes parameter, quadrant of the electric vector, or season. No telescope or instrumental problem is known to have existed throughout the length of the observing programme that could have caused the curiosity. The only obvious indicator is that four of the nine objects showing the effect are variable stars, but there are other variables with comparable errors for the two parameters. Perhaps this is just the randomness inherent in a small-number sample.

Over the range $+4 <$ Mag.$< +6$, there exist comparable numbers of null and non-null standard stars. For those with comparable numbers of filtered observations, the internal errors for the non-null and the null standards are very similar to each other. The non-null analogue of Fig. 3 in Koch & Clarke resembles that representation only weakly because there are no very bright non-null standards and also none with dozens of measures. Because the observations reported here are relatively few, did not employ novel filters, and mostly used stars that had been observed by other workers, they contribute nothing new to the determination of interstellar polarization.

For stars observed more than once with a given filter, the normalized Stokes parameters failed to reproduce the values in the literature for the following cases: the known light variable HD 21291 (U$q$, B$q$, V$q$); HD 41117 (B$q$, V$q$, V$u$, R$u$); HD 204827 (R$q$); and HD 207089 (R$q$). These objects have no obvious common characteristics and the FCO discrepancies should be checked against independent sources. HD 145206 shows more filter-dependent scatter than the other stars. Since this is a variable K4 III star, it probably should not have been

TABLE I

*Non-null standard-star means*

| *HD/GCVS* | *Mag.* | *q*(%) | *u*(%) | *N*(lin.) | *v*(%) | *N*(cir.) |
|---|---|---|---|---|---|---|
| 1070 | +8·62B | −1·38 ( ) | +0·75 ( ) | 1 | | |
| 7927 | +6·15U | −3·03 (9) | −0·55 (9) | 5 | | |
| | +5·66B | −3·26 (3) | −0·45 (1) | 5 | | |
| | +4·98V | −3·31 (3) | −0·37 (2) | 5 | +0·00 ( ) | 1 |
| | +4·35R | −3·15 (7) | −0·24 (1) | 4 | | |
| 17378/V480 Per | +6·25V | −2·86 ( ) | −6·07 ( ) | 1 | +0·02 ( ) | 1 |
| | R | −2·60 ( ) | −5·93 ( ) | 1 | +0·05 ( ) | 1 |
| 21291/CS Cam | +4·38U | −1·68 (4) | −2·35 (3) | 6 | | |
| | +4·62B | −1·89 (4) | −2·59 (3) | 8 | | |
| | +4·21V | −1·93 (6) | −2·80 (2) | 5 | +0·00 ( ) | 1 |
| | R | −1·93 (3) | −2·62 (5) | 4 | | |
| 21389/CE Cam | +4·99U | −1·20 ( ) | −2·47 ( ) | 1 | +0·5 ( ) | 1 |
| | +5·10B | −1·55 (6) | −2·90 (2) | 2 | +0·02 (1) | 2 |
| | +4·54V | −1·63 (6) | −3·08 (2) | 2 | +0·003 (6) | 2 |
| | R | −1·56 (1) | −2·91 (2) | 6 | +0·056 (4) | 2 |
| 23512 | +8·47B | +1·00 ( ) | +1·92 ( ) | 1 | | |
| 25291 | +5·56B | −0·18 (1) | −1·93 (4) | 5 | | |
| | +5·06V | −0·22 (4) | −2·01 (2) | 3 | | |
| | R | −0·14 (8) | −1·84 (8) | 2 | | |
| 41117 | +4·23U | +1·62 (11) | −0·25 (6) | 2 | | |
| | +4·91B | +2·56 (12) | −0·24 (2) | 5 | −0·00 (1) | 2 |
| | +4·63V | +2·64 (2) | −0·32 (6) | 3 | +0·00 ( ) | 1 |
| | +4·31R | +2·72 (6) | −0·23 (3) | 3 | −0·00 ( ) | 1 |
| 43384/PX Gem | +6·32U | +2·38 (9) | −0·85 (2) | 4 | | |
| | +6·70B | +2·54 (7) | −0·88 (4) | 4 | +0·00 ( ) | 1 |
| | +6·25V | +2·93 (10) | −0·89 (4) | 6 | +0·13 ( ) | 1 |
| | +5·78R | +2·60 (9) | −0·70 (1) | 4 | −0·05 ( ) | 1 |
| 52721/GU CMa | +6·65B | +1·35 ( ) | +0·93 ( ) | 1 | | |
| 60325 | +5·46U | +0·88 ( ) | +0·61 ( ) | 1 | | |
| | +6·17B | +1·04 ( ) | +0·48 ( ) | 1 | | |
| | +6·21V | +1·11 (5) | +0·53 (4) | 2 | | |
| | R | +1·51 ( ) | +0·32 ( ) | 1 | | |
| 139137 | +7·20B | −0·85 (3) | +0·73 (4) | 3 | | |
| | +6·51V | −0·98 (7) | +0·67 (2) | 6 | | |
| 140873/PT Ser | +4·97U | −0·80 (2) | +0·04 (5) | 3 | | |
| | +5·37B | −0·95 (8) | +0·12 (1) | 3 | | |
| | +5·40V | −0·95 (2) | +0·103 (8) | 15 | +0·024 (9) | 6 |
| | R | −1·02 (18) | +0·03 (9) | 3 | | |
| 145206 | +6·85B | −0·84 ( ) | −1·05 ( ) | 1 | | |
| | +5·40V | −0·99 ( ) | −0·46 ( ) | 1 | | |
| | R | −0·65 ( ) | −0·99 ( ) | 1 | | |
| 154445 | +5·80B | −3·45 (3) | −0·060 (7) | 3 | −0·00 ( ) | 1 |
| | +5·64V | −3·75 (6) | −0·13 (2) | 4 | | |
| | R | −3·32 ( ) | +0·05 ( ) | 1 | | |
| 183143/HT Sge | +6·86V | +6·105(5) | −0·12 (4) | 2 | | |
| 198478/V1661 Cyg | +5·70U | +2·45 (3) | +0·25 (4) | 4 | | |
| | +5·25B | +2·66 (6) | +0·25 (4) | 6 | −0·0004 (2) | 2 |
| | +4·84V | +2·81 (4) | +0·28 (2) | 10 | | |
| | +4·39R | +2·56 (4) | +0·38 (2) | 5 | −0·0011 (2) | 2 |
| 204827 | +8·69B | −2·81 (5) | +4·98 (6) | 4 | +0·21 (1) | 2 |
| | +8·00V | −2·66 (4) | +4·75 (4) | 5 | −0·06 (2) | 4 |
| | R | −2·53 (2) | +4·37 (5) | 3 | −0·3 (3) | 2 |
| 207089 | +6·70B | +0·08 (2) | +0·58 (5) | 4 | +0·00 ( ) | 1 |
| | +5·29V | +0·08 (2) | +0·55 (1) | 8 | | |
| | R | +0·09 (7) | +0·49 (1) | 3 | | |

observed as a standard at all. Except for HD 145206, all the others display almost constant orientation of the electric vector with wavelength. In a way, it is remarkable that discrepancies against other sources are so few. Of the 19 objects in Table I, nine of them are now known to be low-amplitude light variables of assorted types and two of them are spectroscopic binaries. In addition, 15 of the targets are giants, bright giants, or supergiants that might be expected eventually to be recognized as polarization variables. On the evidence of the limited data in Table I, most of the stars (possibly excepting HD 21291 and 145206) should continue to be understood as stable linear-polarization standards at the level of accuracy of this survey.

The results in Table I were mostly accumulated from 1981 through 1996 during which interval the polarimeter had been dismounted from the reflector only three times. These data should, therefore, be less subject to the optical/mechanical perturbations described by Koch & Clarke for the null standards. Unfortunately, there is no direct way to compare the two data sets since the criterion ($p/\sigma$) used by Simmons & Stewart[2] is almost numerically disjoint for the two sets. This happens because the ratio of the signals between the non-null and null standards is about 500:1 whilst the ratio of their respective errors is of the order of 17:1. Because the ($p/\sigma$)-criterion is so large for the polarized stars, bias-corrected values of $p_0$ calculated with the fitting coefficients given by Stewart[3] do not differ within the errors from the values that may be calculated directly from the normalized Stokes parameters in Table I.

Because the number of circular measures is few and a complete polarization spectrum does not exist for even one target, these observations command little confidence. There might be real signals for HD 21389, 198478, and 204827 but the expected sign reversal with wavelength was not observed for the first two of those stars.

## *Conclusions*

Most standard polarization stars have appeared in lists with the evidence for testing their constancy only implied. It is uncommon to read of further, independent tests of them after their original appearances. This note describes the results of what may be considered inadvertent tests of the polarization parameters for these stars, adds weight to the published linear polarization values for most of them, and alerts the future observer to a few stars that might be avoided in the standardizing process.

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# SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

## PAPER 188: HD 14544, HDE 237201, 66 ORIONIS, HD 216218, AND HD 220102

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The five stars described here share a troublesome characteristic inasmuch as their periods are all very close to a small exact number of years — 2, 1, 3, 2, and 1 year(s), respectively. In all except the last case it has therefore been (and will long remain) impossible to obtain satisfactory phase coverage of their radial-velocity orbits, at least with the instrumentation available to the author. All the same, their orbits are tolerably well established; all have modest eccentricities. The coincidence of the period of HD 220102 with the exact year is not as close as in the other cases; with the help of many observations obtained in the 1980s, and others obtained recently at large hour angles, continuous phase coverage has been achieved.

*Introduction*

The stars treated in this paper were brought to attention in 1999 by de Medeiros & Mayor[1] in their large list of radial and rotational velocities of late-type stars. Naturally, the radial velocities of a goodly number of the ~2000 stars listed had been found to show discordances that suggested that the objects concerned were members of binary systems. In most cases, however, only a few (commonly two) measurements had been made, and they were not given individually although they were promised in a listing to be made available from the *Centre de Données Stellaires*. In the paper itself each star is accorded just one line, which includes the mean velocity, its r.m.s. uncertainty ($\varepsilon$), the r.m.s. dispersion ($\sigma$) of individual measurements about the mean, and an assessment of the statistical likelihood of the velocity being in fact constant. Whereas $\sigma$ is simply determined mathematically from the individual radial-velocity values, $\varepsilon$ is the larger of ($\sigma/\sqrt{n}$) and ($1/\sqrt{n}$ times the 'typical error' of an individual measurement). The likelihood of constancy evidently cannot be determined from the $\varepsilon$ and $\sigma$ quantities that are accessible to the reader, but seems to be based upon the number in a column (headed $E/T$) that is not even mentioned in the description of the table but looks as if it may give a ratio between the value of $\sigma$ actually obtained and the one that might be expected on the basis of internally computed estimates of the accuracies of the observations.

In view of the smallness of the number of measurements for most stars, it might seem that the $\varepsilon$ values estimated by dividing $\sigma$ by $\sqrt{n}$ would often be appreciably less than would be obtained under the normal scheme of dividing $\sigma$ by $\sqrt{(n-1)}$. The exact words used to describe $\sigma$ in the paper[1] are "The radial velocity dispersion (rms)"; but when it became possible to compare the listing of the individual velocities with the $\sigma$ number, it seemed that the latter had been computed by summing the squares of the individual velocities' deviations from the mean and dividing that sum by $(n-1)$ before taking the square root, so the loss of one degree of freedom implicit in the taking of the mean was accounted for in the derivation of $\sigma$; the quantity tabulated is actually the apparent r.m.s. *error*, not residual, so the derivation from it of the r.m.s. error of the mean by division by $\sqrt{n}$ is correct.

When the paper[1] appeared, the present writer scanned it for objects that might usefully be added to the Cambridge spectroscopic-binary programme. It transpired that most of the potentially exciting objects that had large values of $\varepsilon$ were well-known spectroscopic binaries whose orbits had already been determined (in some cases by the writer himself), or were already on the programme, or else were in the southern hemisphere and out of reach, or were Cepheids, or F-type stars with high rotational velocities and large measuring errors. There remained a few which commended themselves for observation, more particularly ones which not only were reported as showing considerable radial-velocity changes but also had rotational velocities larger than those normal for late-type stars, possibly indicating that they were binaries of short period. They were HD 39743, 41380, and 46101 (HR 2054, 66 Ori, and 7 Lyn, respectively), observation of which began in Cambridge in late 2000 when the three objects reappeared in the new observing season after their summer holiday in the daytime sky. The indications of short period turned out to be correct in the cases of HR 2054 and 7 Lyn: their rotations did indeed prove to be related to their orbits, whose periods are 83 and 126 days and were presented in Paper 166[2] of this series in 2002. There is no obvious cause for the rapid rotation (the projected rotational velocity given by de Medeiros & Mayor is $14{\cdot}2 \pm 1{\cdot}0$ km $s^{-1}$) of 66 Ori, whose relatively long period of 3 years has led to its publication being delayed until now.

It was not until late 2001 that the promised listing of individual velocities which underlay the de Medeiros & Mayor paper[1] was actually available. In the light of that more detailed information a fresh consideration was made to identify objects of interest, and several were adopted for observation from Cambridge. They include the other objects discussed in this paper, HD 14544, 216218, and 220102, and HDE 237201 (which in terms of its right ascension of about 4 hours comes soon after HD 14544, thus explaining the order in which the stars are treated here). Observations of them began in each case at the start of the 2002/3 observing season. The Cambridge observing campaign has been uncharacteristically short, but in the present case there is not as much as usual to be gained by continuing the observations over additional cycles, because the availability of (even only two) reliable measurements from 15–20 years ago in the source paper[1] improves the precision of the period by as much as would be achieved by several further years of continued observations. The inevitable gaps in phase coverage of the orbits presented in this paper makes the figures appear worse than those to which attentive readers of this series will have become accustomed, but probably do not greatly impair the reliability of the orbital elements; it may, however, be prudent to regard the formal standard errors as being somewhat on the optimistic side.

## *HD 14544*

HD 14544 is an $8^{1}/_{2}{}^{m}$ star just to the south of $\chi$ Persei, the following (eastern) condensation of the well-known 'Double Cluster in Perseus', also called h & $\chi$

Persei and NGC 869 & 884; it is about 45′ south of the centre of $\chi$ Per. Its magnitude seems not to have been measured from the ground, but *Tycho 2*[3] has listed magnitudes that transform[4] to $V = 8^{m} \cdot 66$, $(B-V) = 1^{m} \cdot 18$. The star was observed for radial velocity at Haute Provence (OHP) a long time ago[5] by the objective-prism method[6]; a mean of $-5$ km s$^{-1}$ was given from two measurements, with quality 'B' assigned to it ('probable error' between 2·5 and 5 km s$^{-1}$). At the same time it was classified as being a bright giant of type K2 II.

Owing to its proximity to h & $\chi$ Per, HD 14544 featured in the results of a flight of the 'SCAP–2000 experiment' in which Golay *et al.*[7] obtained ultraviolet photometry by taking photographs at a wavelength of about 2000 Å from a balloon-borne telescope at an altitude of no less than 40 km, above the atmospheric ozone layer that prevents such wavelengths reaching the ground. The relevant flight took place on 1982 September 17/18 and obtained plates covering 6° fields at the focus of a 130-mm Schmidt-Cassegrain telescope having a fused-quartz corrector plate. Two exposures each of 25 seconds were taken of the area concerned, in which Golay *et al.* listed HD 14544 as their no. 430. The λ2000-Å magnitude, called $U_1$, was determined as 10·74 ± 0·11; a visual magnitude of 8·90 appears to have been adopted from the entry in the *Henry Draper Catalogue*, giving a $(U_1 - V)$ colour index of $1^{m} \cdot 84$. A note draws attention to the fact that $(U_1 - V)_0$ for even K2 III stars reaches $4^{m}$, and the magnitude and type of star 430 mean that it must be more than 1000 pc away and therefore reddened, the implication evidently being that the observed colour index ought to be redder still. The note goes on to say "The $U_1$ index suggests the existence of a companion which could be of type B5."

It may be noticed that the *Tycho* $(B-V)$ is bluer than would usually correspond to a K2 II type, although it is acceptable for K2 III. The significance of any discrepancy would be, however, heavily reliant upon the OHP classification, for which there is no independent support (the *HD* type is K0), whereas the discrepancy in the ultraviolet colour index between the observed two magnitudes and the expected four (even for a normal giant) may be regarded as pretty compelling evidence of a hot companion.

As far as the writer is aware, the only other published information of interest for present purposes about HD 14544 is in the paper by de Medeiros & Mayor[1]. The star is fainter than most of those in that paper, which was mostly limited to $8^{m}$, but extra stars were added in an effort to obtain good representation of types that were scarce among the bright stars. HD 14544 was listed as having two radial-velocity measurements, with a mean of $-3 \cdot 35$, an $\varepsilon$ of 2·40, and a $\sigma$ of 3·39 km s$^{-1}$, from which (when the principles of the calculation of $\sigma$ are appreciated) it could be deduced that the two measurements differed by $2\varepsilon$ or $\sqrt{2}\sigma$, *i.e.*, 4·80 km s$^{-1}$, and therefore must be $-5 \cdot 75$ and $-0 \cdot 95$ km s$^{-1}$. The values actually listed in the table that is now available through the *CDS* gives them as $-5 \cdot 01$ and $-0 \cdot 21$ — the right difference, but both of them being 0·74 km s$^{-1}$ more positive than expected. Analogous (but different) discrepancies are found for all the other stars when the published means are compared with the separately listed velocities. The probable explanation is that the listing of the individual velocities was made after the adoption of a new scheme of reduction[8] of the OHP *Coravel* observations, but that the actually-published *table* was compiled on the previous basis that had been found[8] faulty. That need not concern us unduly here, however, since for our purposes the availability of the individual velocities, with their dates, makes the originally published table otiose. Moreover, three extra observations had somehow been located after publication of the table, so the delay in the provision of the complete listing was not without advantage. The total of five OHP observations, therefore, appear at the head of the listing of radial velocities in Table I. They have been adjusted by $+0{\cdot}8$ km s$^{-1}$ in an effort to equalize their zero-point with that of the Cambridge observations, and in the orbital solution they are weighted $1/2$ to make their variance more comparable with Cambridge's. The rest of the Table presents the 24 measurements taken since late 2002 with the Cambridge *Coravel*.

The orbit follows readily from the set of velocities in Table I; it is shown in Fig. 1 and its elements are:

| | |
|---|---|
| $P = 728{\cdot}8 \pm 0{\cdot}5$ days | $(T)_1 = \text{MJD } 52682 \pm 10$ |
| $\gamma = -2{\cdot}17 \pm 0{\cdot}04$ km s$^{-1}$ | $a_1 \sin i = 47{\cdot}7 \pm 0{\cdot}5$ Gm |
| $K = 4{\cdot}79 \pm 0{\cdot}04$ km s$^{-1}$ | $f(m) = 0{\cdot}00815 \pm 0{\cdot}00023\ M_\odot$ |
| $e = 0{\cdot}120 \pm 0{\cdot}010$ | |
| $\omega = 191 \pm 5$ degrees | R.m.s. residual (unit weight) $= 0{\cdot}13$ km s$^{-1}$ |

The r.m.s. residual is noteworthy as being, by quite a margin, the smallest yet seen in this series of papers, although of course it still does not bear comparison with some of those obtained by instruments designed for planet-hunting capability.

