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A REVIEW OF ASTRONOMY

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2006 November 10th at $16^{h} 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome to this monthly A & G Meeting. First of all I have to announce, a little belatedly, the death of Dr. James Van Allen, the US space pioneer and distinguished Professor of Physics at the University of Iowa College of Liberal Arts and Sciences, who died on 2006 August 9. He was famous, of course, for the discovery of the Van Allen radiation belts. In recognition of his contribution to US space research, Van Allen received 13 honorary doctorates, NASA's Medal of Exceptional Achievement, the Commandeur de l'Ordre du Mérite pour la Recherche et l'Invention, and was awarded the Society's Gold Medal in 1978. I'd like to ask you to stand for a brief moment to remember James Van Allen. Thank you very much.

It is now my pleasure to introduce our first speaker, Dr. Roberto Trotta from the University of Oxford and the RAS Norman Lockyer Fellow; he's going to talk about 'Neutrino properties from cosmological observations.'

Dr. R. Trotta. It is a special pleasure for me to be here tonight and present to this audience some of the results of the research I've carried out so far as the Lockyer Fellow of the Royal Astronomical Society. More specifically, I'd like to give an overview of what we have learned about neutrino physics from cosmological observations, and what we can expect from future probes. Neutrinos are an obviously important building block of the Standard Model of particle physics, and knowledge about their properties has made giant leaps since Wolfgang Pauli first proposed their existence in the 1930s. Thanks to observations of solar and atmospheric neutrino oscillations, we now have positive evidence of the fact that they are massive, although the absolute mass scale remains elusive. As we shall see, this is a question where cosmology can be of considerable help.

From the cosmological perspective, we expect the existence of a cold cosmological neutrino background (CNB), analogous to the cosmic microwave background (CMB) that has been so extensively studied in the last decade or so, first by *COBE* and, more recently, by *WMAP* and by other spectacularly precise observations. Whereas the CMB was released at a redshift of 1100, when the Universe was 380 000 years old, the origin of the CNB goes all the way back to the time of neutrino decoupling, when the temperature of the Universe was around 1 MeV and its age about 1 second. The expansion of the Universe then cooled down this relic neutrino background to a temperature that is expected to be 1·94 K. Furthermore, the same quantum fluctuations that gave rise to the temperature anisotropies observed in the CMB ought to have imprinted temperature fluctuations in the distribution of the CNB as well. Needless to say, neither the neutrino background and even less its temperature fluctuations have been directly observed so far (and probably won't be in the foreseeable future), but there are rather convincing indirect indications of the existence — and properties — of both.

With a density of 56 primordial neutrinos per cm^3 per neutrino species, neutrinos are the second most abundant particle in the Universe after photons. Their ubiquitous presence means that they could influence both the expansion history of the Universe on the one hand, and the growth of structures such as galaxies and clusters on the other. These two effects allow cosmologists to study the number of neutrino species and their total mass. In fact, since relativistic particles dominated the expansion of the Universe at early times, the number of neutrino families determined the abundance of the light elements produced through Big Bang nucleosynthesis (BBN). By measuring the relative abundance of deuterium, helium, and other light elements we can infer the number of neutrino species that were relativistic at the time of BBN. Another effect is on the power spectrum of the CMB, whose first acoustic peak is boosted by the early integrated Sachs–Wolfe effect induced by the presence of neutrinos in the early Universe. Therefore, observations of the shape of the CMB power spectrum lead to constraints on the total number of (light) neutrino species, which are in good (if less precise) agreement with the measurement made at CERN of three neutrino families. It is, however, interesting to remark that cosmological observations are able to constrain all sorts of relativistic particles in the early Universe, not just active neutrinos.

A second neutrino property upon which the attention of cosmologists has focussed is their absolute mass scale. As I mentioned, neutrino-oscillation experiments can only determine the mass difference between two families. But a non-zero neutrino mass becomes observable in cosmology through its effect on the growth of cosmological structures: massive neutrinos free-stream out of collapsing structures, thus erasing fluctuations below a characteristic scale that is inversely proportional to the square of the total (combined) neutrino mass. Galaxy-redshift surveys can thus measure the absolute mass scale by mapping out the distribution of the fluctuations (the power spectrum) with scale.

While present limits on neutrino mass do suffer from a certain dependence on which data sets one chooses to combine, we can safely put an upper limit of about 1 eV on the combined mass, which is much better than constraints obtained through laboratory experiments, such as neutrinoless double beta decays. Some rather aggressive analyses of cosmological data even push the 95% limit down to 0·17 eV, which is tantalizingly close to the level of 0·1 eV that one would expect from atmospheric neutrino-oscillation measurement in the case of a so-called 'inverted hierarchy', where the largest mass difference separates the two most massive neutrinos from the lightest one. But even in the case where neutrino masses are ordered in the so-called 'normal hierarchy' — which would be the worst-case scenario and the most difficult one to probe for cosmology — the minimum neutrino mass is expected to be around 0·05 eV, which is within the reach of future large-scale surveys such as the *Square Kilometre Array*, coupled with sophisticated measurements of the gravitational lensing of CMB anisotropies.

With such encouraging prospects for a cosmological measurement of the neutrino mass, attention is moving to even more ambitious targets. I referred to the theoretical expectation that the CNB should carry temperature differences originating from primordial quantum fluctuations. As such, those anisotropies would in principle contain valuable complementary information about the very early Universe, and about the properties of neutrinos themselves, such as their couplings with exotic types of fields. While direct detection of anisotropies in the CNB appears close to impossible, we can nevertheless look for indirect signatures imprinted by temperature fluctuations in the CNB onto the more familiar CMB power spectrum. In an article that appeared last year in *Physical Review Letters*, Alessandro Melchiorri and I set out to look for anisotropies in the CNB by modelling their influence on the CMB spectrum. In fact, CNB anisotropies induce perturbations in the gravitational potentials that are felt by the photons in the CMB. This process leads to a specific signature in the CMB spectrum whose presence in the *WMAP* data we were able to confirm at a confidence level of about 95%. In other words, current CMB measurements exclude with 95% confidence the possibility that the CNB does not contain anisotropies in it, thus beautifully confirming our theoretical expectations. This is only a first step towards studying in further detail the characteristics of the CNB, in the hope that in the future we will be able to extract information about the very first instants in the life of the Universe.

Thanks to the continuous progress in the quantity and quality of cosmological observations, we can thus confidently forecast that the infinitely large scales of cosmology will continue to help clarify the nature of this infinitely small, and still so mysterious, particle — the neutrino.

The President. Questions?

Mr. M. Hepburn. Is the figure of 2 K for the temperature of the cosmic neutrino background purely dependent upon the theory or are there any supporting experiments?

Dr. Trotta. No, we do not have an observation for this; we work out what the temperature is supposed to be by knowing the temperature of the CMB.

Rev. G. Barber. Are the predictions of the future accuracy at determining the neutrino mass also dependent on the cosmological model?

Dr. Trotta. To a certain extent, yes. Of course it's true that when you go to more powerful experiments and observations, you will be able to break lots of degeneracies. I think the numbers here compare to present numbers where we assume the simplest cosmological model.

The President. Can I just ask whether the presence of sterile neutrinos will change any of the predictions?

Dr. Trotta. For the neutrino background you mean? No, I don't think it will impact on it. Also, because we are able to constrain both the background and the anisotropies at the same time, our result for the viscosity parameter is not dependent on it.

The President. Thank you. [Applause.] Our next speaker will tell us what happened in 2006 August when we had great excitement about the definition of planets. Iwan Williams from QMUL will talk about 'The path to defining a planet.'

Professor I. P. Williams. Contemporary observations are changing our understanding of the Solar System, and it is important that our nomenclature for objects reflects our current understanding. This applies, in particular, to the term 'planet'. The word 'planet' originally described 'wanderers' that were known only as moving lights in the sky.