The orbital period differs from the exact value of two years by just $1{\cdot}7 \pm 0{\cdot}5$ days, so there is no hope of filling in the ~4-month phase gaps at all soon — that would take from 55 to 100 two-year cycles to accomplish! It must be mentioned, however, that the gaps arise only as a result of the particular instrumental set-up at Cambridge involving a northward-going coudé focus, which causes the area to the north of the telescope to be obstructed. Indeed, at its $+56°$ declination,

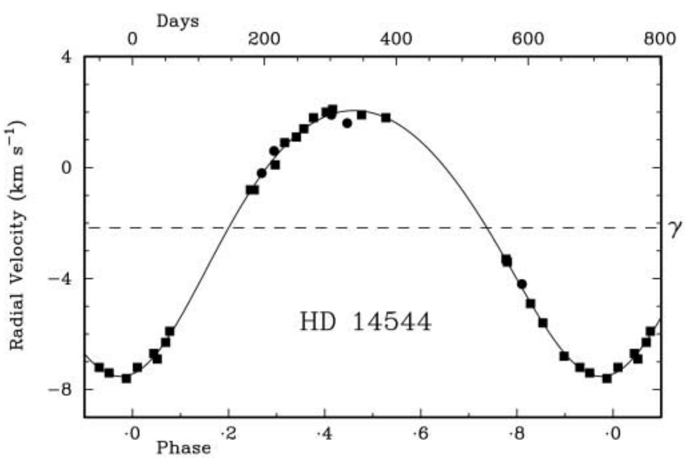


FIG. 1

The observed radial velocities of HD 14544 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Circles represent measurements made with the Haute-Provence *Coravel* and published by de Medeiros & Mayor[1]; squares refer to the author's observations with the Cambridge *Coravel*.

TABLE I

*Radial-velocity observations of HD 14544*

*The sources of the observations are as follows:*
*1986–1998 — OHP Coravel (weight 1/2); 2002–2005 — Cambridge Coravel*

| *Date (UT)* | *MJD* | *Velocity* $km\ s^{-1}$ | *Phase* | *(O−C)* $km\ s^{-1}$ |
|---|---|---|---|---|
| 1986 Oct. 10·02 | 46713·02 | −4·2 | $\bar{8}$·810 | +0·1 |
| 1987 Sept. 28·01 | 47066·01 | +0·6 | $\bar{7}$·295 | +0·3 |
| 1997 Aug. 31·02 | 50691·02 | −0·2 | $\bar{2}$·269 | +0·1 |
| Dec. 14·85 | 796·85 | +1·9 | ·414 | 0·0 |
| 1998 Jan. 7·79 | 50820·79 | +1·6 | $\bar{2}$·447 | −0·4 |
| 2002 Sept. 2·08 | 52519·08 | −3·3 | 0·777 | 0·0 |
| Oct. 28·07 | 575·07 | −5·6 | ·854 | 0·0 |
| 2003 Jan. 7·02 | 52646·02 | −7·4 | 0·951 | 0·0 |
| Feb. 18·90 | 688·90 | −7·2 | 1·010 | +0·1 |
| Mar. 15·84 | 713·84 | −6·7 | ·044 | +0·1 |
| Apr. 2·84 | 731·84 | −6·3 | ·069 | 0·0 |
| Aug. 15·13 | 866·13 | −0·8 | ·253 | −0·2 |
| Sept. 16·05 | 898·05 | +0·1 | ·297 | −0·3 |
| Oct. 18·04 | 930·04 | +1·1 | ·341 | −0·1 |
| Nov. 13·00 | 956·00 | +1·8 | ·377 | +0·2 |
| Dec. 11·99 | 984·99 | +2·1 | ·416 | +0·2 |
| 2004 Jan. 24·90 | 53028·90 | +1·9 | 1·477 | −0·1 |
| Mar. 1·81 | 065·81 | +1·8 | ·527 | 0·0 |
| Sept. 2·10 | 250·10 | −3·4 | ·780 | 0·0 |
| Oct. 7·11 | 285·11 | −4·9 | ·828 | 0·0 |
| Nov. 26·93 | 335·93 | −6·8 | ·898 | −0·1 |
| Dec. 20·92 | 359·92 | −7·2 | ·931 | 0·0 |
| 2005 Jan. 30·97 | 53400·97 | −7·6 | 1·987 | −0·1 |
| Mar. 18·80 | 447·80 | −6·9 | 2·051 | −0·2 |
| Apr. 6·82 | 466·82 | −5·9 | ·078 | +0·1 |
| Aug. 7·07 | 589·07 | −0·8 | ·245 | 0·0 |
| Sept. 28·14 | 641·14 | +0·9 | ·317 | +0·1 |
| Oct. 27·00 | 670·00 | +1·4 | ·356 | 0·0 |
| Nov. 29·94 | 703·94 | +2·0 | ·403 | +0·1 |

HD 14544 is actually circumpolar at Cambridge and so in principle would be observable on every night in the year by a system such as that at OHP, where the telescope is fork-mounted and the *Coravel* is at the Cassegrain focus. The hour-angle travel of the Cambridge telescope, even at high declinations, is restricted to about ±6 hours from the meridian, and the ~$2^h$ right ascension of HD 14544 causes it to be below the Pole in the summer months when the nights are short and the star is inaccessible throughout.

The area of the dip that the star produces in radial-velocity traces is no greater than that normally seen from stars with types near K0 III; objects that are later in type or more luminous could be expected to give bigger dips. We cannot pretend to classify stars just from the size of their radial-velocity dips, but we *can* say that either the late-type spectrum of HD 14544 must be diluted by the blue continuum of a hot companion or else the star has probably been misclassified. It would not be surprising if the '$v \sin i$' value, derived in the rather simplistic fashion current at Cambridge where the extra 'macroturbulence' of high-luminosity stars is ignored, would be at least a few km s$^{-1}$ for a star of the K2 II type that HD 14544 is supposed to have, but in fact the mean value is only 1·5 km s$^{-1}$ with a formal uncertainty much less 1 km s$^{-1}$. We notice also that the r.m.s. residual of the radial velocities does not leave much scope for the 'jitter' often seen in the velocities of high-luminosity stars. In fact every piece of evidence, none of which is decisive in isolation, tends to support a conclusion that the luminosity of HD 14544 is no higher than that of a normal giant.

*HDE 237201*

The identification HDE 237201 comes from the *Henry Draper Extension* catalogue, which was published in a number of *tranches* after the main catalogue had been completed in 1924. The section relevant to HDE 237201 was published[9] by Miss Cannon in 1928, and referred to a Milky Way area in Camelopardus. The star of interest to us here is at declination 57°, very nearly the same as that of HD 14544, but is about an hour and a half following that star — not very far at that declination. It is about 8° north-following α Per. It is in an area of sky that seems to have been curiously overlooked by Bayer[10] and Flamsteed[11] when they were assigning constellation designations to the bright stars. Only three stars in the whole constellation have been dignified by Greek-letter designations, and there is an enormous area in the preceding part of the constellation without any Flamsteed numbers at all. The western boundary of Camelopardus is at about 3¼ hours' R.A., but the first Flamsteed star, 1 Cam ($5^m \cdot 77$), is not encountered until past 4½ hours. By that point, as may be discovered from the *Bright Star Catalogue*, no fewer than 19 stars brighter than 1 Cam (one of them brighter even than α Cam!) had been passed over; yet, once the numbering starts, quite faint stars are numbered. The star 20 Cam, for example, does not feature in the *Bright Star Catalogue*, and for good reason, since it is only $7^m \cdot 47$; 21 Cam is $6^m \cdot 86$; and 22 Cam, whose orbit was given[12] in Paper 182 of this series, is only $7^m \cdot 04$. The last Flamsteed star in Camelopardus is no. 53, which is just past $8^h$, although the constellation extends to beyond $14^h$ at very high declinations; and there are no numbered stars above declination 69° although the constellation goes to 86°. The '32 Cam' identified in *Uranometria 2000*[13] (HR 4892/3) is a mistake, one that has frequently been perpetrated and whose nature was in fact explained long ago in a note in the back of the *Bright Star Catalogue*[14].

Owing to its position in the Milky Way, HDE 237201 has featured in three substantial investigations of galactic star fields. The first was by McCuskey[15], who numbered the star 144 in his region LF6. He obtained direct plates with the 24/36-inch *Burrell Schmidt*[16] in the red (Kodak 103a−E emulsion with a Wratten 22 filter) and blue (IIa−O), from which he derived $m_{pg} = 10^m \cdot 51$ and a colour index of $2^m \cdot 34$; he also took objective-prism plates with a 4° prism, and classified the star as K3 IIp. There is a note, "Peculiarity in UV due to close companion." It may refer to the pair of faint stars that are a little over 1′ following and are about three magnitudes fainter in the blue than the star of interest, but one cannot be sure without knowing the orientation of the dispersing prism; the spectra must be a few millimetres in length, equivalent to a few minutes of arc on the sky, and so are at risk of being overlapped by the spectra of other objects within such a distance in the direction of dispersion.

Rydstrom[17] took plates with a 7° objective prism on the 100/135-cm Schmidt of the Kvistaberg Observatory. Classifications were not made directly by looking at the plates, but were derived from numerical quantities measured from tracings of the spectra. A type of gK4 was found for HDE 237201.

Very recently, the Zdanavičiuses[18] have published the results of an investigation of the same area of sky by means of stellar photometry obtained with a CCD in the 7-colour Vilnius system[19]. They interpreted the colours of HDE 237201 as implying that it has an 'mk type' of k5·5 III and an absolute magnitude of $-0 \cdot 68$, is seen through an interstellar cloud with a visual absorption $A_V = 1^{\mathrm{m}} \cdot 07$, and is at a distance of 470: pc. In a discussion in a separate paper[20] they specify that the normal precision of $A_V$ is $0^{\mathrm{m}} \cdot 1$ and that of the distance is 20–25%, but it would appear from the colon that is attached to the distance value for HDE 237201 that in the case of interest the uncertainty is greater.

There does not seem to be any ground-based photoelectric photometry of HDE 237201, but like all stars of such brightness it was measured by *Tycho*. The values given by *Simbad*, *Vizier*, and by transformation[4] of the $V_T$ and $B_T$ values given by *Tycho 2*[3] are all slightly different from each other, but are close to $V = 8^{\mathrm{m}} \cdot 82$, $(B-V) = 1^{\mathrm{m}} \cdot 75$; allowance for the reddening implied by the interstellar absorption noted above indicates that $(B-V)_0 \sim 1^{\mathrm{m}} \cdot 4$.

De Medeiros & Mayor's paper[1] noted that there were two OHP radial velocities of HDE 237201; applying the same logic as for HD 14544, we can deduce that they differed by about 10 km s$^{-1}$. Again, however, there is a bonus in the complete listing, which includes no fewer than six radial velocities altogether, amply confirming the reality of the variability and doubling the observed range. In fact it was not necessary to make any further observations at all to divine that the period is extremely close to one year: on the assumption of zero eccentricity the six OHP velocities gave an orbital solution, which they fitted with an r.m.s. residual of less than 0·3 km s$^{-1}$, with a period of 364 ± 0·3 days.

As related in the *Introduction* above, the star was then promptly taken onto the Cambridge observing programme; 31 measurements have been made, and they are listed in Table II after the six from the de Medeiros & Mayor list, which have been increased as usual by +0·8 km s$^{-1}$. By themselves the Cambridge velocities yield a period of 364·25 ± 0·37 days; the inclusion of the OHP velocities, which merit half-weight, refines the value to 364·09 ± 0·09 days. For exactly the same reason as for HD 14544, it is impossible to reach HDE 237201 with the Cambridge telescope for a considerable interval in the late spring and summer, so there is a large gap in phase coverage; it has been reduced to the absolute minimum that is possible from Cambridge. The gap could be filled from OHP, but none of the six OHP observations is in it, and indeed there would be no incentive for an observer to bother to observe below the Pole (which does involve some inconvenience, even at OHP) if he were not aware of any specific need. The orbit is plotted in Fig. 2 and has the following elements:

| | |
|---|---|
| $P = 364 \cdot 09 \pm 0 \cdot 09$ days | $(T)_1 = $ MJD $52598 \cdot 7 \pm 2 \cdot 3$ |
| $\gamma = -14 \cdot 71 \pm 0 \cdot 06$ km s$^{-1}$ | $a_1 \sin i = 51 \cdot 2 \pm 0 \cdot 4$ Gm |
| $K = 10 \cdot 36 \pm 0 \cdot 08$ km s$^{-1}$ | $f(m) = 0 \cdot 0405 \pm 0 \cdot 0009\ M_{\odot}$ |
| $e = 0 \cdot 159 \pm 0 \cdot 007$ | |
| $\omega = 111 \cdot 1 \pm 2 \cdot 5$ degrees | R.m.s. residual (unit weight) = 0·26 km s$^{-1}$ |

It is seen that the orbital period is frustratingly close to one year. The difference from the exact year is 1·16 ± 0·09 days, so the gap in Cambridge phase coverage

TABLE II

*Radial-velocity observations of HDE 237201*

*The sources of the observations are as follows:*
*1986–1999 — OHP Coravel (weight 1/2); 2002–2005 — Cambridge Coravel*

| *Date (UT)* | *MJD* | *Velocity* *km s*$^{-1}$ | *Phase* | *(O – C)* *km s*$^{-1}$ |
|---|---|---|---|---|
| 1986 Nov. 23·87 | 46757·87 | −15·3 | $\overline{16}$·958 | 0·0 |
| 1987 Sept. 28·13 | 47066·13 | −5·2 | $\overline{15}$·804 | +0·5 |
| 1997 Aug. 30·11 | 50690·11 | −4·5 | $\overline{5}$·758 | +0·5 |
| 1998 Jan. 7·83 | 50820·83 | −25·3 | $\overline{4}$·117 | +0·1 |
| Oct. 15·12 | 51101·12 | −10·1 | ·887 | −0·5 |
| 1999 Jan. 19·83 | 51197·83 | −26·0 | $\overline{3}$·152 | −0·3 |
| 2002 Sept. 28·11 | 52545·11 | −7·7 | 0·853 | −0·1 |
| Oct. 28·10 | 575·10 | −13·4 | ·935 | −0·1 |
| Dec. 11·11 | 619·11 | −23·0 | 1·056 | +0·1 |
| 2003 Jan. 5·03 | 52644·03 | −25·7 | 1·124 | −0·2 |
| 15·94 | 654·94 | −25·6 | ·154 | 0·0 |
| 23·89 | 662·89 | −24·8 | ·176 | +0·6 |
| Feb. 13·89 | 683·89 | −24·1 | ·234 | −0·2 |
| Mar. 15·90 | 713·90 | −20·2 | ·316 | +0·3 |
| 30·86 | 728·86 | −18·5 | ·357 | 0·0 |
| Apr. 16·84 | 745·84 | −15·9 | ·404 | +0·2 |
| Aug. 20·12 | 871·12 | −5·1 | ·748 | −0·2 |
| Sept. 14·17 | 896·17 | −5·5 | ·817 | +0·6 |
| 29·14 | 911·14 | −7·7 | ·858 | +0·1 |
| Oct. 12·11 | 924·11 | −10·2 | ·894 | −0·2 |
| Nov. 4·10 | 947·10 | −15·2 | ·957 | 0·0 |
| 13·06 | 956·06 | −17·1 | ·981 | +0·3 |
| 27·01 | 970·01 | −20·6 | 2·020 | 0·0 |
| Dec. 5·96 | 978·96 | −22·3 | ·044 | +0·1 |
| 21·98 | 994·98 | −24·6 | ·088 | 0·0 |
| 2004 Feb. 22·90 | 53057·90 | −23·0 | 2·261 | −0·1 |
| Sept. 5·13 | 253·13 | −5·7 | ·797 | −0·1 |
| Oct. 22·12 | 300·12 | −12·8 | ·926 | −0·2 |
| Nov. 20·11 | 329·11 | −19·6 | 3·006 | −0·1 |
| Dec. 26·90 | 365·90 | −25·7 | ·107 | −0·5 |
| 2005 Jan. 30·98 | 53400·98 | −25·0 | 3·203 | −0·1 |
| Apr. 6·85 | 466·85 | −16·9 | ·384 | +0·2 |
| 21·85 | 481·85 | −15·6 | ·425 | −0·5 |
| July 20·10 | 571·10 | −5·7 | ·671 | 0·0 |
| Aug. 7·08 | 589·08 | −5·2 | ·720 | −0·2 |
| Sept. 17·12 | 630·12 | −6·9 | ·833 | −0·2 |
| Nov. 14·10 | 688·10 | −18·1 | ·992 | +0·2 |

would migrate round the orbit in a cycle whose length has one-$\sigma$ limits of about 290 to 340 years — too long to wait!

The spectral type of k5·5 III derived from Vilnius photometry[18], or the gK4 from Rydberg's hybrid method[17], does not confirm the bright-giant luminosity of the K3 II directly classified from an actual spectrum[15], but no adjudication

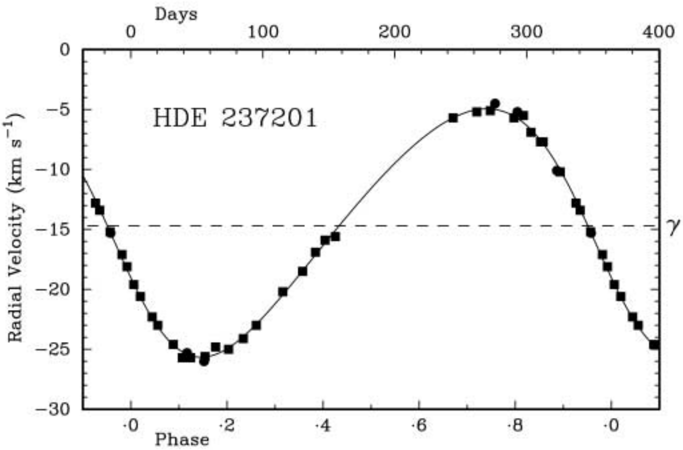


FIG. 2

As Fig. 1, but for HDE 237201.

on the modest differences is possible here. The dip seen in radial-velocity traces is of generous depth and would be consonant with any of the proposed types. The star is quite faint (by *HD* standards) in the blue where the *Coravel*s operate ($B \sim 10^{m}\cdot 6$), and the r.m.s. velocity residual of 0·26 km s$^{-1}$ is not large enough to conceal any substantial real instability.

*66 Orionis*

66 Ori (HR 2145, HD 41380) is much brighter and more easily found than the other stars described in this paper, being a 5½-magnitude object about 4° south-following Betelgeuse. $V$ magnitudes and $(B-V)$ colours very close to $5^{m}\cdot 62$ and $1^{m}\cdot 05$, respectively, have been given by Cousins[21], Argue[22], Appenzeller[23], and Johnson *et al.*[24]. The same authors have provided less-concordant values for $(U-B)$, ranging from $0^{m}\cdot 74$[24] to $0^{m}\cdot 83$[23].

The spectrum was first classified in 1890 in the *Draper Catalogue*[25], as type H; it was revised to K in the *Revised Harvard Photometry*[26] of 1908, and to K0 in the *Henry Draper Catalogue*. Before the latter catalogue was published, however, a considerably different type of G3 had been found[27] from slit spectrograms obtained with the 60-inch reflector at Mount Wilson. They were subsequently utilized[28] for another effort at classification and for the determination of the spectroscopic parallax. The type "estimated" (by visual comparison with spectrograms of standard stars) was G4, whereas the "measured" type (from measures of the Balmer lines on tracings of the spectra — explained in more detail in Paper 186[29] and of course still better in the original paper[28]) was G1, suggesting that the Balmer lines are 'too strong' in relation to the metallic lines; the absolute magnitude was given[28] as $-1^{m}\cdot 1$. A later Mount Wilson paper[30] gave the type again as G4 and revised the absolute magnitude to $0^{m}\cdot 0$. It is probably on the basis of that type and luminosity that the *Bright Star Catalogue* gives what must be only a *quasi*-MK type of G4 III. There is, however, an actual MK classification by Appenzeller[23] of G7 II (of which *Simbad* is unaware, because in place of the paper concerned it lists one, on a completely different topic, that was published on an adjacent page of the same journal); the classification was made from slit spectrograms obtained with the Yerkes 40-inch refractor. It ought to be regarded much more favourably than the *Bright Star Catalogue*'s G4 III, because the *Hipparcos* parallax is only $0''\cdot 00131 \pm 0''\cdot 00076$, leading formally to an absolute distance modulus as great as $9^{m}\cdot 4$ and thus to an absolute magnitude of $-3\cdot 8$, although with great uncertainty. Even a $2\sigma$ increase in the adopted parallax would still leave 66 Ori with an absolute magnitude brighter than $-2$, so the high luminosity implicit in Appenzeller's classification (and partly recognized in the initial Mount Wilson one) is certainly vindicated. The colour indices are far too red for G4 III (they are[31] approximately those normally found at types K0 III and G5 Ib). Of course they may have been exaggerated by interstellar reddening, in which case the corresponding absorption would mean that the luminosity is even higher than is derived directly from the parallax and apparent magnitude; an effort to identify from the literature any star that shows substantial reddening near the line of sight to 66 Ori has, however, not been successful.

There is a considerable bibliography recorded by *Simbad* for 66 Ori, but it is mostly astrometric, arising from the inclusion of the star in the series of *Fundamental Catalogues* (most recently the FK6[32]) begun in the 19th Century by Auwers[33], and is not of direct significance to us here, unless indeed the underlying data could be prevailed upon to disclose the photocentric motion, which appears unlikely since the star is so far away. It seems surprising that a star listed in the *Bright Star Catalogue* as being on the edge of the Hertzsprung Gap has not been studied for its astrophysical interest.

The three Mount Wilson spectrograms[27] mentioned above were measured for radial velocity, but only the mean value, $+32\cdot 7$ km s$^{-1}$, was published in the original paper[27], which dates from 1915. The individual dates and velocities were published more than 50 years later by Abt[34]; the plates are shown there to have been taken in consecutive months in the autumn of 1914, and to have given results that were accordant to well within the uncertainties to be expected of them. The Mount Wilson result (with an empirical correction of $+0\cdot 5$ km s$^{-1}$) was listed in the 1953 *Radial Velocity Catalogue*[35], and remained the only radial-velocity measurement of 66 Ori up to that time. Although neither the original authors[27] nor the *Radial Velocity Catalogue* made any suggestion that the velocity might be variable, the seemingly clairvoyant compiler of the *Bright Star Catalogue*[36] attached the suffix "V?" to the velocity recorded there, which was still the same velocity of $+33$ km s$^{-1}$ found[27] in 1915. (The means by which the star secured entry into a list[37] of 'close binaries observed polarimetrically' seemed equally enigmatic, but was eventually traced to a misidentification by Luna[38] of 66 Ori with HD 32964, which is actually the early-type SB2 system 66 *Eridani*!) Retrospective justification arrived, however, in 1999, when de Medeiros & Mayor[1] reported major discordances between three *Coravel* radial velocities. As noted in the *Introduction* above, it was the combination of the discordances and the high rotational velocity noted for 66 Ori that prompted its immediate inclusion in the Cambridge radial-velocity programme. The same information on 66 Ori as appeared in the paper de Medeiros & Mayor[1] was repeated in one by de Medeiros, da Silva & Maia[39] in 2002; the mean velocity, however, was not included in the later paper[39].

During the past five years 37 measurements have been made at Cambridge of the radial velocity of 66 Ori. They are set out in Table III, after the Mount

## TABLE III

*Radial-velocity observations of 66 Orionis*

*The sources of the observations are as follows:*
*1914 — Published Mount Wilson observation[27,34], not used in orbital solution;*
*1986–1989 — OHP Coravel; 2000–2004 — Cambridge Coravel*

| *Date (UT)* | *MJD* | *Velocity* km s$^{-1}$ | *Phase* | *(O − C)* km s$^{-1}$ |
|---|---|---|---|---|
| 1914 Oct. 5·50 | 20410·50 | +31·6 | $\overline{29}$·262 | −2·7 |
| Nov. 3·48 | 439·48 | 34·1 | ·288 | −0·6 |
| Dec. 7·41 | 473·41 | 36·6 | ·319 | +1·8 |
| 1986 Nov. 9·13 | 46743·13 | 34·2 | $\overline{5}$·380 | −0·2 |
| 1987 Sept. 29·13 | 47067·13 | 24·0 | $\overline{5}$·676 | +0·1 |
| 1989 Oct. 23·13 | 47822·13 | 34·6 | $\overline{4}$·368 | +0·1 |
| 2000 Oct. 6·17 | 51823·17 | 17·2 | 0·032 | −0·3 |
| 17·17 | 834·17 | 18·7 | ·043 | +0·1 |
| Nov. 4·23 | 852·23 | 20·2 | ·059 | −0·3 |
| 14·14 | 862·14 | 21·7 | ·068 | +0·2 |
| 20·15 | 868·15 | 22·5 | ·074 | +0·3 |
| 30.15 | 878·15 | 23·3 | ·083 | +0·1 |
| Dec. 9·12 | 887·12 | 24·3 | ·091 | +0·2 |
| 22·03 | 900·03 | 25·2 | ·103 | −0·1 |
| 2001 Jan. 6·99 | 51915·99 | 26·9 | 0·117 | +0·1 |
| 16·97 | 925·97 | 27·4 | ·127 | −0·2 |
| 27·94 | 936·94 | 28·2 | ·137 | −0·3 |
| Feb. 6·96 | 946·96 | 29·5 | ·146 | +0·3 |
| Mar. 4·92 | 972·92 | 31·3 | ·170 | +0·5 |
| 19·89 | 987·89 | 31·2 | ·183 | −0·4 |
| Sept. 30·13 | 52182·13 | 34·9 | ·361 | +0·3 |
| Oct. 12·19 | 194·19 | 34·6 | ·372 | +0·1 |
| 31·13 | 213·13 | 34·1 | ·390 | −0·1 |
| Dec. 12·09 | 255·09 | 33·4 | ·428 | −0·1 |
| 2002 Jan. 18·04 | 52292·04 | 32·4 | 0·462 | −0·3 |
| Feb. 20·86 | 325·86 | 32·1 | ·493 | +0·3 |
| Mar. 26·84 | 359·84 | 30·8 | ·524 | 0·0 |
| Apr. 7·84 | 371·84 | 30·6 | ·535 | +0·2 |
| Sept. 9·17 | 526·17 | 24·2 | ·676 | +0·3 |
| Oct. 4·20 | 551·20 | 22·4 | ·699 | −0·3 |
| Nov. 7·18 | 585·18 | 20·6 | ·730 | −0·2 |
| Dec. 5·06 | 613·06 | 19·3 | ·756 | +0·1 |
| 2003 Jan. 10·06 | 52649·06 | 17·3 | 0·789 | +0·1 |
| Feb. 13·95 | 683·95 | 14·9 | ·821 | −0·3 |
| Mar. 17·80 | 715·80 | 13·8 | ·850 | +0·3 |
| Apr. 6·84 | 735·84 | 12·4 | ·868 | −0·1 |
| Sept. 24·20 | 906·20 | 16·5 | 1·024 | −0·1 |
| Oct. 18·18 | 930·18 | 19·1 | ·046 | +0·1 |
| Nov. 13·14 | 956·14 | 22·1 | ·070 | +0·3 |
| Dec. 8·06 | 981·06 | 24·0 | ·093 | −0·3 |
| 2004 Jan. 9·04 | 53013·04 | 26·9 | 1·122 | −0·3 |
| Sept. 16·18 | 264·18 | 34·9 | ·352 | +0·3 |
| Dec. 27·12 | 366·12 | +32·5 | ·446 | −0·6 |

Wilson and OHP ones. An adjustment (judged in conjunction with that for HD 216218) of $-0 \cdot 5$ km s$^{-1}$ has been applied to the Cambridge measurements, and one of $+0 \cdot 8$ km s$^{-1}$ has been made to the others, all in an effort to maintain the zero-point that has normally been used in this series of papers. By themselves, the Cambridge velocities yield an orbit with a period of $1092 \cdot 2 \pm 1 \cdot 6$ days; when the OHP measurements from nearly 20 years ago are brought in (with equal weighting), the period is almost the same but its standard deviation is reduced to $0 \cdot 9$ days. The orbit is illustrated in Fig. 3, in which the Mount Wilson observations are also plotted; although they were not used in the solution they are seen to be in agreement with it as nearly as could be expected. The orbital elements are:

| | | | |
|---|---|---|---|
| $P$ | $= 1091 \cdot 8 \pm 0 \cdot 9$ days | $(T)_0$ | $=$ MJD $51788 \pm 4$ |
| $\gamma$ | $= +25 \cdot 02 \pm 0 \cdot 05$ km s$^{-1}$ | $a_1 \sin i$ | $= 173 \cdot 1 \pm 1 \cdot 6$ Gm |
| $K$ | $= 11 \cdot 90 \pm 0 \cdot 11$ km s$^{-1}$ | $f(m)$ | $= 0 \cdot 174 \pm 0 \cdot 005 \, M_\odot$ |
| $e$ | $= 0 \cdot 246 \pm 0 \cdot 006$ | | |
| $\omega$ | $= 223 \cdot 1 \pm 1 \cdot 7$ degrees | R.m.s. residual | $= 0 \cdot 25$ km s$^{-1}$ |

The number of days in three years is $1095^3/_4$, so the period that is determined here for 66 Ori differs from that exact value by $3 \cdot 95 \pm 0 \cdot 9$ days, causing the observing seasons to migrate very slowly around the orbit. On a given date of the year the system will be at one of three phases, which will migrate around the orbit in a cycle of nearly a thousand years. A given phase, however, will migrate around the year in a cycle of 'only' about 100 orbits or 300 years. The star is a mere 4° north of the equator — quite low in the Cambridge sky — and its right ascension is very close to the summer solstice; indeed it was carried past the solstitial colure by precession just 100 years ago. The observing season consequently extends very little beyond the equinoxes on either side of the winter solstice; the gaps in phase coverage of the orbit therefore occupy nearly $^1/_6$ of the cycle each, so it will be 50 years before they could be filled by

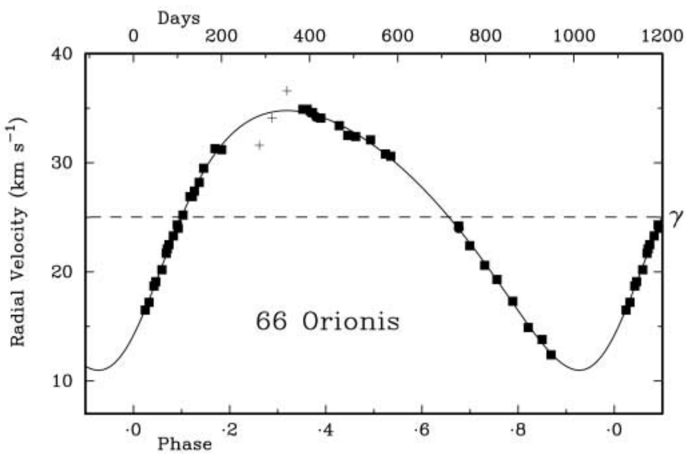


FIG. 3

As Fig. 1, but for 66 Orionis. The crosses plot velocities measured by Adams[27] 92 years ago.

observations made with the writer's instrumentation. However, the fact that 66 Ori is nearly 20° south of the ecliptic places northern observatories, particularly those at high latitudes, at a great disadvantage; from a moderate southern latitude there would be little if any intermission between observing seasons. Although the extremely patchy phase coverage makes the orbit appear cosmetically terrible, it can have only the most muted effect on its actual determination, which may be rated satisfactory according to the standards normally achievable with the *Coravel*s.

The mass function of $0 \cdot 174\ M_\odot$ is quite large enough to indicate an interesting lower limit to the mass of the undetected secondary star. The masses of late-type evolved stars of bright-giant luminosities are by no means well documented; one case that *is* is the primary component of the eclipsing composite-spectrum system HR 6902, which has been found[40] to have a type of G9 II — not far from that of the primary in 66 Ori — and a mass of $3 \cdot 9\ M_\odot$. For assumed masses of 3, 4, and $5\ M_\odot$ for the 66 Ori primary, the minimum masses of the secondary would be about $1 \cdot 5$, $1 \cdot 8$, and $2\ M_\odot$. Such masses would indicate a secondary no further down the main sequence than about F2, A9, or A6, respectively. Even an A6 V star would be four or five magnitudes fainter than the primary probably is, but the primary's luminosity is so uncertain that no figure with any pretence of accuracy can be given. The masses quoted are, however, minima, which would pertain if the orbital inclination were near 90°; significantly lower inclinations would correspond to masses that would be larger to any degree, so it is not impossible that a careful investigation would reveal the spectrum to be composite, or that in the far ultraviolet it will prove to be that of an A-type star.

### *HD 216218*

Like the three stars treated above, HD 216218 has been classified with luminosity class II, and (as in the cases of HD 14544 and HDE 237201) that is without doubt why it came to be on the programme of de Medeiros & Mayor[1] where its velocity variability came to light. It is an *Hipparcos* star, with a parallax of $0''\cdot 00314 \pm 0''\cdot 00103$, leading to a distance modulus of about $7^{m}\cdot 5$, albeit with a $1$-$\sigma$ uncertainty approaching one magnitude. The G9 II classification was made by Yoss[41] in the course of his early work on CN strengths determined from objective-prism spectra obtained with the *Burrell Schmidt*[16]; he used the 4° and 6° prisms in conjunction, to yield spectra with a reciprocal dispersion of 110 Å mm$^{-1}$ at H$\gamma$. The CN in HD 216218 was considered to be of normal intensity. The only other classification appears to be the *HD* type of K2. We are indebted, as so often happens, to *Tycho* for such photometry as is available, $V = 8^{m}\cdot 41$, $(B-V) = 1^{m}\cdot 04$. In the light of the distance modulus it therefore appears that the absolute magnitude is in the neighbourhood of $+1$, indicating that the star is a normal giant rather than being of bright-giant luminosity. The only other salient contribution to our knowledge of HD 216218 is the set of discordant radial-velocity measurements by de Medeiros & Mayor[1]. Three such measurements were promised in the published paper, but when the individual listing was produced there were more, as in the cases of HD 14544 and HD 237201 above: in the present case there were as many as five extra, making a total of eight OHP observations, which are listed at the head of Table IV. Like the other OHP velocities used in this paper, those in Table IV have been increased by $0 \cdot 8$ km s$^{-1}$.

TABLE IV

*Radial-velocity observations of HD 216218*

*The sources of the observations are as follows:*
*1986–1998 — OHP Coravel; 2002–2005 — Cambridge Coravel*

| Date (UT) | MJD | Velocity km s$^{-1}$ | Phase | (O–C) km s$^{-1}$ |
|---|---|---|---|---|
| 1986 July 17·10 | 46628·10 | −21·7 | $\bar{8}$·816 | +0·2 |
| Oct. 7·93 | 710·93 | −15·5 | ·929 | −0·2 |
| 1987 Aug. 29·05 | 47036·05 | −19·5 | $\bar{7}$·376 | −0·3 |
| 1997 Aug. 29·99 | 50689·99 | −19·5 | $\bar{2}$·391 | +0·1 |
| Sept. 3·97 | 694·97 | −19·8 | ·397 | 0·0 |
| 20·91 | 711·91 | −20·6 | ·421 | −0·2 |
| Nov. 13·78 | 765·78 | −22·5 | ·495 | −0·4 |
| 1998 Aug. 22·06 | 51047·06 | −18·9 | $\bar{2}$·881 | −0·1 |
| 2002 July 21·08 | 52476·08 | −21·0 | 0·842 | −0·1 |
| Aug. 13·15 | 499·15 | −19·3 | ·874 | −0·1 |
| Sept. 11·03 | 528·03 | −16·5 | ·913 | +0·1 |
| 29·05 | 546·05 | −14·9 | ·938 | −0·2 |
| Oct. 21·95 | 568·95 | −12·3 | ·969 | −0·1 |
| Nov. 6·97 | 584·97 | −10·9 | ·991 | −0·2 |
| Dec. 18·82 | 626·82 | −8·3 | 1·049 | +0·1 |
| 2003 Jan. 11·78 | 52650·78 | −8·4 | 1·082 | +0·1 |
| July 14·07 | 834·07 | −17·7 | ·333 | +0·2 |
| Aug. 9·08 | 860·08 | −19·1 | ·369 | −0·1 |
| Sept. 14·04 | 896·04 | −20·3 | ·418 | +0·1 |
| Nov. 12·89 | 955·89 | −22·3 | ·500 | −0·1 |
| Dec. 15·80 | 988·80 | −22·8 | ·546 | +0·2 |
| 2004 Jan. 12·73 | 53016·73 | −23·4 | 1·584 | +0·1 |
| May 17·12 | 142·12 | −23·6 | ·756 | −0·2 |
| June 22·09 | 178·09 | −22·5 | ·805 | −0·2 |
| July 6·07 | 192·07 | −21·3 | ·825 | +0·3 |
| Aug. 31·03 | 248·03 | −17·1 | ·901 | +0·3 |
| Oct. 5·99 | 283·99 | −13·6 | ·951 | +0·1 |
| 25·92 | 303·92 | −11·6 | ·978 | 0·0 |
| Nov. 13·86 | 322·86 | −9·9 | 2·004 | 0·0 |
| 26·83 | 335·83 | −8·8 | ·022 | +0·3 |
| 2005 Feb. 8·75 | 53409·75 | −9·9 | 2·123 | −0·4 |
| May 15·11 | 505·11 | −15·2 | ·254 | −0·3 |
| June 9·08 | 530·08 | −16·1 | ·288 | +0·2 |
| 23·08 | 544·08 | −16·5 | ·308 | +0·5 |
| Oct. 25·93 | 668·93 | −21·7 | ·479 | +0·1 |

The 39 Cambridge radial-velocity observations made since 2002 July appear in Table IV. They have had an adjustment of −0·5 km s$^{-1}$ applied to them, the same as that applied to the corresponding measures of 66 Ori, a star of very similar colour. Alone, they give an orbit with a period of 726·4 days; the addition (with equal weight) of the OHP data, which began almost 20 years ago, refines it to 728·62 days. The complete set of elements is given below, and the velocity curve is shown in Fig. 4.