This is not the first time that it has been necessary for the meaning of the term to change. On the Ptolemaic model of our System, there were seven planets — Moon, Mercury, Venus, Sun, Mars, Jupiter, and Saturn — which was good, as seven was regarded as a 'good' number. When this was replaced by the model of Copernicus, the Sun and the Moon ceased to be planets but the Earth became one. The Earth has thus only been a planet for about 500 years! In 1781, Uranus was discovered by Herschel, thus bringing the number of planets back to the magical seven. However, this was not to last either, for in 1801 two additional members of the Solar System were discovered and named Ceres and Pallas, followed in 1807 by two more, Juno and Vesta. In a table on page 416 of a book called *A Treatise in Astronomy* by Sir John Herschel published by Longman in 1833, we find the planets listed in the following order: Mercury, Venus, Earth, Mars, Vesta, Juno, Ceres, Pallas, Jupiter, Saturn, and Uranus. Herschel was not altogether happy with numbers 5–8 being included as planets and had suggested as early as 1802 that as they were asteroidal (*i.e.*, stellar) in appearance, that 'asteroids' might be a better term for them. Even as late as 1845, textbooks were consistently stating that there were 11 planets. The discoveries of Astraea at the end of 1845 together with that of Neptune several months later, after its existence had been predicted because of anomalies in the orbit of Uranus, changed that. Following the lead of the Royal Astronomical Society, most of the world took the term 'minor planet' as a descriptor, while the USA oscillated between 'minor planet' and 'asteroid'. At its formation the IAU adopted the term 'minor planet'.

For a decade or so after this, the individual objects were recognized almost exclusively by means of names and symbols, exactly the same system as used for the historic planets. With the ever-increasing number of minor-planet discoveries, names taxed the memories of all but a hardened few, not to mention the ingenuities of those who designed the symbols. The next 80 years saw a steady increase in the number of minor planets, and the world seemed happy with this classification even though the actual boundary between planet and minor planet had never been defined.

In the early part of the 20th Century, there appeared to be some anomalies in the orbit of Uranus still unresolved and this prompted a continued search for an additional planet. In 1930 Pluto was discovered close to the predicted position on the sky and was immediately hailed as the additional planet. It was assigned a mass consistent with it being the source of the anomalies, that is, several Earth masses, and this incorrect mass continued to be used for several decades. In 1978, Pluto's first moon was discovered, which allowed a good mass determination to be made. This turned out to be less that that of the Moon, or about 0·002 Earth masses.

In 1992, the first of a new group of trans-Neptunian objects, 1992 QB1, was discovered, so Pluto was no longer alone, and in 2004, the object now known as Eris was found to be larger than Pluto. Several other members of the trans-Neptunian region are also comparable in size, though smaller than Pluto. So, something had to change, at the very least including Eris as a planet, thus making ten. But the line between Pluto and the next largest is very fuzzy and it seemed appropriate now to look at the whole question of what is a planet.

There are three broad ways one could go: (*i*) accept that the term is cultural and historical rather than scientific and thus the division is arbitrary; (*ii*) base a definition firmly on the physics of the individual body; (*iii*) base it on dynamics or its rôle within a system of bodies.

In 2004, Division III of the IAU, Planetary Systems Sciences, looked at the problem by setting up a 19-member committee. The IAU, aware of the cultural aspect, requested viable options to be discussed. This committee produced three possibilities, broadly in line with the three described above, namely:

(*a*) (*1*) Planet is a term that currently includes the following nine objects orbiting the Sun: Mercury, Venus, Earth, Mars, Jupiter, Saturn, Uranus, Neptune, and Pluto. All have a gravity-determined equilibrium shape, with a smallest radius greater than 1000 km. Objects with these same properties that orbit the Sun are also planets. (*2*) Smaller objects orbiting the Sun will be classified as dwarf planets when they are demonstrated to have a gravity-determined equilibrium shape.

(*b*) Planetary Object: A planetary object is an object whose mass is great enough for its self gravity to be able to overcome all rigid body forces leading to the object taking up a hydrostatic equilibrium shape. Trans-Neptunian Planets: A Trans-Neptunian Planet is a Planetary Object whose semi-major axis is greater than the semi-major axis of Neptune.