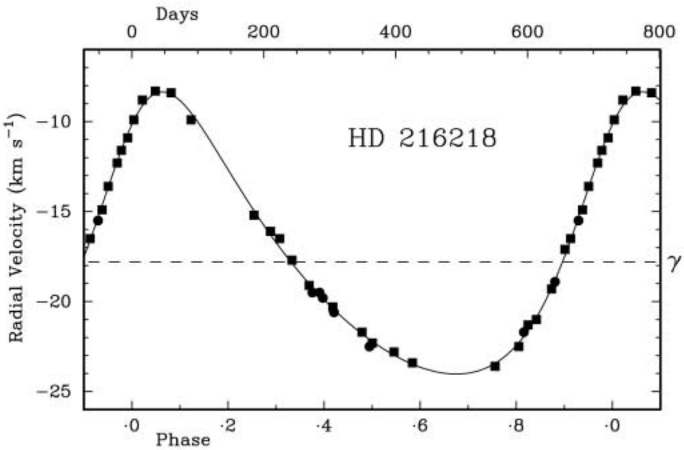


FIG. 4
As Fig. 1, but for HD 216218.

| | | | | | |
|---|---|---|---|---|---|
| $P$ | = | $728 \cdot 62 \pm 0 \cdot 23$ days | $(T)_0$ | = | MJD $51862 \cdot 7 \pm 2 \cdot 9$ |
| $\gamma$ | = | $-17 \cdot 81 \pm 0 \cdot 04$ km s$^{-1}$ | $a_1 \sin i$ | = | $75 \cdot 7 \pm 0 \cdot 7$ Gm |
| $K$ | = | $7 \cdot 84 \pm 0 \cdot 07$ km s$^{-1}$ | $f(m)$ | = | $0 \cdot 0326 \pm 0 \cdot 0009$ $M_\odot$ |
| $e$ | = | $0 \cdot 269 \pm 0 \cdot 008$ | | | |
| $\omega$ | = | $320 \cdot 2 \pm 1 \cdot 6$ degrees | R.m.s. residual | = | $0 \cdot 21$ km s$^{-1}$ |

Once again the period is very close to an integral number of years, being just $1 \cdot 88 \pm 0 \cdot 23$ days short of 2 years, so the phases seen at a given date in the year migrate round the orbit in a cycle of about 400 orbits or 800 years, while the dates at a given phase migrate round the year in half that time, which is still much too long to delay this paper while we wait for the phase coverage to even itself out! The star is less unfavourably placed than 66 Ori, however, being about 1° north of $\mu$ Peg, and the only calendar months in which it is totally inaccessible to the Cambridge telescope are March and April. Once again, the gaps in phase coverage look bad in the figure, but probably cause little real degradation of the precision of the orbital elements.

*HD 220102*

HD 220102 is a star almost (but not quite) bright enough for the *Bright Star Catalogue*, to be found in the centre of the approximately equilateral triangle of $5^{m}$ stars 1, 4, and AR Cassiopeiae. It is not actually the star (HD 220369 = HR 8894, $5^{m} \cdot 56$) shown in *Norton's*[42] at that place, but is about 20′ north-preceding it. It is very close to the galactic plane, in the area of the 'Cas OB2 Association'. Deutschman *et al.*[43] measured its magnitude and colours as $V = 6^{m} \cdot 63$, $(B-V) = 0^{m} \cdot 63$, $(U-B) = 0^{m} \cdot 46$; similar values were given for $V$ and $(B-V)$ by Bouigue[44], but Parsons & Montemayor[45] gave a slightly bluer colour, $0^{m} \cdot 59$, for $(B-V)$. That colour index is near to the one measured by

*Tycho*, whose value depends upon whether one looks at the *Hipparcos* catalogue itself[46] ($0^m \cdot 598$), *Vizier* ($0^m \cdot 585$), or *Simbad* ($0^m \cdot 547$).

The spectral type of HD 220102 has been something of a bone of contention. The *HD* type is F2. By virtue of its position in Selected Area[47] 19, the star was included in the *Bergedorfer Spektral-Durchmusterung*[48], in which it is listed with type F5. The F2 type was given again by Young[49] in 1939 from the David Dunlap Observatory, but Christie & Wilson[50], at about the same time, recognized supergiant characteristics in the spectrum for the first time and classified it as cF3. Bouigue[44], however, at Haute-Provence and/or Toulouse, considered it to be a main-sequence star, giving its type as F2 V, but other papers[51–53] from the same observatories listed it as F5 II; to what extent they copied it from paper to paper or to what extent they were giving independent classifications is not clear. Georgelin[53] deduced that the star has a colour excess of $0^m \cdot 25$ and a distance of 400 pc. Baker[54] derived absolute magnitudes from the equivalent widths of the oxygen triplet at λ7774 Å and obtained $M_V = -5 \cdot 06$, leading him to offer the type of F2 Ib.

Parsons & Montemayor[45] found a 'photometric type', from photometry that included bands in the 'space ultraviolet', of F0 I. A Soviet group[55] also utilized space-ultraviolet data, from *Glazar*, and deduced that HD 220102 must have a hot companion, whose spectral type might be anything from B8 Ib–II to sdOB (according to the text) or to sdB (according to their Table IV). One might suppose that the need to postulate the hot companion would arise from an excess of flux in the ultraviolet, but in a second paper[56] a group with overlapping membership referred to excess *absorption* in the ultraviolet and deduced that there exists a circumstellar envelope. They gave a type of F0 I and a distance of 1190 pc, and considered that there is absorption at λ1640 Å amounting to $3^m \cdot 50$, of which $1^m \cdot 56$ is to be attributed to the envelope, for which they deduced a radius of 960 AU and total mass of $3 \cdot 6 \times 10^{-4}\ M_\odot$. The existence of circumstellar dust was suggested by a consideration of infrared colour indices from *IRAS*. In still another paper[57], again with overlapping authorship, the ideas of circumstellar material and a sdB stellar companion were put forward simultaneously, the distance then being put at just 1000 pc. Bersier[58] interpreted Geneva photometry as implying a colour excess $E(B-V)$ of $0^m \cdot 247$, but there is room for suspicion that any such estimate could be vitiated by the unconsidered presence of a hot companion. Comparatively recently, Gray and (different sets of) collaborators offered two papers[59,60] concerned with the physcial basis for luminosity classification. They[59] gave "precise spectral types" for a good many stars, of which HD 220102 was one and was classed as F2 II and attributed a colour excess of $0^m \cdot 25$, the figure already proposed both by Georgelin[52] and by Bersier[58]. In the second paper[60] the excess was given as $0^m \cdot 24$, and the stellar characteristics were quantified as $T_{eff} = 6880 \pm 80$ K, $\log g = 2 \cdot 05 \pm 0 \cdot 1$, and the 'microturbulence' $\xi_t = 4 \cdot 7 \pm 0 \cdot 5$ km s$^{-1}$. Shortly afterwards, Andrievsky *et al.*[61] adopted a model for which the corresponding quantitites were 6600K, $1 \cdot 0 \pm 0 \cdot 2$, and $2 \cdot 7$, respectively, so it seems that there remain even now some discrepancies in the characterization of HD 220102. The discordance in the surface gravities is particularly disturbing, corresponding as it does to a factor of 11 in either mass or luminosity; it amounts to $4 \cdot 7$ times the quadratic sum of the claimed standard errors, so at least one of the values[60,61] must be actually mistaken.

There can be little doubt, however, of the star's high luminosity, since the *Hipparcos* parallax is zero to well within its uncertainty and suggests a distance at least of the order of 1 kpc. All the same, a small *caveat* needs to be sounded, since the star is quite near the ecliptic pole and will be shown to have an orbit with a period very close to one year — just the sort to stultify all efforts to determine a parallax. The $a_1 \sin i$ value is little more than a sixth of an astronomical unit, and the photocentric motion can be no greater (and if the companion is of non-negligible luminosity would be less) than the orbital motion of the primary. In the absence of any information about $\sin i$, however — and the mass function would allow quite a small value for it — we cannot be certain that the quantity $a$ itself is not comparable with 1 AU or that the photocentric orbit would not therefore correspondingly be of an angular size comparable to that of the parallactic one. There is no way of knowing whether the orientation and phasing of the photocentric orbit are such as to reinforce, diminish, or to be separable from, the apparent motion arising from annual parallax; even the direction of movement in the orbit is unknown.

Before de Medeiros & Mayor[1] observed it, HD 220102 had already featured in no fewer than five papers giving radial velocities. First, Christie & Wilson[50] reported a mean of $-21 \cdot 9$ km s$^{-1}$ from three Mount Wilson plates, whose individual dates and velocities were subsequently provided by Abt[62]. At almost the same time Young[49] offered a mean of $-27 \cdot 2$ km s$^{-1}$, with a 'probable error' of $2 \cdot 1$ km s$^{-1}$, from four plates taken at the Cassegrain focus of the *DDO* 74-inch reflector. It is perhaps regrettable that nobody has undertaken for the *DDO* the job that Abt[34,62] did for Mount Wilson, by publishing individually the results that have been listed only as means. Young did in fact suspect variability in the velocity of HD 220102, because the star is flagged as having "a somewhat larger range than the agreement of the lines would lead one to expect", and the extreme range of the four plates is noted as 16 km s$^{-1}$. Then three papers[51–53] were published (in duplicate, in the *Journal des Observateurs* and the *Publications de l'Observatoire de Haute-Provence*), giving mean values of velocities measured by the French objective-prism procedure; their results were $-32$ B7, $-30$ D4, and $-37$ B7, respectively, where the first number is the mean velocity, the letter indicates a range of 'probable errors', of which the relevant ones are $2 \cdot 5$–5 km s$^{-1}$ for B and $>10$ km s$^{-1}$ for D, and the ensuing number indicates how many individual measurements were averaged to obtain the specified mean. It has to be said that none of the velocities mentioned in this paragraph can contribute usefully to the discussion in this paper, and they are therefore not mentioned again. The Mount Wilson velocities seem very poor (two of the three could not in any case be plotted in the orbit diagram below because they would fall well outside its boundaries), while the others are presented only as means, and anyway the objective-prism velocities are never accurate enough to be used in orbits. Indeed, even in the hands of the originator of the technique, a venture to produce a spectroscopic orbit[63] proved counter-productive, as has been discussed in sufficient detail elsewhere[64].

De Medeiros & Mayor[1] gave a mean velocity for HD 220102 from no fewer than 39 *Coravel* measurements, and clearly identified it as a spectroscopic binary. Their subsequent listing actually included 42 velocities, which readily yielded an orbit without requiring any additional input. Those authors may have refrained from publishing such an orbit themselves owing to the extremely poor phase distribution of their data: the period was $373 \cdot 6$ days, and with one exception the observations were restricted to the months from June to December. Although the total time span was nearly 18 years, during which the 8-day discrepancy between the period and the exact year would have built up to a very useful extent to enable the phase gap to be reduced, no advantage was taken of it, perhaps because the orbit had not in fact been solved during the observing campaign.

The present writer therefore made it his business to regularize the phase distribution by making additional observations at the phases that were unrepresented in the OHP data, a task that has required them to be started at the beginning of April, when the star is over six hours east of the meridian in the dawn twilight, at the very limit of the hour-angle range of the Cambridge telescope. A total of 43 measurements has been made at Cambridge; they deliberately include a few made at phases that were already well covered at OHP, in an effort to ensure the correct determination of relative zero-point. In the final solution of the orbit, the OHP velocities have been offset by $+0 \cdot 8$ km s$^{-1}$ as usual, and have been given half-weight to bring their variance into line with that of the Cambridge measures. The observations are listed in Table V; their solution is plotted in Fig. 5, and its elements are:

| | | |
|---|---|---|
| $P = 373 \cdot 62 \pm 0 \cdot 07$ days | $(T)_{19} = $ MJD $51247 \cdot 3 \pm 1 \cdot 2$ | |
| $\gamma = -29 \cdot 47 \pm 0 \cdot 04$ km s$^{-1}$ | $a_1 \sin i = 26 \cdot 88 \pm 0 \cdot 29$ Gm | |
| $K = 5 \cdot 69 \pm 0 \cdot 06$ km s$^{-1}$ | $f(m) = 0 \cdot 00555 \pm 0 \cdot 00018\ M_{\odot}$ | |
| $e = 0 \cdot 392 \pm 0 \cdot 009$ | | |
| $\omega = 169 \cdot 2 \pm 1 \cdot 6$ degrees | R.m.s. residual (unit weight) $= 0 \cdot 29$ km s$^{-1}$ | |

The period is seen to differ from one year by $8 \cdot 37 \pm 0 \cdot 07$ days — a less exact coincidence than in the other cases treated here — so it migrates in such a fashion as to 'lose' a year on the calendar every 43 or 44 orbits (44 or 45 years). Enough of that migration cycle has elapsed to enable the orbit to be covered at all phases, as has not been possible with the other stars. Between phases $\cdot 3$ and $\cdot 5$, Fig. 5 shows seemingly nasty runs of residuals, first positive and then negative, but what (if any) significance they may have is unknown. The dip seen in radial-velocity traces of HD 220102 is considerably broadened: the $v \sin i$

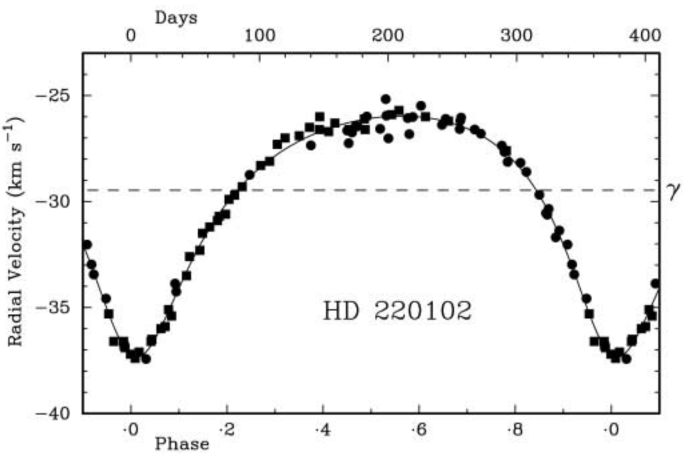


FIG. 5

As Fig. 1, but for HD 220102.

TABLE V

*Radial-velocity observations of HD 220102*

*The sources of the observations are as follows:*
*1979–1998 — OHP Coravel (weight 1/2); 2002–2004 — Cambridge Coravel*

| *Date (UT)* | *MJD* | *Velocity km s⁻¹* | *Phase* | *(O−C) km s⁻¹* |
|---|---|---|---|---|
| 1979 Nov. 5·88 | 44182·88 | −33·9 | 0·092 | +0·6 |
| 6·85 | 183·85 | −34·3 | ·094 | +0·1 |
| 1980 Oct. 21·92 | 44533·92 | −37·4 | 1·031 | −0·3 |
| 1981 Aug. 31·03 | 44847·03 | −30·3 | 1·869 | +0·1 |
| 1986 July 24·05 | 46635·05 | −26·1 | 6·655 | +0·1 |
| Aug. 5·07 | 647·07 | −26·0 | ·687 | +0·4 |
| Oct. 4·89 | 707·89 | −29·7 | ·850 | 0·0 |
| 10·98 | 713·98 | −30·6 | ·866 | −0·3 |
| Dec. 2·77 | 766·77 | −37·3 | 7·008 | 0·0 |
| 1987 June 16·08 | 46962·08 | −25·2 | 7·530 | +0·8 |
| July 7·10 | 983·10 | −26·0 | ·587 | 0·0 |
| Aug. 13·06 | 47020·06 | −26·2 | ·686 | +0·2 |
| 29·03 | 036·03 | −26·8 | ·728 | +0·1 |
| Sept. 28·86 | 066·86 | −28·2 | ·811 | +0·3 |
| Nov. 7·93 | 106·93 | −33·0 | ·918 | +0·1 |
| 1988 July 21·07 | 47363·07 | −25·4 | 8·604 | +0·6 |
| Sept. 21·95 | 425·95 | −27·4 | ·772 | +0·2 |
| Nov. 12·01 | 477·01 | −32·0 | ·909 | +0·5 |
| 26·82 | 491·82 | −34·6 | ·948 | +0·4 |
| 1989 June 9·06 | 47686·06 | −26·4 | 9·468 | −0·2 |
| Aug. 15·07 | 753·07 | −26·4 | ·647 | −0·2 |
| Nov. 25·89 | 855·89 | −33·4 | ·923 | −0·1 |
| 1990 Mar. 27·12 | 47977·12 | −28·7 | 10·247 | +0·1 |
| June 19·08 | 48061·08 | −26·4 | ·472 | −0·3 |
| July 28·00 | 100·00 | −26·1 | ·576 | −0·1 |
| Sept. 18·06 | 152·06 | −26·6 | ·715 | +0·1 |
| 1991 July 20·08 | 48457·08 | −25·9 | 11·532 | 0·0 |
| Sept. 15·01 | 514·01 | −26·6 | ·684 | −0·2 |
| Oct. 19·89 | 548·90 | −27·7 | ·778 | 0·0 |
| Nov. 28·75 | 588·75 | −31·7 | ·884 | −0·5 |
| 1992 Nov. 28·78 | 48954·78 | −30·6 | 12·864 | −0·3 |
| 1993 July 21·08 | 49189·08 | −26·0 | 13·491 | +0·1 |
| Dec. 17·77 | 338·77 | −31·4 | ·892 | +0·2 |
| 1994 Aug. 9·08 | 49573·08 | −26·6 | 14·519 | −0·6 |
| Nov. 30·80 | 686·80 | −28·6 | ·823 | +0·2 |
| 1995 July 23·09 | 49921·09 | −26·6 | 15·450 | −0·4 |
| Aug. 24·08 | 953·08 | −27·0 | ·536 | −1·0 |
| Nov. 24·82 | 50045·82 | −28·1 | ·784 | −0·3 |
| 1996 Sept. 17·00 | 50343·00 | −26·8 | 16·580 | −0·8 |

TABLE V (*concluded*)

| *Date (UT)* | | | *MJD* | *Velocity* *km s*$^{-1}$ | *Phase* | *(O – C)* *km s*$^{-1}$ |
|---|---|---|---|---|---|---|
| 1997 | July | 11·10 | 50640·10 | −27·3 | 17·375 | −0·5 |
| | Aug. | 12·08 | 672·08 | −26·7 | ·460 | −0·5 |
| 1998 | Aug. | 18·08 | 51043·08 | −27·2 | 18·453 | −1·0 |
| 2002 | July | 14·10 | 52469·10 | −28·3 | 22·270 | +0·1 |
| | | 21·11 | 476·11 | −28·1 | ·289 | −0·1 |
| | | 27·08 | 482·08 | −27·3 | ·305 | +0·5 |
| | Aug. | 2·10 | 488·10 | −27·0 | ·321 | +0·5 |
| | | 13·10 | 499·10 | −26·9 | ·350 | +0·2 |
| | | 21·13 | 507·13 | −26·5 | ·372 | +0·4 |
| | | 29·09 | 515·09 | −26·0 | ·393 | +0·7 |
| | Sept. | 5·03 | 522·03 | −26·7 | ·412 | −0·2 |
| | Oct. | 24·00 | 571·00 | −25·9 | ·543 | +0·1 |
| 2003 | Apr. | 7·16 | 52736·16 | −36·6 | 22·985 | +0·2 |
| | | 8·18 | 737·18 | −36·9 | ·988 | 0·0 |
| | | 16·15 | 745·15 | −37·4 | 23·009 | −0·1 |
| | | 19·15 | 748·15 | −37·1 | ·017 | +0·2 |
| | | 29·12 | 758·12 | −36·5 | ·044 | +0·2 |
| | May | 6·12 | 765·12 | −36·0 | ·062 | 0·0 |
| | | 12·09 | 771·09 | −35·1 | ·078 | +0·1 |
| | | 26·09 | 785·09 | −33·5 | ·116 | −0·2 |
| | June | 13·09 | 803·09 | −31·2 | ·164 | +0·1 |
| | | 19·08 | 809·08 | −30·9 | ·180 | −0·2 |
| | | 28·08 | 818·08 | −29·9 | ·204 | 0·0 |
| | Oct. | 12·00 | 924·00 | −26·6 | ·488 | −0·5 |
| | Nov. | 27·93 | 970·93 | −26·0 | ·613 | 0·0 |
| | Dec. | 15·85 | 988·85 | −26·2 | ·661 | +0·1 |
| 2004 | Jan. | 29·82 | 53033·82 | −27·6 | 23·782 | +0·1 |
| | Apr. | 3·18 | 098·18 | −35·3 | ·954 | 0·0 |
| | | 7·17 | 102·17 | −36·6 | ·965 | −0·7 |
| | | 15·15 | 110·15 | −36·8 | ·986 | +0·1 |
| | | 20·14 | 115·14 | −37·2 | ·999 | 0·0 |
| | May | 6·10 | 131·10 | −36·6 | 24·042 | +0·2 |
| | | 17·09 | 142·09 | −35·9 | ·071 | −0·4 |
| | | 22·08 | 147·08 | −35·4 | ·085 | −0·5 |
| | June | 5·08 | 161·08 | −32·6 | ·122 | +0·4 |
| | | 13·05 | 169·05 | −32·3 | ·144 | −0·2 |
| | | 15·08 | 171·08 | −31·5 | ·149 | +0·3 |
| | | 28·08 | 184·08 | −30·7 | ·184 | −0·1 |
| | July | 3·08 | 189·08 | −30·6 | ·197 | −0·4 |
| | | 10·08 | 196·08 | −29·7 | ·216 | −0·1 |
| | | 16·00 | 202·00 | −29·3 | ·232 | −0·1 |
| | Sept. | 14·06 | 262·06 | −26·6 | ·392 | +0·1 |
| | | 26·05 | 274·05 | −26·3 | ·425 | +0·2 |
| | Oct. | 7·05 | 285·05 | −26·6 | ·454 | −0·3 |
| | | 19·00 | 297·00 | −26·1 | ·486 | 0·0 |
| | Nov. | 14·94 | 323·94 | −25·7 | ·558 | +0·3 |

value given by de Medeiros & Mayor[1] is 7·7 km s$^{-1}$ with an uncertainty given as 1 km s$^{-1}$, which is intended to be not the formal standard error but a self-imposed lower limit to the claimed accuracy of the result. On the same basis the Cambridge value is 8·6 ± 1 km s$^{-1}$, although the formal s.d. of the mean is less than 0·1 km s$^{-1}$; the Cambridge value could be expected to be mildly inflated by extra (but ignored) turbulent broadening in the atmosphere of the high-luminosity star. It is not impossible that the rotation of the star is in forced synchronization with the orbit. At the eccentricity of the HD 220102 orbit, the pseudo-synchronous rotation period is very nearly half the orbital period, and to obtain a $v \sin i$ of $7 \cdot 7$ km s$^{-1}$ at that period requires that $R \sin i \sim 29\ R_\odot$. We notice that that is not much less than the $a_1 \sin i$ value in the table above, which equates to about 39 $R_\odot$, although there is no reason to regard the comparability as objectionable.

If we consider HD 220102 to be at a distance of 1 kpc and correspondingly to have a modulus of ten magnitudes, its observed apparent magnitude leads to a luminosity estimate of $M_V \sim -3^{\mathrm{m}} \cdot 4$; when account is taken of the quarter-magnitude reddening that has been repeatedly suggested[53,58,59] it is raised to slightly brighter than $-4^{\mathrm{m}}$, so the appropriate luminosity class might be Ib–II. There remains, however, considerable scope for re-consideration of the distance, as well as for determining the nature of the companion star. If the companion is really a B star, as proposed by Tovmassian *et al.*[55], then the small mass function must betoken quite a low orbital inclination, perhaps in the region of 20–30 degrees, although the mass numbers to be inserted in the function are somewhat conjectural. That would increase the radius that corresponds to pseudo-synchronism by a factor of two or three, making it perhaps less likely that the rotation is synchronized or else emphasizing the high-luminosity nature of the primary star.

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# IS THE CEPHEID V1726 CYGNI AN OVERTONE PULSATOR?

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A re-analysis of the interstellar extinction corrections for stars near V1726 Cyg in the cluster Anon Platais yields a space reddening for the Cepheid of $E_{B-V} = 0 \cdot 33 \pm 0 \cdot 02$, consistent with a revised spectroscopic reddening for the star of $E_{B-V} = 0 \cdot 36 \pm 0 \cdot 03$. At the distance of the cluster Anon Platais, that parameter leads to a luminosity of $\langle M_V \rangle = -3 \cdot 08 \pm 0 \cdot 07$ for V1726 Cyg, consistent with fundamental-mode pulsation. There is no strong evidence for overtone pulsation in the Cepheid, despite its sinusoidal light curve.

*Introduction*

V1726 Cyg (= BD + 48° 3398) is a small-amplitude, sinusoidal Cepheid that lies on the outskirts of a sparsely-populated, anonymous, open cluster discovered by Platais[1] while studying the much richer, nearby cluster M39. The available photometric, proper motion, and radial-velocity data for V1726 Cyg and cluster stars[2,3] provide strong evidence that the Cepheid is a member of the cluster Anon Platais. As a result, the reddening and distance inferred for V1726 Cyg from nearby cluster stars imply that it is an overtone pulsator, as also argued on the basis of its rate of period change[4]. The Cepheid was not included in the *Hipparcos* parallax programme[5], so the cluster distance cannot be confirmed astrometrically. The radius for the Cepheid obtained from its pulsation period[6], however, gives a luminosity consistent with cluster membership for either fundamental-mode or overtone pulsation.

The interpretation of the properties of V1726 Cyg as a cluster member relies directly upon a knowledge of the star's interstellar reddening. Two independent estimates have been published: a space reddening of $E_{B-V} = 0{\cdot}43 \pm 0{\cdot}02$ derived from two photometrically-identified B-type stars bracketing V1726 Cyg[2], and a value of $E_{B-V} = 0{\cdot}34$ established from spectroscopic observation of the Cepheid[3]. Given that both estimates are mutually exclusive, we decided to re-examine the inferred space reddening for V1726 Cyg and its implications for the pulsation mode of the Cepheid.

*The reddening of V1726 Cyg*

The original photometric reddening of V1726 Cyg relied upon the inferred properties of only two spatially-adjacent stars, yet there are several other reddened stars lying within a few arcminutes of the Cepheid. An additional complication is that both objects have colours that match those of a range of stars that, depending upon rotation rate and aspect angle, could be reddened dwarfs with intrinsic properties anywhere in the B9·5–A5 spectral range, where rapid rotation is fairly common[7]. The intrinsic *UBV* colours of such stars depend directly upon their rotational velocities and the inclination of the rotation axes to the line of sight[8], and could account for systematic errors as large as ±0·10 in colour excesses $E_{B-V}$ derived by dereddening a star's observed colours to the zero-age zero-rotation main sequence[9]. In general, rapid rotators are more frequently encountered in cluster nuclei than in cluster coronae[9], although the field of V1726 Cyg lies in the corona of Anon Platais[2].

The potential for a biassed photometric reddening in the case of V1726 Cyg is established by the fact that both bracketing stars have alternative de-reddening solutions consistent with a smaller space reddening. The reddening of cluster stars was therefore re-examined, and *JHK* data from the 2MASS survey[10] were included as a means of testing the derived solutions. The results are presented in Fig. 1 as a detailed map of the reddening in the field immediately surrounding V1726 Cyg, the plot showing derived colour excesses, in units of $0^{m}{\cdot}01$, for stars in Anon Platais roughly as a function of spatial location. Different symbols are used to identify the colour excesses for likely F-type stars (most trustworthy), B9·5–A5 dwarfs (least trustworthy), and B-type stars (reasonably trustworthy). The F-type stars are denoted by normal numbers, the B-type stars by italicized numbers, and the B9·5–A5 dwarfs by both solutions separated by a slash (*e.g.*, 23/44). Given the effects of rapid rotation on the colours of stars in the last group[8], their true space reddenings could lie anywhere in the interval between

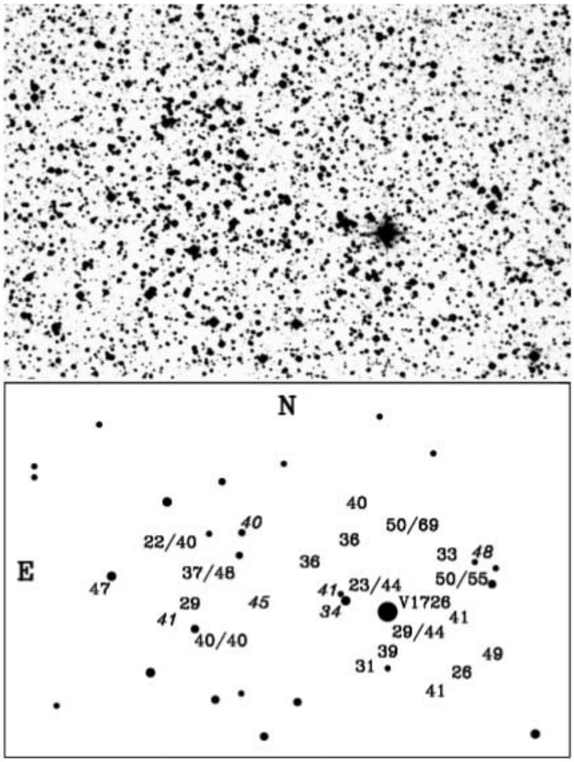


FIG. 1

A finder chart for the $15' \times 10'$ field of V1726 Cygni (top), adapted from the red image of the field in the Palomar Observatory Sky Survey. The bottom section is a schematic of the same field to illustrate the spatial variation of reddening across the field. Numbers denote the colour excess $E_{B-V}$ in units of $0^{m}\cdot 01$ for several cluster members, particularly near V1726 Cyg, which is identified. Normal numbers denote AF and F-type stars, italics denote mid-to-late B-type stars, and slashed numbers denote the double solutions for B9·5–A5 stars.

the two listed values. All values have been converted to equivalent colour excess $E_{B-V}$ (B0) for a B0 dwarf[11], and there appears to be no distinction between the results for photoelectrically and photographically observed stars.

There is only one F-type star within $1'$ of V1726 Cyg; its derived reddening is $E_{B-V}\,(\mathrm{B0}) = 0\cdot 36$. There are seven F-type stars within $2'$ of V1726 Cyg, and they have an average space reddening of $E_{B-V}\,(\mathrm{B0}) = 0\cdot 35 \pm 0\cdot 02$. An identical result is obtained when one includes the colour excesses for two B-type dwarfs lying within $2'$ of V1726 Cyg. The average field reddening established by AF-type and B-type stars corresponds to $E_{B-V} = 0\cdot 33 \pm 0\cdot 02$ s.e. for a star with the colours of V1726 Cyg[11].

The new space reddening for the Cepheid is slightly smaller than what is observed in the core of Anon Platais[2], but is notably not as large as the earlier estimate of $E_{B-V} = 0\cdot 43 \pm 0\cdot 02$, which must be considered as erroneous. The inferred space reddening for the Cepheid is consistent with its spectroscopic reddening[3], but that is also in need of adjustment.

The study of V1726 Cyg and Anon Platais by Usenko *et al.*[3] was based upon a single high-resolution echelle spectrogram of the Cepheid obtained on HJD 2451003·231. The inferred pulsation phase was $\phi = 0\cdot109$ in the original study[3], but is $\phi = 0\cdot028$ according to the updated ephemeris for the Cepheid[4]. The star reaches its bluest colours at about the same phase, just after light maximum, with an observed value[2] of $B-V = 0\cdot84 \pm 0\cdot01$. The effective temperature of V1726 Cyg at that phase was inferred from a very sensitive line-ratio technique[12]. The resulting value of 6275 ± 80 K corresponds to $(B-V)_0 = 0\cdot48 \pm 0\cdot025$ according to the relationship[13] adopted in the study. The implied spectroscopic reddening for the Cepheid is $E_{B-V} = 0\cdot36 \pm 0\cdot03$, similar to the newly derived space reddening of $E_{B-V} = 0\cdot33 \pm 0\cdot02$. The spectroscopic and space reddenings now agree to within their cited uncertainties, although we adopt the space reddening in what follows because of its slightly higher precision.

*The pulsation mode of V1726 Cyg*

With the space reddening derived here and the parameters for Anon Platais found earlier from zero-age main sequence fitting[2], the derived luminosity for V1726 Cyg is $M_V = -3\cdot08 \pm 0\cdot07$, close to the value obtained by Platais[1] in his original photographic study of the field. By way of comparison, the expected luminosity[2] for a fundamental-mode pulsator with the period of the Cepheid is $M_V = -2\cdot99 \pm 0\cdot07$; that for an overtone pulsator is $M_V = -3\cdot47 \pm 0\cdot07$. The case for overtone pulsation in V1726 Cyg presented earlier[2,4] is clearly no longer supported by the new evidence on its interstellar reddening and derived luminosity.

It is worth noting that the photometric study[2] of V1726 Cyg as a member of Anon Platais pointed out a strong argument for fundamental-mode pulsation in the Cepheid, namely the parameters for V1726 Cyg as an overtone pulsator lead to an unacceptably small value for the colour term in the period–luminosity–colour (PLC) relation. The evidence in favour of overtone pulsation involved the Cepheid's overly blue intrinsic colour, a feature that is eliminated with the new space reddening derived here, as well as its Fourier parameters $\phi_{21}$ and $\phi_{31}$, which are similar to those of Cepheids identified as probable overtone pulsators[14]. The significance of second-order or higher terms in Fourier decompositions of small-amplitude Cepheids like V1726 Cyg that have sinusoidal light curves is now recognized to be fairly low[15], so all objections to fundamental-mode pulsation in V1726 Cyg may no longer apply.

An independent case can be made using the star's rapid rate of period increase[4], which originally seemed to require the assumption of overtone pulsation to be consistent with the rates of period increase observed in other Cepheids in the third crossing of the instability strip. That problem is now resolved by more detailed information currently available on rates of period change observed in galactic Cepheids[16]. Because of the finite width of the instability strip in effective temperature, Cepheids lying on the hot edge of the strip are as much as 10% more massive than those on the cool edge of the strip of identical pulsation period[16]. That is reflected in a spread in rates of period change for stars of identical period, reflecting the mass dependence for their different rates of evolution across the strip. The large rate of period increase for V1726 Cyg relative to other Cepheids with periods of 4 days can now be attributed to its location on the hot side of the instability strip, consistent with its small light amplitude[16].

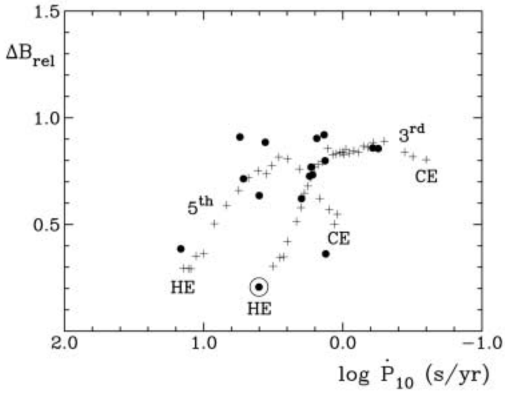


FIG. 2

A plot of light amplitude $\Delta B$ relative to the maximum value for each pulsation period *versus* rate of period change $\dot{P}_{10}$ as normalized to Cepheids with periods of 10 days. Dots are Cepheids exhibiting period increases with $P = 4 \cdot 2 \pm 1 \cdot 0$ days, while plus signs indicate the general trends observed for Cepheids with periods from 4 to 8 days. Separate trends for Cepheids identified as being in third and fifth crossings are indicated, along with where the hot edge (HE) and cool edge (CE) of the strip lie for each crossing. V1726 Cyg is the circled point.

The last point is illustrated in Fig. 2, which depicts the observed rates of period increase for Cepheids with periods of $P = 4 \cdot 2 \pm 1 \cdot 0$ days, as taken from an on-going compilation of Cepheid period changes[16]. V1726 Cyg, with its large rate of period change and small light amplitude, $\Delta B$, falls on the hot edge of the instability strip for third-crossing Cepheids, consistent with its other known parameters.

In Fig. 2 the rate of period change $\dot{P}$, as adjusted to its equivalent value for a 10-day Cepheid, is used as a surrogate for effective temperature to map the location of Cepheids in the instability strip. The variation of blue amplitude $\Delta B$ *versus* $\dot{P}$ is depicted for Cepheids having periods of 4–8 days, with the plotted amplitudes normalized relative to the maximum observed values at each period. The observed dispersion in the values of $\dot{P}$ for short-period Cepheids exhibiting period increases implies the existence of pulsators in both third and fifth crossings of the instability strip[16]. There is a similar dispersion in $\dot{P}$ for short-period Cepheids exhibiting period decreases that implies the existence of pulsators in both second and fourth crossings of the strip. V1726 Cyg can be identified as the Cepheid lying closest to the hot edge of the instability strip among those with periods less than 8 days, according to currently available information on rates of period change.