(*c*) A planet moves about the Sun along a path that does not let it approach another planet to within a distance several thousand times larger than its physical radius. It alters, with its gravitational attraction, the motion of nearby planets by a small, but detectable amount. All other bodies that can come close to, or cross, its path have masses at least several thousand times smaller than that of the planet itself.

The IAU executive then set up a committee of six people that came up with a unanimous recommendation, namely: "A planet is a celestial body that (*a*) has sufficient mass for its self-gravity to overcome rigid body forces so that it assumes a hydrostatic equilibrium shape, and (*b*) is in orbit around a star, and is neither a star, nor a satellite of a planet."

It then made the mistake of trying to expand and clarify as follows: "We distinguish between the eight classical planets discovered before 1900, which move in nearly circular orbits close to the ecliptic plane, and other planetary objects in orbit around the Sun. All of these other objects are smaller than Mercury, and constitute a subclass of planets called 'dwarf planets'. We recognize Ceres to be a planet, *viz.*, a dwarf planet, by the above definition. We recognize Pluto to be a planet by the above scientific definition, *i.e.*, a dwarf planet, as are a number of recently discovered large trans-Neptunian objects. In contrast to the classical planets, these objects typically have highly inclined orbits with large eccentricities and orbital periods in excess of 200 years. We designate these objects, of which Pluto is the prototype, as a new class that we call 'plutons'."

This proved very unpopular with the membership, 'plutons' being extremely repugnant, as was highlighting Ceres as a planet once again. It also got involved in a debate as to when a moon became a double planet.

Following numerous discussions at the General Assembly, a much modified proposal was put to the membership and carried. This was:

"(*1*) A 'planet' is a celestial body that is in orbit around the Sun and has sufficient mass for its self-gravity to overcome rigid body forces so that it assumes a hydrostatic equilibrium (nearly round) shape, and has cleared the neighbourhood around its orbit. (*2*) A 'dwarf planet' is a celestial body that is in orbit around the Sun, has sufficient mass for its self-gravity to overcome rigid body forces so that it assumes a hydrostatic equilibrium (nearly round) shape, but has not cleared the neighbourhood around its orbit, and is not a satellite."

An amendment was put (but lost), namely to insert the word 'classical' in front of 'planet' in (*1*) above. If carried, this would have made dwarf-planets into planets and thus be similar to the spirit of the original proposal, but it was not.

So where are we now? Pluto is a dwarf planet and has acquired a minor-planet number, 134340. Eris is also a dwarf planet with number 136199. There will undoubtedly be many more dwarf planets announced in the near future.

The President. Any questions?

Mr. P. Sutherland. Can I just ask if the IAU is likely to revisit this next time round?

Professor Williams. No resolution passed at the IAU is forever. But indeed, if any resolution is proposed in the official way, before the deadline of the IAU, then the IAU is bound to discuss that particular resolution; so any member can put forward any resolution they like, provided they do it in due course, following the rules.

Dr. F. Diego. Yes, just a comment. I think this question is about origins, it is about families of objects and how these objects originated. We have planets, we have Trans-Neptunian Objects (TNOs), we have comets, and they all have defined places in the Solar System. The TNOs have very specific kinds of orbits, which are not tilted in exactly the same plane as the classical planets; they have tilted, eccentric orbits, and they have a different composition as well, mainly water. So, all these things can be put together in a kind of different classification, but it's difficult semantically. How can you tell what is a planet and what is not and find names for these things? But whatever the classical planets are, they are different from the other objects, and there are, I think, three different classes of object.

Professor Williams. I think all the resolutions have it in various forms that there are certainly three different classes of objects, and there may be even more.

Dr. Diego. And one more thing: Pluto was discovered in the United States and that is important; that is why there is controversy.

The President. Well, there is a lot of interest, so we'll take a few more questions.

Dr. C. Trayner. Why did the resolution say "around the Sun", and not "around a star"?