An additional argument regarding the location of V1726 Cyg on the hot edge of the instability strip is its surface gravity of $\log g = 2 \cdot 3$, obtained spectroscopically[3] at a time when the Cepheid was near mean radius. Cepheids of comparable pulsation period, Y Lac and T Vul, have slightly smaller values of $\log g = 2 \cdot 05 \pm 0 \cdot 25$ and $\log g = 2 \cdot 0$, respectively[17], consistent with expectations for stars of slightly lower mass lying closer to the centre of the instability strip.

*Discussion*

Since the inferred parameters for V1726 Cyg as a member of Anon Platais are modified in the present study, it is informative to re-examine the cluster colour–magnitude diagram (CMD) to test the star's new properties against those for other likely cluster members. Fig. 3 illustrates a reddening-corrected CMD for high-probability cluster stars, including V1726 Cyg. Theoretical isochrones corresponding to an age of $8 \cdot 0 \pm 0 \cdot 1$ years and solar metallicity are shown for comparison. The isochrone for $\log t = 8 \cdot 0$ produces reasonably good agreement in most respects, except that it does not cross the region of the Cepheid instability strip, as depicted in the figure. That might be a problem with the models of Meynet *et al.*[18] that could be resolved by a different treatment of convection[19]. It is worth noting that Usenko *et al.*[3] found excellent agreement of the parameters for V1726 Cyg, albeit for a slightly larger evolutionary age, with the evolutionary tracks of Schaller *et al.*[20], which are the basis for the Meynet *et al.*[18] isochrones.

The location of B-type cluster stars in the CMD of Fig. 3 is consistent with their known properties. The second brightest cluster member in the numbering scheme of Turner *et al.*[2] is star 1, which is identified as an Ap star of the HgMn group[3] as well as a shell star. Conceivably it is a rapid rotator viewed pole on, if its low value[3] of $v \sin i$ and displacement from the evolved main sequence in the CMD is an indicator[21]. The third and fourth brightest cluster members, stars 5 and 111, clump together near $V_0 \simeq 11 \cdot 0$ in Fig. 3. Both exhibit the spectroscopic indicators of moderate rotation[2,3], but are not displaced significantly from the evolved cluster main sequence. The fifth-brightest cluster member, star 10, is also displaced significantly from the main

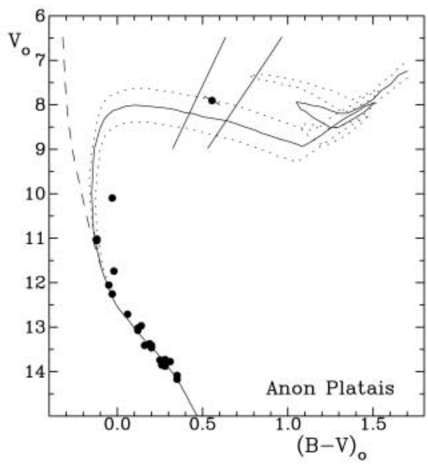


FIG. 3

A reddening-corrected colour–magnitude diagram for high-probability members of Anon Platais, with theoretical isochrones superimposed for ages of $\log t = 8 \cdot 0 \pm 0 \cdot 1$ (solid curve and dotted curves, respectively). The dashed line represents the zero-age main sequence (ZAMS), and lines denote the observationally-defined boundaries for the Cepheid instability strip. V1726 Cyg is the brightest cluster member, and its colour and brightness variations are depicted by short lines.

sequence in Fig. 3, which may again indicate a rapidly rotating star viewed nearly pole-on[21].

Although a sinusoidal light curve is sometimes taken to be the signature of an overtone pulsator, as noted for the type 'c' RR Lyrae variables[22], that does not appear to be the case for V1726 Cyg. Fundamental-mode pulsation is also considered likely in Polaris[23], another sinusoidal Cepheid of small amplitude in a sparse star cluster, while SZ Tau, which is a comparable object in the cluster NGC 1647, is considered to be a likely overtone pulsator[24]. Conceivably, light-curve shape is not the clear diagnostic of pulsation mode that it is argued to be[14], although light amplitude is also argued to be a factor[15]. It would be interesting to examine other sinusoidal Cepheids associated with open clusters to test whether or not their derived parameters are consistent with fundamental-mode or overtone pulsation.

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# CORRESPONDENCE

*To the Editors of 'The Observatory'*

## *Sir Fred Hoyle*

The reviews[1,2] of Simon Mitton's biography of Fred Hoyle and of the one-day conference 'The scientific legacy of Fred Hoyle' have prompted me to put on paper reminiscences of my erstwhile supervisor and subsequent colleague.

Fred was certainly a complex, not to say a highly emotional character. He could almost have been an example for David Hume's dictum: "Reason is, and should be, the slave of the passions". This shows up in his often over-polemical style, and his impatience with a more tentative presentation. For most of us, the feeling that we are taking a significant step towards the ultimate resolution of an important astrophysical problem is sufficient motivation to justify the investment of intellectual and emotional energy. But for Fred, I suspect that this was not enough: he needed to feel that in whatever he was working on at the moment, he was producing at least a close approximation to the definitive answer.

Professor Peacock comments[1] on his persistent advocacy of some version of the Steady State concept, against the inexorable accumulation of contrary evidence. I hazard the guess that, as with my late friend Dennis Sciama, there was an underlying fear of transience; if we as human beings, and equally, individual cosmical bodies — planets, stars, galaxies — must ultimately die, let us be consoled by the thought of an eternally existing Universe with a built-in rejuvenator: continuous creation. A very different reaction from Eddington's serene acceptance of an evolving Universe: "It seems rather silly to keep on doing the same thing over and over again".

Earlier in his career, Fred showed the equally disconcerting, contrary ability to change from one firmly-held position to another, $\pi$ radians out of phase, yet equally firmly-held. An example: shortly after his famous 1950 radio lectures *The Nature of the Universe*, Herbert Dingle reproved him for not making clear that much of the theory he was describing — *e.g.*, on the origin of the Solar System — was personal to him. Dingle cited the electromagnetic model put forward by Hannes Alfvén. In a letter to the *Listener*, Fred stated that Alfvén's ideas could be dismissed by "a straightforward application of the Maxwell stress tensor". In fact, the Maxwell stresses exerted by a rotationally sheared magnetic field can play a crucial rôle in cosmogony through their transfer of angular momentum; and one of the first to recognize this was Fred himself, who built it in to a later model (which I personally found more convincing than Alfvén's).

Of course one admired both his flexibility of mind and his fecundity of ideas, but neither would have been inconsistent with a more moderate advocacy of both the earlier and later models. Perhaps this was more of a problem for other professional astronomers, and especially for physicists working in areas with less scope for a creative imagination, some of whom were led to dismiss astrophysics as just unfounded speculation. However, in his lay audience, including many future astronomers, Fred's popular writings excited enthusiastic admiration for both content and style. As Walter Baade said to me: "If he had written: 'On the one hand the evidence supports this theory, but on the other hand there is this and that difficulty...', the reader would have thrown the book against the wall!"

In his really outstanding work, *e.g.*, on nucleogenesis, and on stellar structure and evolution, he combined a highly original, broad-brush approach with a punctilious concern for accuracy in detail. But he could also be slapdash, and was certainly not afraid to 'think aloud', sometimes in successive publications separated just by months. But where he was wrong, he was often wrong in an interesting and stimulating way. And as for things like the paper on archaeopteryx, surely Fred, like Homer, is allowed to nod.

In Martin Rees's words, in his golden years he "injected more good ideas into the field of astronomy and cosmology than anyone else". In future histories of astronomy, I expect his name to go down with Karl Schwarzschild and Eddington as one of the towering pioneering figures.

Yours faithfully,
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REVIEWS

**The Solar-B Mission and the Forefront of Solar Physics** (ASP Conference Series, Vol. 325) edited by T. Sakurai & T. Sekii (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 448, 23·5 × 15·5 cm. Price $77 (about £44) (hardbound; ISBN 1 583 81187 7).

*Solar-B* is a joint Japanese–UK–US mission, due for launch in 2006, which will use a combination of optical, EUV, and X-ray instrumentation to study the interaction between the Sun's magnetic field and its corona. In particular, the EUV imaging spectrometer (*EIS*) is being built by a consortium led by MSSL with Louise Harra as PI.

The mission will build on the enormous success of the recent and current missions, namely, *Yohkoh*, *SoHO*, *TRACE*, and *RHESSI*, which have together revolutionized our understanding of the Sun and have promoted solar physics as one of the most exciting and vibrant branches of astronomy today. Indeed, it will play an important rôle in continuing the sense of excitement about solar physics and will hopefully answer some of the key questions about the corona and its subtle linkage to the solar surface that have recently been raised.

This book is the proceedings of the 5th Solar-B Science Meeting held in Tokyo in 2003 November. It was attended by many of the world's experts and describes in detail the key puzzling aspects of solar activity, including convection and subsurface magnetic fields, the surface manifestations of magnetic fields, the MHD of the solar atmosphere, coronal loops, coronal heating mechanisms, solar flares and magnetic reconnection, and finally large-scale coronal dynamics. It will act as an ideal preparation for the imminent launch of *Solar-B* and provides an invaluable review for those wanting to understand the most recent advances in our understanding of the solar corona and its relevance for the rest of the Universe. — ERIC PRIEST.

**The Light-Time Effect in Astrophysics, Causes and Cures of the O–C Diagram** (ASP Conference Series, Vol. 335), edited by C. Sterken (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 370, 23·5 × 16 cm. Price \$77 (about £42) + \$20 airmail shipping (hardbound; ISBN 1 538 31200 8).

This volume, number 335 in the rapidly growing ASP Conference Series, contains the proceedings of a workshop held in Brussels, Belgium, in 2004 July. The workshop was the brainchild of Christiaan Sterken, who not only chaired the SOC and edited the proceedings, but himself presented papers that fill no less than fifteen per cent of the pages of the volume. The remaining five members of the SOC were a broadly representative group, despite their modest numbers, and all but one of them actually attended the meeting. The listed participants number 46, although one appears in the list twice. The participants were predominantly European, with only ten coming from places more distant than the eastern limits of that continent, Russia and Turkey.

The preface states that the format of the meeting followed tradition in having a few invited papers, plus some oral contributed ones and some posters. It is not easy to deduce which of the published papers fall into each of those categories, however, since no indication is given in the table of contents, nor on the title page of each paper. Moreover, the distribution of the lengths of the papers is quite continuous and smooth, showing no clear demarcations in length that would distinguish between categories, save for a very few exceptionally long papers. As sometimes happens with such proceedings, several (in this case, four) papers are included, none of whose authors appear to have attended the meeting.

The subject matter covered in the papers is quite diverse, and much of it is unrelated to light-time effects. The subtitle perhaps better reflects the topics discussed than does the main title, because nearly every paper has some relevance to, or makes some use of, O–C diagrams. The volume brings home to the reader how widespread is the use of such diagrams, and indeed begins with a lengthy and salutary paper by the editor on their use and misuse. There follows a substantial initial section of fourteen more papers on methods, statistics, and descriptions of major programmes. Of particular interest to this reviewer were simultaneous solutions of astrometry and light-time by I. Ribas, and the use of light-time delays to assign observed pulsation frequencies to one or other component of a binary system, both of whose members vary, in this case $\theta^2$ Tau, by M. Breger.

Next comes a very diverse little group of four papers relevant to the Solar System and exoplanets, including two by the editor plus a lengthy and interesting account of historical eclipses by F. R. Stephenson, the lone British participant. Two papers involving relativity follow, one on gravitational lenses and the Hubble constant and the other on binary pulsars, the latter again by the editor, demonstrating the remarkable breadth of his interests and expertise. Finally there is a long section including many short papers, which one may surmise were posters, mainly on individual binary or variable stars or on small groups of them.

No small workshop can hope to cover the whole of its field, but this one does a very creditable job, not the least part of which is to demonstrate the widespread use of its principal technique. And no volume, no matter how carefully edited, can hope to eliminate all minor errors and inconsistencies, typographical ones included, and this one is no exception. But it fills a very useful niche in the broad sweep of astronomical research now encompassed by the ASP Conference Series. — COLIN SCARFE.

**Looking for Life, Searching the Solar System**, by P. Clancy, A. Brack & G. Horneck (Cambridge University Press), 2005. Pp. 352, 23·5 × 15·5 cm. Price £25/$40 (hardbound; ISBN 0 521 82450 8).

The last decade has seen growth and acceptance of the science of astrobiology, the study of the origin and evolution of life and its potential occurrence throughout the Solar System and beyond. Concomitant with that growth has been the publication of texts on astrobiology, some aimed at specialists, others at a more general readership. The theme and structure of many of these books has now become familiar: to examine life on Earth, then Mars, then beyond Mars to planets beyond the Solar System. When asked to review this book I was somewhat reluctant because I felt that the market for the subject might be becoming saturated. However, when I read the book, I was very pleased to find that it tackled the subject from a different angle, giving a new perspective on the material, and hence is a valuable addition to the astrobiology canon.

The premise from which the book starts is that exploration is a human imperative. It opens with a description of some of the great voyages of human exploration of Earth: Columbus, Magellan, Cook, Darwin, Amundsen, Scott, Shackleton, and the first astronauts. The authors use experiences from these voyages to highlight the hardships and stresses that humans are prepared to suffer in order to explore, and the ingenuity and perseverance shown that can overcome the problems of adverse circumstance. All this is taken as a sign that humanity is destined to explore — and that the next set of discovery missions should be looking beyond Earth. We can now see that the theme running through the text is that human exploration of space is a necessity. The chapters following the introduction are all slanted toward this idea.

Part II, 'How can we know life?', is broken into several chapters, covering the molecular origins of life, the limits to life, and life's signature. It starts with a clear explanation of the chemistry of cells, and goes on to discuss the steps that take us from chemistry to biology. The influence of chemistry on biology continues with a description of how the physical environment (temperature, pH, salinity, *etc.*) sets boundaries to the ability of micro-organisms to survive. This leads, quite naturally, into a chapter on the potential transfer of life between planets, and the ability of bacteria to survive the radiation doses of interplanetary space, before returning to how we might recognize the signature of life either in the most ancient rocks on Earth, or on other planets.

So far, the book has not diverged much from the track followed by other astrobiology texts. But Part III, 'Life in the search for life beyond Earth', takes us into territory less frequently explored in 'origin of life' works. Chapters on human exploration of space, problems of human spaceflight, and interplanetary ethics are all clear indicators of the authors' feelings that human exploration is required before we can expect to make a full inventory of any solar system. The final parts of the book ('The cosmic biological imperative' and 'Our cosmic destiny') include discussions of the range of technologies that will be required to enable human space exploration, and how human missions to Mars and beyond might be structured.

Overall, the book is an informative and well-written account of astrobiology from the perspective of a contribution from human exploration of the Solar System. Although I am less sanguine than the authors about the timescale for such a leap in humanity's technological ability, the subject is certainly worthy of the thorough treatment it is given here. — MONICA GRADY.

**Worlds on Fire: Volcanoes on the Earth, the Moon, Mars, Venus and Io**, by C. Frankel (Cambridge University Press), 2005. Pp. 358, 23·5 × 16 cm. Price £25/$40 (hardbound; ISBN 0 521 80393 4).

Charles Frankel's book managed to both delight and irritate me at the same time. It describes volcanism on five planetary bodies: Earth, the Moon, Mars, Venus, and Io and is arranged with two chapters per planetary body. The first chapter describes the types and styles of volcanism on that particular body and the second of the pair gives a 'Cook's Tour' of a number of volcanic sites of that body: geography, geology, and human interest. The chapters on the Earth also describe the effect of volcanism on climate as well as on the evolution of life and mass extinction, and the tour guide was so fascinating it made me want to pack my rucksack and set off immediately!

The lunar chapters give a fascinating synopsis of the development of knowledge of volcanism on the Moon from the 17th Century onwards, and the realization that the Moon had in fact been volcanically active, together with a description of models for the formation of the Moon. The words conjure up amazing mental pictures, *e.g.*, the *Apollo 15* astronauts collecting spades-full of emerald-green soil from the Mare Imbrium. The itinerary takes in Apollo landing sites, the Marias Hills, Gruithuisen domes, and the Aristarchus plateau, amongst others.

The Mars chapter disappoints, in that there is no colour in the images presented, *e.g.*, the topographic map is in greyscale, whereas it looks much more imposing when presented in colour. In 57 pages the author covers the history of observation of Mars with telescopes, remote sensing, and eventually the landers. He introduces the major volcanic edifices and the mineralogy of Martian volcanics, including the possibility that there are andesitic basalts there. Life on Mars is discussed as well as plate tectonics and the effect of the volcanicity on the Martian climate.

The second Martian chapter begins to irritate in that Frankel attempts to invent methods of getting over the logistical problems posed by such things as the sizes of the volcanic edifices and the depth of the graven, *e.g.*, surrounding Alba Patera. He whimsically describes using Mars vehicles, balloons, or rocket hoppers. These detracted from the descriptions of the itinerary for me. It may have been better just to describe the constraints of the tour.

He again provides wonderful pen portraits of Venus: "a planet with a runaway greenhouse effect, the world whose atmospheric temperature is so high that its rocks glow red in the dark". He poses the mystery of sulphur dioxide in the high atmosphere: logically it should come from the volcanic eruptions, but the atmospheric pressure is so high that it should be prevented from reaching such levels. However, the itinerary is again disrupted by postulations about the type of craft needed to undertake this trip. I could go for the pressurized diving bell, but the thought of taking an oscillatory balloon trip up and down through the atmosphere, in order not to be affected by the high temperatures, simply left me unable to read the words for worrying about whether I would want to risk the high temperatures affecting the mechanics of the balloon; the thought of cooking in the Venusian atmosphere distracted me from the descriptions of the edifices! It may well be that someone younger and with a more accepting mindset may find these postulations exciting.

The first chapter on Io again fired my imagination. The moment that active volcanism was first discovered on a body other than Earth is described. Frankel explains how scientists changed their minds about the nature of this volcanism from being sulphur systems to ultramafic komiatites; he then puts forward a model for the source of this magma. He also gives a good explanation of what drives the volcanism on Io: namely, the tidal interactions between Jupiter and its Galilean satellites.

I think that this is a book that will be eminently suitable for an adolescent who is interested in planetary science. The stated readership is the layman, but it "explores volcanic processes in sufficient detail to serve as a guidebook for students of the earth sciences". 'Layman' for me equates to an adult who may find the explanations patronizing, *e.g.*, "the earth grows like a deck of cards that is constantly replenished with new cards up the middle of the deck". In other places explanations are placed in brackets, *e.g.*, "hyaloclastics (submarine lava and ash)", which I personally found much easier to read. When he doesn't slip into this type of translation for the layman, the explanations given are very clear, *e.g.*, when describing the effect of the volatile content of a magma on the explosivity of the eruption; he is very capable of writing in a style that does conjure up such visual pictures in the mind of the reader.

There are no references contained within the text, which I found surprising because they would allow the reader to follow things up in more detail. In addition I found the lack of colour pictures within the text disappointing, because there are so many now available, although, realistically, this would increase the cost of publication.

Overall, I think that this book is a superb taster for an adolescent who has an interest in planetary science and I can imagine that it would pull them deeper into the discipline, but some of the descriptions may be a little immature for the average adult layman. I think there is probably insufficient scientific detail for a senior student of planetary sciences. — EVE SCOTT.

**Astroparticle Physics**, by C. Grupen (Springer, Heidelberg), 2005. Pp. 441, 24 × 16 cm. Price £27/$59·95/€34·95 (hardbound; ISBN 3 540 25312 2).

*Astroparticle Physics* is a timely attempt to describe the status of a booming field at the interface of astrophysics, particle physics, and cosmology that has produced so many exciting results over the past few years. The format of the book, with the wide margin for keywords and figures, the extensive glossary, and the problems for each section (with detailed solutions), make it very readable and provide a good starting point for undergraduate and postgraduate students interested in astroparticle physics.

The first few chapters give a concise introduction to the historical development of the field, the basics of the standard model of particle physics, kinematics and cross-sections, and the physics of detectors. The wide scope of the book naturally allows only superficial coverage of subjects in the later chapters, which often necessitates consolidation with more detailed texts. Unfortunately, the more recent developments in the field remain rather qualitative and short.

A major shortcoming of the book is the numerous little inaccuracies and omissions throughout the chapters (some are even grossly misleading), which spoil the overall good impression. They range from the short-term variability of TeV gamma sources (p. 119), over the definition of the GZK cut-off (p. 59) and the explanation of Fermi acceleration (p. 68), to the formation of heavy elements in SN explosions (p. 66), just to name a few. Therefore the book should be read critically. — JOHANNES KNAPP.

**A Different Universe: Reinventing Physics from the Bottom Down**, by R. B. Laughlin (Basic Books, London), 2005. Pp. 304, 24 × 16 cm. Price £15·50 (hardbound; ISBN 0 465 03828 X).

It's a long time since I read a book that sent me scurrying quite so quickly and often to old university textbooks or more fashionable Google searches to check up on dimly-remembered facts or concepts. That response alone exemplifies the book's main virtue to me; it is nothing if not supremely stimulating and thought-provoking.

In these days of database searches, it does seem strange, though, that the theme of the book should be hidden behind a rather obscure title. The book is in fact entirely concerned with promoting the concept and understanding of emergent phenomena in the physical world — a subject surely worth showing up in book-title searches since this particular volume claims to show us no less than "why everything we think about fundamental physical laws needs to change". Hardly an insignificant claim, but Laughlin is clearly on a mission.

What is 'emergence' and why is it important? One early web-search revealed a summary article from the philosophy department of the author's own university[1], which proposes the following definition: "Emergent entities (properties or substances) 'arise' out of more fundamental entities and yet are 'novel' or 'irreducible' with respect to them. (For example, it is sometimes said that consciousness is an emergent property of the brain.)" While the study of emergence in the physical world feels relatively new, we learn in the same article that philosophers have been grappling with emergence for over a century now and we are well aware of the current author's recent interest in these concepts. At the same time, though, there appears to be some (philosophical) doubt over whether the phenomena that concern him are "not only epistemologically, but also ontologically, emergent." At that point it seemed wise to retreat back to the book and save investigation of that particular distinction for another rainy day.

The rigidity of solids as a result of the regularity of crystal lattices is an often-quoted and accessible example of emergence (or result of higher organizing principles) in physics, although examples also abound in the Universe at large. These properties, it is claimed, will never be deducible from basic principles no matter how basic or detailed a level the reductionist method of physical inquiry eventually reaches. This is not to refute the validity or utility of reductionism in many circumstances, but rather to suggest that it may have some significant and insurmountable limitations.

For many scientists, however (although apparently not solid-state physicists, chemists, or biologists), the adoption of the emergence concept is a solidly anti-reductionist stance and is a "dangerous and ludicrous" idea tantamount to a pusillanimous acceptance of natural phenomena which may, as yet, be unexplained or not understood, but whose time will surely come. Laughlin eloquently and vociferously disagrees, which raises the question of whether it is possible, or indeed necessary, to disentangle the message from the messenger. Other reviews of this book[2,3], from what one might call 'physics insiders', have hinted at possible personal and political agendas, which might be colouring the physics storyline. Be that as it may, from an outsider's viewpoint, any such shenanigans can only add spice to the telling of an already tasty story.

Enthusiastic as I am over the package as a whole, there are some quibbles with the content and the style. The author and his professional community no doubt view this as a low-level introduction, but much of the discussion assumes a high level of familiarity with solid-state physics and all too often the text references monographs or professional journals for further information or clarification — not much use for the lay reader or for those remote from specialist libraries. The inclusion of much more background information on particularly important or esoteric physics topics would have been appreciated. Although probably symptomatic of the author's personality and intellect, I was stumped by some of his one- or two-line quips, which all too often left me wondering whether they were meant to be ringing endorsements or, on the contrary, ironic dismissals of the previous discussion.

Since the reductionist/emergence debate seems set to be increasingly topical, these factors suggest there is still a market for the emergence of a slightly broader-appeal version of this text. In the meantime, for anyone wanting a quick taster (of style as well as content), an article[4] by the author and his colleague David Pines is a good start. If that intrigues, go search for the book! — C. D. PIKE.

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**Extra-Planar Gas** (ASP Conference Series, Vol. 331), edited by R. Braun (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 404, $23 \cdot 5 \times 16$ cm. Price \$77 (about £40) + \$20 airmail shipping (hardbound; ISBN 1 583 81194 X).

This volume includes the contributions to a lively meeting devoted to the gas in the immediate vicinity of galaxies. There has been an on-going debate as to the origin of and physical conditions in the high-velocity clouds (HVC) since the time of their discovery some 40 years ago. In recent years a large body of new data at many wavelengths has been added to the original H I observations, which significantly broadens the picture available of the local environment of normal galaxies. It is now evident that a number of physical processes are responsible. The meeting brought together researchers in the diverse observational and theoretical areas which illuminate this subject.

One component of the extra-planar gas is thought to be the result of the continuing process of galaxy evolution, where gas is falling into galaxies from tidal disruption of neighbouring dwarfs or from the residual cosmic flow. Ultraviolet observations show that there is clear evidence of the impact of the H I gas with the hot oxygen halo. Weak gaseous haloes around normal galaxies are not uncommon and are detected in the absorption spectra of background quasars. The argument for an origin of at least some of the HVC gas from outside the parent galaxies carries some weight. The alternative scenario in which much of the extra-planar gas originates within the parent galaxy has a strong pedigree. Clearly on-going star formation in late-type galaxies can generate superwinds which eject hot gas from the galactic discs and produce X-ray halos. Similarly outflows are evident in radio galaxies. When one adds the gentler 'fountains', there are adequate processes to generate outflows.

This book captures the excitement of the debate emerging from the rich source of new data becoming available on the nearby environment of galaxies. A study of it will reward any graduate student or experienced researcher with an interest in this evolving subject. — R. D. DAVIES.

**Marswalk One: First Steps on a New Planet**, by D. J. Shayler, A. Salmon & M. D. Shayler (Springer, Heidelberg), 2005. Pp. 244, 24 × 17 cm. Price £18·95/$29·95/€24·95 (paperback; ISBN 1 852 33792 3).

The first human footsteps on Mars now seem almost possible within our lifetimes, following President Bush's announcement of a new era of American space exploration to the Moon and ultimately Mars, and the recent European commitment to the Aurora Martian-exploration programme. The enormous complexity of the missions envisaged is explained in *Marswalk One*, which addresses many of the engineering, operational, and practical issues a Martian-exploration team would have to consider. Many technical requirements will be, necessarily, developed from the Apollo missions, but the additional challenges beyond lunar exploration form a major part of this book. The book's introductory material covers previous unmanned missions to Mars, and early proposals for manned space flight. Someone suggested to me that, until very recently, the time to a manned Mars mission is always $(n+30)$ or so, where $n$ is the current year. Looking back, some of the early proposals did seem unduly optimistic; but we are still at roughly the same stage with the Aurora programme!

The technology that would be needed to get humans to Mars, and what they might do when they arrive, is addressed in detail. The sartorial requirements of visitors to Mars are emphasized. Interesting comparisons are drawn between the mechanics of Martian space suits, lunar-exploration suits, 'conventional' space suits for extra-vehicular activity (EVA; incidentally I could not find a definition for this acronym in the text and had to look it up elsewhere), and pressurized attire for use on Earth, particularly for under the oceans. In general, more attention is paid to operational and technological aspects, for example, potential routes to explore the areas around the spacecraft's landing site, rather than to the science to be carried out by the human explorers. It seems a pragmatic choice to cover the practical aspects of humans getting to Mars and how to keep them safe and well first, before the scientific detail. Where the science is discussed, there are strong analogies made with lunar exploration, as the Moon is of course the only extraterrestrial object to have been visited by humans. For example, surface-science exploration would follow on from the *Apollo Lunar Surface Experiment Package*. The measurements made on this package and a proposed 'Mars Surface Experiment Package' are compared, and complex aspects of the science to be carried out by humans on Mars discussed.

The unique demands of the years away from Earth that would be placed on the individuals, and the specific advantage of human, rather than robotic, Martian exploration are also covered. I smiled wryly at a breath of the 19th Century amidst the very 21st-Century subject matter when it was suggested that "bachelor and spinster" astronauts might be more suitable than married ones! (Social issues aside, I hope that this anachronistic and unflattering wording will be removed in a future edition; its usage is, at best, quaint.) Some problems are completely new — what would be the psychological effect of seeing Earth in the sky as a bright light indistinguishable from Venus, for example?

Scientifically and technically the book appears to be accurate (with a couple of minor errors, *e.g.*, HPa as the abbreviation for hectopascal instead of hPa), though I became irritated by the lack of cross-referencing between chapters. Instead of cross-referencing, certain points are repeated at length, which gives the impression that different authors' contributions may have lacked adequate synthesis at the final stages.

Given that the book's central acceptance of the need to send people to Mars is by no means widespread (several countries, including the UK, have refused to commit to the manned aspects of the Aurora programme), the question of motivation is dealt with somewhat clumsily. In the preface, the authors imply that the need for human exploration of Mars is an *a-priori* assumption for the human race, which seems entirely reasonable for this sort of practical guide. Yet later on, when the scientific objectives of a visit to the Martian surface come up, the possibility that humans might not actually be needed is broached. This issue, surely an integral part of any discussion of human Martian exploration, is only covered obliquely despite the contradiction of an explicit statement, "we still need firm justification for a journey to Mars". Whilst this is my major criticism, *Marswalk One* is still a detailed, thorough, and apposite account of every aspect of human Martian exploration, from a technological and engineering perspective. — KAREN APLIN.

**The Observation and Analysis of Stellar Photospheres, 3rd Edition**, by D. F. Gray (Cambridge University Press), 2005. Pp. 533, 25·5 × 18 cm. Price £55/$80 (hardbound; ISBN 0 521 85186 6).

The first edition of Gray was published as long ago as 1975, and it's natural to ask if a reference book with such ancient antecedents, in only its third edition in 30 years, can remain useful, relevant to modern astrophysics, and competitive in a burgeoning publications market. In this case my unhesitating answer to that question is "yes"; the second edition of *Stellar Photospheres* (published 1991) is one of the more heavily thumbed volumes on my shelves, and this new edition sustains the strengths of its predecessors while introducing new material, including (for the first time) exercises that enhance its usefulness as source material for lecture courses.

What underpins this continuing utility is the authority of the author, whose personal interests and expertise are reflected in the contents of the book: 'observation' should be read to mean primarily 'optical spectroscopic observation', while 'analysis' is substantially 'LTE model-atmosphere analysis'. His extensive research experience further biasses the content even within that framework; much more attention is paid to working in Fourier space than is normal, gratings are discussed in terms of real-world issues and not just idealized formulae, and, unusually, macroscopic line broadening is discussed at length. Lest there be any ambiguity, let me say that this is definitely a *good thing*; all the expected standard topics are well covered, with a clarity that readily betrays the author's deep familiarity with, and understanding of, both the central physics and the empirical practice. Any atypical content and emphasis also reflects this first-hand command of the material, and sets the book apart from (and above) a number of others that attempt to cover broadly similar ground — although, in truth, very few books aspire to cover both the trials of data acquisition *and* the theory and practice of the analysis of stellar spectra in enough detail that one could get up and running in either area without (much) recourse to other sources.

*Stellar Photospheres* addresses spectrographs and detectors, the fundamentals of radiative transfer, the relevant microphysics, and continuum and line formation with a deftness that makes it accessible at undergraduate level, and with enough depth (and referencing to primary sources) to make it useful for the research scientist. Recommended to all with interests in these areas. — IAN D. HOWARTH.

**Payload and Mission Definition in Space Sciences**, edited by V. Martínez Pillet, A. Aparicio & F. Sánchez (Cambridge University Press), 2005. Pp. 393, 25·5 × 18 cm. Price £75/$130 (hardbound; ISBN 0 521 85802 X).

Question: What does an undergraduate in one of the 'Space Science and Systems'-style courses currently running at the likes of Kent, Southampton, and Leicester, need to know early on in their education if they are to make a career of space science? Answer: They need to understand the relationship between the providers of missions (the Agencies) and the true consumers of scientific opportunities (the space-science institutes). They need to know, once an outline mission has been described, how the instrument complement is assembled, how the operations are determined, how much freedom is available to change and adapt, and how a system is developed.

This book gives a great deal of this information in a surprisingly digestible format. At first look, a small number of extended articles with these titles do not seem to provide a coherent and complete whole. Mostly because the authors represent an experienced and eminent bunch of space scientists, the coverage is nearly complete despite the limited implied scope of the article titles.

The collection starts, most logically, with an article by Gimenez (ESTEC) describing the ESA approach to mission development. This is most welcome for a number of reasons at undergraduate level — not least because it demonstrates that the Agency has a well-crafted approach to mission programmatics, which to my mind is superior to that of NASA. Young scientists need to know this when weighing European opportunities against the lure of the apparently better-funded US scene. Langevin follows, from a very experienced perspective, with a detailed exposition of mission planning that should not be missed by budding planetary scientists. It is so important that instrument proposers understand these principles, in order that mission durations do not come as a surprise and that cruise-science opportunities be maximized.

X-ray and UV astronomy are mature disciplines with several successful space-borne observatories. They are well expounded in two articles by Barcons and Harrison, giving both the science and the instrumentation aspects of these two windows on stellar processes. The article by Appourchaux provides a timely focus on the rationale, design, development, operations, and results of a single instrument in solar and heliospheric science. It is in effect a case history for some of the generic aspects treated in previous articles, and the approach to the problems is most informative. A short history of that most esoteric but useful of astronomy disciplines, astrometry, is given by Perryman. Europe has made a major contribution, and the author describes how this lead will be maintained with *Gaia*. The coverage of space-science disciplines is almost complete with Balogh's article on space physics, with an emphasis on fields and particles. Again, a comprehensive *exposé* is given on the science and the slew of technologies that go into the instruments.

Finally, Coradini has written a lengthy piece on planetary missions and instruments.This is where much of the enthusiasm of present-day young scientists lies, and is deserving of a comprehensive review. The balance between remote sensing and *in-situ* measurements is about right, reflecting the tensions in the community as to the cost of surface measurements of limited geographic extent.

Having been impressed by the comprehensive cover of this collection of articles, one is tempted to identify the deficiencies. Astrobiology, gravitational waves, cosmic dust? Nevertheless, the mission aspects and developmental issues are well covered. Whilst science in low Earth orbit (associated with manned spaceflight) receives no mention, I consider that to be a minor omission which does not detract from the book's desirability as a reference work. My principal criticisms are the price and the lack of an index. — TIM STEVENSON.

**Pocket Guide to Stars and Planets**, by Ian Morison & Margaret Penston (New Holland, London), 2005. Pp. 192, 26 × 15 cm. Price £9·99 (paperback; ISBN 1 845 37025 2).

It is unusual for a book on practical amateur astronomy to be written by two professional astronomers, but this one is. Ian Morison is a radio astronomer who helped design the *Merlin* array, while Margaret Penston was for many years at the RGO and worked on the *Hipparcos* results. But both have been presidents of the Society for Popular Astronomy, and have been deeply involved with education and amateur observing.

Their book begins somewhat unusually with some history — but only to show how observations made with small telescopes (by Galileo and Tycho) have changed the face of astronomy, by way of encouragement to get out there and observe. They then go on to describe briefly but succinctly our understanding of what stars and planets actually are, by way of preparation for the main theme of the book, which is observing the sky.

These days, that invariably means writing for a world readership, which leads to complications of its own. There are plenty of ways of dividing up the sky, and beginners really do need as much help as they can get. The approach taken by Morison & Penston is straightforward but effective: for each season and hemisphere, give a map of the key constellations showing how you can hop from one to another. This is the way most people learn the sky, and it works well. The maps are in general clear and accurate, though with some oddities when it comes to the main non-stellar objects, which are shown very poorly. Furthermore, none of the maps in the book delineate the Milky Way other than by the galactic equator — and though one might argue that so few people can see it anyway, it would not have been too difficult to add it to the all-sky charts.

The book ends up with one of its strongest features — what the authors call the 'Astronomical A-List', which features the 50 top objects that the amateur can observe. This is a good number, and the objects are well chosen to be visible with the naked eye, binoculars, or a small telescope. The finder charts are not always ideal, though in the case of a comparatively difficult object such as M87 there are some helpful hints in the text, which are clearly written by someone who has faced the problem of finding a small and faint galaxy with limited aperture. The book suffers from some sloppy editing — wrong captions, poor or wrong labelling, and some clumsy artwork. On the Moon, for example, we have Sinus Iridium for Sinus Iridum, and on some star maps we have Formalhaut for Fomalhaut. The Coalsack is apparently a brilliant white blob though its label is hovering near Alpha Centauri. The picture on page 159 that claims to be of Sagittarius is in fact a close-up of the area round NGC 6231 in Scorpius.

Overall, however, the book will help any beginner find their way around the sky, and I hope that the Astronomical A-List will become a popular source of observing fun. — ROBIN SCAGELL.

**Revolutionaries of the Cosmos: The Astro-Physicists**, by I. S. Glass (Oxford University Press), 2005. Pp. 317, 24 × 16 cm. Price £35 (hardbound; ISBN 0 198 57099 6).

According to the dust jacket, "Galileo, Newton, Herschel, Huggins, Hale, Eddington, Shapley, and Hubble: these astronomers applied ideas drawn from physics to astronomy and made dramatic changes to the world-pictures they inherited." I suppose that British astronomy should be proud that seven of the eight were English-speaking and I further suppose that a continental author might have drawn up a rather different list. Nonetheless, they certainly all played major rôles in the advance of astronomy and all are worthy of examination as scientists and as people.