Professor Williams. There is a perfectly acceptable definition of an extra-solar planet (ESP), because the problem is at the high-mass end, and the definition which was already accepted in Sydney by the IAU in 2003 is that a planet is a body which cannot burn deuterium; and therefore, as far as ESPs are concerned, that is all that matters — we are far, far away from discovering extrasolar Plutos. So it was thought best to keep it simple, and not to try to make a definition which might later be contradicted when other ESPs are found. There is no need at the present time for such a definition.

The President. I know everyone would like to talk on this subject, but I'm going to take one more question, which is from David Hughes.

Professor D. W. Hughes. This is just a minor comment about a lovely gentleman in the American Museum in New York, who put on an exhibition of planetary science but only had two rooms to do it in; two rooms have eight walls and therefore there had to be only eight planets, so I think eight is the new magic number that we have to stick to!

Professor Williams. Probably right!

The President. I think on that note we'll stop and thank Iwan very much. [Applause.]

Let me welcome Professor Athena Coustenis from Paris–Meudon, who is going to give us the 2006 Harold Jeffreys Lecture, on the subject 'Titan after the *Cassini–Huygens* mission.' [A summary of Professor Coustenis' talk appears in *Astronomy & Geophysics*, **48**, 2·14, 2007.]

The President. Well, thank you for that wonderful talk about the *Cassini–Huygens* mission and Titan. The image at the end was absolutely unbelievable — there seemed to be a structure there that looked like a sort of theatre.

Professor Athena Coustenis. An ancient Greek theatre.

The President. Yes, it looked like a Greek theatre! [Laughter.] Right, we have time for a few questions.

Dr. L. Dones. An earlier idea was that Titan must be covered with an ocean, but now you say that perhaps a few lakes are enough to supply the methane; so what has changed?

Professor Coustenis. Well, we realized that Titan was more solid than we thought. First, the echoes from radar measurements were more compatible with a solid surface; and then at the time we thought that perhaps the reservoir is underground, beneath the surface — a sub-surface reservoir could be large enough and there could still be interactions with the atmosphere, through pores, for instance. But then, the model description of Titan's haze in particular — which is not perfect still today — managed to convince us that perhaps we don't need such a large amount of liquid on the surface. The fact that we had the haze in the clouds in the atmosphere, and a small expanse of liquids at the surface, would be enough to account for the methane abundances that we have today in the atmosphere. Then, very recently, we have discovered a large number of lakes at Titan's north pole, which could account for the atmospheric methane based on a seasonal cycle.

Dr. G. Q. G. Stanley. From the radar images, can you get an actual height for the dunes?

Professor Coustenis. No, we don't yet have a height measurement. The models tell us that it's very difficult for the dunes to be created by wind as we know it. On Titan we think that it's the influence from Saturn — it's a wind created by the Saturn tides that would make the shape of the dunes, but I don't think we have yet measured the exact height. We're getting there, as also for the bright region at the equator that I showed you, but we still don't yet know what the height is there either. Mountains just south of Titan's equator, at heights of 1·5 km, have also been reported by the *VIMS* team.

Mr. H. Regnart. Are there any views as to the cause and composition of the duned surface areas, and how extensive are they?

Professor Coustenis. They're very extensive. Actually if you saw the movie, there are several locations where they're seen. Their cause could be that in those areas (this is work by Ralph Lorenz and colleagues) you would have mainly a fine-grained ice that is shaped into the dunes that we see; but why at these locations in particular and not elsewhere, I personally don't know.

Mr. Regnart. What I would point out is that the measured wind speed is about 1 m s^{-1}: would that be enough?

Professor Coustenis. Yes, because the ice grains would be very fine — it's ice, not rock — and the atmosphere is dense.

The President. We're going to take one more question.

Ms. Angeliki Kiakotou. You said there were going to be 44 *Cassini* fly-bys up to the end of 2008. In the data that you have now, how many fly-bys have been analysed?

Professor Coustenis. We've had about 20 fly-bys so far. The data we've analysed covers, at least for what I'm doing, about ten or twelve of them, because you have to wait until they're processed and calibrated. I was once still working on data from one *Voyager* fly-by about fifteen years later, and we will have 44 *Cassini* fly-bys in total; so I'm expecting that we're looking at third-generation astronomers from now who are going to be working on the data! So we need young people. Help!

The President. Well, let's thank Athena for a really tremendous talk. [Applause.]

If you were at the last meeting you'll know that the Burlington House apartments are now closed for refurbishment, and they will be closed for about a year while the building is completely transformed for the Society's purposes and for the benefit of members. So we will have much better apartments when the work is finished. In the meantime, one big disadvantage is that we can't offer the drinks party while Burlington House is closed; but Quentin tells me that he will lead a party to some local pub, if you meet in the archway at the entrance to the courtyard. The next monthly meeting will be on Friday, December 8.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2006 December 8th at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. We have a full and very exciting programme. First of all, however, it's my pleasure to announce the 2006 awards of the Royal Astronomical Society. As I have quite a lot of awards to announce please don't applaud each one, as it would take a very long time; perhaps you could applaud at the end to show our appreciation of the recipients of these awards.

The Gold Medal of the Society for Astronomy is awarded to Professor Len Culhane of the Mullard Space Science Laboratory, while the Gold Medal for Geophysics is awarded to Professor Nigel Weiss of the University of Cambridge. The Eddington Medal is awarded to Professor Igor Novikov, Director of the Theoretical Astrophysics Centre, Copenhagen, and the Price Medal is awarded to Professor Andrew Jackson, Swiss Federal Institute of Technology (ETA), Zurich. The Fowler Award for Astronomy goes to Dr. Graham Smith of the University of Birmingham and that for Geophysics is given to Dr. Duncan Mackay of the University of St. Andrews. An Award for Service to Geophysics goes to Professor Aftab Khan of the University of Leicester. Finally, the following are awarded Associateships of the Society: in astronomy ('A'), Dr. Brian Boyle, Director of Australia Telescope National Facility, Virpi Niemela*, Emeritus Professor at La Plata University, Argentina, and Dr. Roberto Terlevich, Instituto Nacional de Astrofisíca Optica y Electronica, Puebla, Mexico. The 'G' Associates

*The Editors regret to announce the death of Professor Niemela; an obituary appears on page 202.

are Dr. Laike Asfaw, Director of the Geophysical and Astronomical Observatory, Addis Ababa, Ethiopia, Professor Marcel Goossens, of the Catholic University, Leuven, Belgium, and Dr. James Klimchuk, from NRL in Washington DC — so can we applaud them? [Applause.]

Our first talk today is by Dr. Kevin Fong from the Centre for Aviation, Space and Extreme Environment, who is going to talk about a topic that has been much in the news recently, 'Human space flight: challenges and opportunities'.

Dr. K. Fong. The challenges associated with human space exploration are numerous but we, as a scientific community, should regard this endeavour as presenting opportunity rather than threat. The history of exploration is nothing if not fraught with risk. Ferdinand Magellan's much celebrated first circumnavigation of the globe left port in 1519 with a fleet of five ships and over four hundred crewmen. It would return three years later with only one ship and eighteen sailors, Magellan himself having been slain during a battle with islanders in the Philippines. This expedition, however, is remembered not for its losses but for its achievements and discoveries, amongst them the Magellanic Clouds so familiar to the astronomical community.

Risk and exploration go hand in hand. The rôle of space life and medical sciences in contemporary space-flight operations is to mitigate risk. This is achieved through strategies of primary prevention and the study of human physiology in an attempt to understand better the response of the body to the space-flight environment.

The space environment appears to affect nearly every physiological system. Weightlessness itself leads to osteoporosis, muscle wasting, cardiac alterations, disorders of spatial orientation and hand–eye co-ordination as well as space-motion sickness. These changes appear to progress with time and so are of concern when planning future, long-duration-deployment missions and in particular human expeditions to Mars. However, while a few of these changes are maladaptive, the majority represent an appropriate adaptation to the weightless environment of space and predominantly cause problems only upon return to Earth. Furthermore, the difficulties experienced by astronauts when readapting to the Earth environment immediately after landing are short lived and the majority of symptoms pass within hours and days.