Ian Glass, in a well-produced volume (but with a few trivial editorial lapses scattered throughout), has given us potted histories of these "revolutionaries", drawn in the main from fully cited biographies, with sidelights from an assortment of other references, and coloured in with his interpretations of their characters. Many quotes are included from the "astro-physicists" themselves, their contemporaries, and other commentators, giving the stories a 'full flavour'. Thus the book stands as a good introduction to those eight key figures together with their contributions to astronomy, and readers whose appetites are whetted can progress further from this baseline *via* the bibliographies given at the end of each chapter.

I had previously acquainted myself with some of our heroes through the works of West, Osterbrock, Christianson, *et al.*, but it was enjoyable getting to know Herschel and Huggins for the first time. While the former appears to have been a pleasant and well-balanced character, Huggins and practically all the others discussed by Glass seem to have been prone to *prima-donna*-ish attitudes or other less-becoming personal traits. We are familiar with Newton's contemptible treatment of Hooke, Hale's bouts of depression, and perhaps of Hubble's 'Walter-Mitty'-like embellishment of his war-time and other activities, but I was not aware of Huggins' craving for scientific fame (p. 129), for example. However, we have no good reason to suppose that scientists are any less afflicted by personality failings than the rest of the population.

Ian Glass has produced eight nicely balanced stories, each with enough science to make them worthwhile reading for students entering astronomy and wanting some historical background, while at the same time being useful starting points for those with a more direct interest in the history of astronomy. — DAVID STICKLAND.

**The Fate of the Most Massive Stars** (ASP Conference Series, Vol. 332), edited by R. M. Humphreys & K. Z. Stanek (Astronomical Society of the Pacific, San Francisco), 2005. Pp. 423, 23·5 × 15·5 cm. Price $77 (about £44) (hardbound; ISBN 1 583 81195 8).

The content of this book is perhaps not what one might immediately suppose from the title (*i.e.*, evolution through to supernovae and their subsequent end products); if its content reflected nature proportionately, then the fate of the most massive stars would overwhelmingly be to become $\eta$ Carinæ!

Kris Davidson provides some justification for this heavy bias towards one star by asserting that "in terms of significance and breadth of work to be done, *it amounts to a topic* [author's italics], functionally equivalent to a class of objects". Well, there may be just a smidgen of hyperbole in that (perhaps excusable from

'Dr. Carinae'), but it's certainly true that this one star offers an exceptional wealth of observational features that challenge physical interpretation; *e.g.*, spatially resolved optical spectroscopy of the bipolar homunculus effectively encodes latitudinal variation of conditions in the stellar wind, and long-term monitoring reveals a 5·5-year period in just about all datasets, from X-ray to radio régimes. However, although these extraordinarily detailed observational studies have prompted a good deal of new physical interpretation, it's disconcerting that, while an eccentric orbit is generally (though not universally) assumed, the fact is that there's simply no really satisfactory model even for that 5·5-year period; life would surely be a lot easier if there were fewer data, of poorer quality!

Perhaps the most interesting new theoretical insight motivated by η Car is the rapidly developing understanding of mass loss near and above the Eddington limit, where Spiegel, Shaviv, Owocki, and others have introduced a new paradigm of laterally inhomogeneous winds that are porous to radiation. This picture seems to be consistent with the remarkable inference that the mechanical energy associated with the outflow in the 'great' and 'lesser' eruptions of the 1840s and '90s was comparable to the radiative energy output.

Remaining 'most massive star' topics, including core-collapse supernovae, other extreme objects, and general theoretical considerations (massive-star evolution and instabilities) occupy around 20% or so of the book. Although the scientific content is consistently interesting and reasonably varied, the level of editorial control is disappointing. It's perhaps unreasonable to expect editors of proceedings volumes to take the trouble to intervene at the level of individual contributions (so there's the usual scattering of broken English, stylistic inconsistencies, and LaTeX abuse), but the frequency of typographical errors and general carelessness in areas of direct editorial responsibility (*e.g.*, the post-presentation discussions, and even the front-cover title) is exceptionally intrusive — high enough to be irritating and, in several cases, downright confusing. It's a shame that this significantly detracts from a useful proceedings of what was evidently a vigorous and very successful meeting, held in the glorious surroundings of Grand Teton National Park. — IAN D. HOWARTH.

**Discovering Astronomy, 5th Edition**, by S. J. Shawl, K. M. Ashman & B. Hufnagel (Kendall/Hunt Publishing, Dubuque, Iowa), 2006. Pp. 744, 27·5 × 21·5 cm. Price $79·95 (about £45) (paperback; ISBN 0 757 51787 0).

Many non-science majors choose an introductory astronomy course as a science credit to meet degree requirements. With this text, students will discover a lot more than just astronomy. They will discover that science is an on-going process subject to constant revision and refinement, and not an isolated subject of study. The importance of this cannot be overstated in today's increasingly technological and complex world, where a little scientific literacy is needed to participate fully in modern society.

The authors' passion, not only for astronomy but also for good science education, runs through every facet of this book. That passion hits with full force in the preface, which I, as an educator, read with great interest. Here is high dedication to teaching the process of science. The preface explains a pedagogical approach that is inquiry-based and engaging. It highlights direct involvement of the student through hands-on activities and carefully conceived, thought-provoking inquiries that get to the heart of the concepts described in the text.

*Discovering Astronomy* is a survey of the subject from its earliest beginnings to today's modern science. It is familiar material updated to include recent discoveries and presented with a fresh approach. The content organization makes sense to a newcomer. It follows the course of the development of astronomy as a science, starting with the Solar System and the development of astronomical techniques, followed by stars, galaxies, the Universe, and finally the possibility of life 'out there'. Early on, the subject of pseudoscience is introduced, explored, and compared to science. This important section is well done and clears the way for fuller development of critical thinking skills that can more easily distinguish between science and pseudoscience.

Most areas of astronomy cross over into most other areas of astronomy, and so the text's content is not rigidly compartmentalized. Instead, the authors have taken an integrated approach so students can grasp concepts within a larger context. This makes it easier for students to see the significance and application of knowledge from one area to other areas of astronomy.

Overall, the organization and layout of pages, chapters, and sections are good, making reading and study easy. Illustrations, charts, and diagrams are carefully designed and clear. The text is well written and flows logically to develop fully its ideas. A number of random text samples of the book yielded an average Gunning-Fog readability index of 14.3, suitable to second-year university students. Several useful appendices and a glossary complete the book. There are a very few minor typographical errors. For example, Charon is referred to as Chiron on page 10, and Figure 12-20 on page 318 refers back to Figure 12-18b instead of the correct Figure 12-17b. The book does not include a multimedia CD. A supplemental-activities manual is available separately. The book is printed on medium-weight matte stock so its 700+ pages are less of a weight to carry and pay for — a worthwhile feature for students. *Discovering Astronomy* offers teachers and students thoroughness, clarity, flexibility in approach, and enthusiasm for the science of astronomy. — MARY LOU WHITEHORNE.

**Deep Sky Objects: The Best and Brightest from Four Decades of Comet Hunting**, by David H. Levy (Prometheus Books, Amherst), 2005. Pp. 362, 21 × 13·5 cm. Price $20 (about £11) (paperback; ISBN 1 591 02361 0).

The reviewer always enjoys reading books written by observers where the enthusiasm for the night sky really shows through and this is certainly true of this publication. David Levy, in his introduction, suggests this book is different from many devoted to deep-sky objects as it is more an *approach* to observing them rather than a mere listing. Nevertheless, the list of 336 objects which form Part 2 and which include many available to those suffering significantly light-polluted skies, will act as an inspiration to go out and observe, even for those just starting out.

His observing techniques rather mirror those of Charles Messier, where deep-sky objects were often found whilst searching for comets and many looked remarkably like those Solar System intruders. The introductory section provides useful information on the different classes of deep-sky objects and their properties. However, this background detail and explanation continues throughout the book as each object's description is followed by notes about how they fit into categories but still, in some cases, have quite unique characteristics.

My only slight concern is the almost inevitable introduction of yet another set of names such as 'Levy 86' for the Andromeda Galaxy, already well known as Messier 31 and NGC 224. Although cross-references are provided, there is always a danger that subsequent observers will merely report the 'new name' to the confusion of those unfamiliar with the current book. Having said that, many beautiful asterisms are included which do not have a previously-catalogued name, such as Levy 69-P for Wendee's Ring, to which is added 'J2204.3 + 4508' to reflect the modern catalogues which tend to use the position on the sky.

The book does not seem to contain any drawings by the author but rather a series of photographs by various collaborators, and the quality of some of these suffers a little from the method of printing. It does, however, include many images of objects rarely seen in other books, such as a series of Tombaugh clusters, after the great discoverer of Pluto. The inclusion of such unusual objects will provide me with an impetus to try and observe these new groups even after years of going through NGC objects.

The book is littered with snippets about astronomy, including the author's visit to Bath and the house where William and Caroline Herschel used to live. With their strong links to cataloguing deep-sky objects in the past, such trips mean a great deal to the modern-day observer.

After the descriptive notes of Part 2, the next section lists all the objects in order of their 'L' number and which the author describes as a 'Catalog of Comet Masqueraders'. Although I have found that some deep-sky objects, especially globular clusters, do genuinely look like comets, there are many in this listing which seem quite different in appearance to my eyes. However, as so many 'false alarm' reports are of deep-sky objects thought to be comets, such listings are certainly worthwhile. The listing is also repeated in RA order. The appendix contains a series of maps of the constellations with deep-sky objects plotted, although the computerized format would perhaps make them difficult to use at the telescope.

I thoroughly enjoyed the book and despite the numerous deep-sky publications which have preceded it, this latest addition is well worth adding to the shelves of the active observer. — GUY M. HURST.

**The Sky at Night 2001–2005**, by Sir Patrick Moore (Phillip's, London), 2005. Pp. 183, 19·5 × 12·5 cm. Price £9·99 (paperback; ISBN 0 540 08808 0).

This is the twelfth book in this series; anyone who has read any of the earlier books will know what to expect and will not be disappointed. There are thirty-seven chapters covering the BBC television programmes from 2001 November to 2005 March and they really do seem to have "something for everybody" as promised in the introduction. The beginning and end of the Universe, the Solar System, stars of many kinds, and equipment for use by both amateurs and professionals all feature along with art, music, and a bit of philosophy.

The book is not completely free from errors but they are very minor. Each chapter stands alone but when reading the book from cover to cover there is sometimes more repetition than would seem necessary. I would also prefer to know with which programmes the listed guests were associated.

Most of the programmes shown now can be obtained on CD but I hope the publication of this series continues. Looking back over past books in the series they all seem to stand the test of time well and there is no reason to suppose that this one will not do the same. Electronic-media standards seem to change rapidly but the 'number one eyeball' should still be valid well into the future. — RITA WHITING.

**Oxford Dictionary of Space Exploration**, edited by E. J. Dasch (Oxford University Press), 2005. Pp. 403, 19·5 × 13 cm. Price £9·99 (paperback; ISBN 0 192 80631 9).

When first this book arrived at the editorial office, I offered it for review to a colleague with some experience in dictionaries, but he shied away pointing out that he'd been a consultant on the project. I was not to have known that, because there is essentially no background information in the book concerning "a team of experts" used in its compilation, nor outlining its aims or scope. Nonetheless, it looks like a useful compendium for the space buff, being reasonably up to date and containing a wealth of information, with appendices valuable for the composition of quizzes!

Although space exploration is the avowed theme, there is a reasonable dose of astronomy included, with generally adequate descriptions of planets, stars, galaxies, *etc*., so the book-cover exhortation to "explore the universe with this fascinating guide" is not totally without foundation (although it certainly wouldn't be the book of choice for that purpose). Most of the *Dictionary*, however, is firmly rooted in the space business, with all manner of jargon explained, including many of the hundreds of abbreviations and acronyms which litter the field. There is some inconsistency here with, for example, AAS being included but not RAS. Similarly, the *Hubble Space Telescope* is referenced both as an acronym and in full, whereas the *International Ultraviolet Explorer* only appears in full. But for the patient browser, these may not prove too much of a drawback. In fact, I have the impression that terms with an origin in the USA, especially connected with NASA programmes, are covered more fully than those originating elsewhere. By way of example, while the NASA facility on Wallops Island gets a mention, the ESA tracking station at Villafranca del Castillo ('Vilspa') doesn't.

A useful feature is the inclusion of a large number of references to websites which may be consulted for more information on a wide range of topics. All in all, you can't go far wrong for a very modest £9·99. — DAVID STICKLAND.

**David Levy's Guide to Variable Stars, 2nd Edition**, by D. H. Levy (Cambridge University Press), 2005. Pp. 262, 24·5 × 17 cm. Price £18·99/$26 (paperback; ISBN 0 521 60860 0).

This is a revised edition of David Levy's *Observing Variable Stars: A Guide for the Beginner*, originally published in 1989. Regrettably, I find that the author has made essentially no corrections, and all the problems that should have been caught by a competent editor remain untouched. I also find that some sections were reproduced, verbatim, with their errors and infelicities in the author's *The Sky: A User's Guide* (CUP, 1991).

Both editions succeed moderately well in introducing amateurs to variable-star observation, but Levy operates on the premise that the field is so complex that sweeping statements and over-simplifications suffice. The discussion of the many types of variables is woefully inadequate. Referring to RR Lyrae and RV Tauri stars under 'Cepheids' (p. 26) is only likely to lead to confusion at a later date. Similarly, the statement (p. 27) that "Some semiregulars defy our attemps [*sic*] to find any trace of periodicity ..." is presumably an oblique reference to the irregulars of the L, LB, and LC classes. Some of the statements about stellar evolution need revision in the light of modern knowledge.

The discussion of the nomenclature of variables is yet another aspect that is wrong. The author would have done well to refer to Argelander's original description. The attribution of the Chaco Canyon images to Anasazi 'observations' of the supernova of 1054 A.D. is now widely discredited. The supernova discoveries by the Rev. Evans between 1981 and 1986 are mentioned, but his 30 discoveries or co-discoveries since then are discounted. Other discoveries, such the 160-odd by the UK's Tom Boles and Mark Armstrong are simply ignored.

There is a short chapter on CCD observing, but even this is likely to confuse readers. Flat fielding, bias frame, and dark frame are just three of the terms used with no explanation whatsoever.

The description of specific topics and techniques (such as averted vision) is confused or inadequate. Julian Days are mentioned on p. 38 but described on p. 46. Although a table gives JDs for day zero of a number of years, it would have been simple to include tables giving day numbers for the first of each month for leap and non-leap years. Elsewhere, Levy says that if observers find JD confusing, they should simply note down clock time. There is no mention of using a digital clock set to JD (or UT).

Although many charts are included, errors and omissions abound. Some variables specifically discussed are not shown anywhere; some comparisons are missing; and although on p. 87 observers are told to estimate W Cyg with the mag. 6·1 and 6·7 stars, its chart is not included. Many charts are utterly inadequate for either finding or making estimates.

I am deeply disappointed in this edition, and feel that readers deserve better. I certainly cannot recommend this or the previous edition as a satisfactory introduction to the subject. — STORM DUNLOP.

**Philip's Pocket Star Atlas**, by J. Cox (Philip's, London), 2005. Pp. 64, 19·5 × 12·5 cm. Price £4·99 (paperback; ISBN 0 540 08792 0).

This little book, first published in 1993, is in its fourth edition and it is easy to see why it has proved so popular. In just 60 profusely illustrated pages it covers the basics of astronomy in a clear, economical style.

A major feature is the star charts, each with generous overlaps, covering the whole sky in surprising detail. Included are stars down to 5th magnitude, naked-eye variables, doubles, and even the colours of some of the brighter stars. Deep-sky objects include open clusters, globular clusters, diffuse nebulae, and galaxies. A surprising omission is the planetary nebulae, so such well-known objects as M27 and M57 are absent. Although perforce on a small scale, the claim that these charts would serve the traveller and more experienced observer is well justified.

Numerous tables give adequate information on the objects to be found on the charts. In addition, other tables on the Solar System describe eclipse and planetary phenomena and give projections until beyond 2025. All this for under a fiver!

My adverse comments are few. The diagram showing the range of lunar risings has some superfluous azimuths indicated. The common problem of confusion between the degree sign and zero has resulted in a greatest elongation of Mercury of 280°, and in the star charts 47 Tuc is so identified in one chart but by its NGC number in another. This, however, is nit-picking. This is a wonderful book for its price and can be heartily recommended. — RICHARD H. CHAMBERS.

**Philip's Stargazing 2006**, by H. Couper & N. Henbest (Philip's, London), 2005. Pp. 64, 23 × 16 cm. Price £6·99 (paperback; ISBN 0 540 08789 0).

This book is one of the range of Philip's small paperbacks giving inexpensive reviews of various aspects of astronomy for the general public. Its aim is to present a guide to the month-by-month changes in the northern night sky, chiefly from a British perspective. Each month is dealt with in four pages. A brief description of the seasonal changes is accompanied by a chart showing the brighter stars, the positions of the Moon and planets, and other relevant objects as seen from UK latitudes. Although adequate for the purpose, the user will probably need to use a larger-scale chart in practice. One prominent constellation is selected each month for a more detailed description. The planets, the Moon, and other phenomena, as appropriate for the time, are briefly described. Special events, such as meteor showers, are given separate treatment and each month has its 'object', its 'picture', and its 'topic' for brief examination. Following the monthly reviews is a four-page description of Solar System objects followed by tables of deep-sky objects arranged by constellation. The final chapter is a brief but useful critical guide on optical equipment.

One problem which the novice will encounter is the use of unfamiliar terminology. Thus 'magnitude' requires a preliminary explanation rather than the inadequate treatment it receives towards the end. The same could be said of other designations such as NGC, M, HD, and the Greek alphabet. Simply, a brief introduction of terms is required.

There appears to be no important event overlooked in this publication, and for those without recourse to other sources of information this will act as a more-than-adequate alternative. — RICHARD H. CHAMBERS.

## *Here and There*

### SUNNY SIDE UP, PRESUMABLY

Altair is an extraordinary star. … it rotates extremely rapidly, … it must have the general shape of a fried egg. — *The Daily Telegraph*, October Night Sky, 2001 September 29, p. 26.

### JUPITER CUT DOWN TO SIZE

…there was great excitement in the Department of Astronomy at the upcoming occultation of the bright star [beta] Scorpii by Jupiter.… For such occultations you have to be in the right place at the right time — the path of the occultation on the ground is narrow. — *JA&A*, **26**, 1191, 2005.

### ONE GOOD BREAK DESERVES ANOTHER

. . . Mars Climate Orbiter did not successfully employ aero-breaking techniques at Mars, but instead crashed into the planet and was lost! — *The Observatory*, **125**, 334, 2005.

### BIG ATOMS OUT THERE

A new short-wavelength radio image of Sgr A* has made it possible [to] establish the intrinsic size of Sgr A* for the first time. It is 1 atomic unit … across. — *Nature*, **438**, No. 7064 (2005 November 3), ix.

### MUST HAVE THE (MAGNETIC) BRAKES ON

… the fastest speed clocked for a neutron star, … at around 1100 km per hour … — *Astronomy & Geophysics*, **46** (2005 October), 5·5.

# 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.

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## NOTES TO CONTRIBUTORS

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