There are a variety of possible architectures and trajectories which might be employed in future human missions to Mars. These are constrained by propulsive technology and astrodynamic considerations. Of these architectures the shortest in duration amount to little more than a year in total but allow for only short surface stays, typically around 30 days in length. Longer surface stays are afforded by employing Hohmann transfers for both the outbound and inbound journeys. Such architectures allow mission planners to maximize the time available for exploration, typically committing the crew to a surface stay of around a year and a half. However, with outbound and inbound legs of approximately six months in duration and 18 months spent on the surface, these missions involve long periods of exposure to weightlessness, reduced gravitational loading, and radiation.

The effects of weightlessness on the human body continues to be studied by space agencies in an attempt to understand better how best to maintain the health of astronaut crews. But thus far these effects have not limited our activities in low Earth orbit and do not appear to be show-stopping problems for missions to Mars. However, the physiology of weightlessness, or more correctly of microgravity, provides a unique tool with which to probe certain aspects of human physiology. The fundamental properties of many human biological systems are poorly under-stood. We do not, for example, truly understand how bone is able to model and remodel itself in response to changes in the overall applied mechanical strain. Studies of human physiology in space may contribute to a greater understanding of muscle, and the rôle of the inner ear in spatial orientation tasks and visual tracking.

Space technology will also mean spin-offs for Earth-based medicine. There are several, good, British innovations which have failed to find proper funding because we are not part of the space programme. Robert Marchbanks' Doppler ultrasound probe for monitoring intracranial pressure is but one example. It is believed that space-motion sickness may be due to increases in intracranial pressure. The currently accepted method of measuring this on Earth is by drilling a hole in the skull and performing simple manometry. An ultrasonographic device has been developed which can do the same thing and may be of great benefit to medicine. But spin-off technology is not the sole, nor even the main justification for human space flight. There is a growing acceptance that there is fundamental science that can best and perhaps only be done by human explorers working in partnership with automated platforms.

The nascent field of astrobiology investigates some of the most fundamental questions that life science has to ask at the start of this new century, and the roots of this discipline owe much to the efforts of Frank Drake and his colleagues, who derived the equation:

$$N = R^{\star} f_p \, n_e f_l f_i f_c \, L,$$

where N is the number of civilizations in our Galaxy with which we might expect to be able to communicate at any given time, $R^{\star}$ is the rate of star formation in our Galaxy, f_p is the fraction of those stars that have planets, n_e is the average number of planets that can potentially support life per star that has planets, f_l is the fraction of the above that actually go on to develop life, f_i is the fraction of the above that actually go on to develop intelligent life, f_c is the fraction of the above that are willing and able to communicate, and L is the expected lifetime of such a civilization for the period that it can communicate across interstellar space.

Drake's famous equation provides a general expression for the number of civilisations in our Galactic neighbourhood, at this moment in time, capable of communicating with us over interstellar distances. To some, questions regarding the preponderance of intelligent life in the Universe appear at best unanswerable and at worst absurd. Indeed, when Drake's equation was first published in 1961 science was only capable of investigating the first term: the rate of star formation in our Galaxy. The last three terms in the Drake equation, f_i, f_c, and L refer to the development of intelligence, communications technology, and the lifetime of civilizations. Science is at this time unable sensibly to constrain these terms and they remain a source for conjecture. However, if we leave aside these terms we are left with an expression for the ubiquity of life in the Universe, U.

$$U = R_x f_p \, n_e f_l,$$

where R_x is the number of stars in the Universe, f_p is the fraction of those stars that have planets, n_e is average number of planets that can potentially support life per star that has planets, and f_l is the fraction of the above that actually go on to develop life.

Advances in extrasolar-planet detection, along with the recent launch of the *COROT* mission, will allow us better to constrain the second and third terms in the equation above. Constraint of the fourth term, f_l, the fraction of planets potentially capable of supporting life that actually go on to do so, will require the comprehensive geological exploration of both the lunar and Martian surfaces. These questions pertain to the nature and origin of life and its ubiquity in the Universe. This knowledge is as fundamental to life science as particle physics is to physical science and it is increasingly clear that human exploration will be a necessary component of this effort.

Finally, we should not ignore the wider benefits of human space flight. Human space exploration is a first-class tool for inspiring school children and encouraging them into science careers. The supply of scientists and engineers in the UK is currently under threat. In the period between 1994 and 2004, 24 UK physics departments closed. It is sobering to consider that, should this trend continue, by 2024, when the human exploration of the Moon is scheduled to begin in earnest, there will be no physics departments left in this country and likely no science base of sufficient quality with which to prosecute those programmes of exploration.

Ferdinand Magellan, son of Portugal, was unable to fulfil his exploratory ambitions under the flag of his parent nation. He sailed instead for Spain and though the expedition was marred by loss it is best remembered for its extraordinary legacy of discovery. At the start of this century we stand at the shores of the new ocean of space. The events of these times will decide how this country chooses to sail that ocean and whether we choose to rise to the challenges before us and seize the opportunities at hand or instead shrink from an imaginary threat.

The President. Thank you for that very inspiring and entertaining talk; are there any questions?

Dr. R. C. Smith. You seem to be making an argument that is based mainly on the medical development or physiological development that you are interested in; why go to Mars, why not just the *International Space Station*?

Dr. Fong. That's not the way I meant it to come across; I think that there are many fundamental aspects of life science that we can learn from space physiology, but I also think it is a two-way process: we can learn more about ourselves by going into space, but at the same time humans will facilitate part of some elements of the exploratory effort that this century has to offer.

Rev. G. Barber. You spoke about the problems of the micro-gravity environment; what about radiation, especially for a long trip to Mars?

Dr. Fong. As I understand it, on a one-year journey to Mars, the background cosmic radiation is still within safe limits. The problem is solar particle events, and the Apollo crews, who got pretty much outside the bulk of the protective magnetosphere, were very lucky not to experience one, since the epoch of their flights was actually one of very high solar activity. It is more difficult for the long journeys because we don't know enough about solar particle events, but my feeling is that we'll engineer that out. I think that the technological advances that will make these missions possible in the future will be more rapid transit — nuclear-thermal rockets, or magneto-plasma rockets — plus engineering on the surface and shielding on the vehicle. Radiation is not my special area, but I'm led to believe that a water jacket around the central cabin can provide a lot of shielding, possibly not enough for a large solar particle event, but for smaller events I understand it will be sufficient.

Mr. H. Regnart. May I make a brief comment, rather than a question? It's just this: I've wanted to go to Mars since I was four and a half, which is more than fifty years ago, so naturally my emotional bias is very much in favour of human space flight. But considering the enormous costs, the risks, and the cost of trying to minimize the risks, there are very good rational grounds for sticking to robotics. May I therefore suggest two rational grounds for human space flight, both of an insurance, or reinsurance nature: the first is in case we can't cope with an incoming bolide robotically, and the second is in case we carry on as we are and have to put a Noah's Ark, for example, on the Moon or Mars for the survival of mankind and some other species after we have made this place uninhabitable.

The President. I'm going to have to stop you there; I think you have made your point very well, thank you.

Mr. J. Stone. We have the knowledge to produce the hardware to go out, and we have the desire — in fact, that's not stopping us. What *is* stopping us is people in the Government who, several years ago, said they couldn't see any commercial future for satellites. Meanwhile the rest of Europe has manned space programmes and they see a benefit in that while we seem to be a little out of touch, to say the least.

Dr. Fong. I've been thinking about this for ten years and I'm not a human-space-flight evangelist. I don't say we must do it because it's there, or any of those things. I don't think we have sufficient evidence to justify £150M a year to sign-up at this time — after all that's nearly our entire public spending on space as it is. I think you would rightly see it as a threat, if I said to you that is what I want to ask the Chancellor to do. However, I do believe we need to take the next step — that we need actually to suck it and see. I think that means a strategic involvement at a fraction of the sign-up cost and setting some metrics. If there are no more kids in science by the end of that, or if you haven't done any good science, then fine, peel away and never do it again, but we haven't even given it that chance and I think that we need to.

The President. The RAS Council actually discussed this issue yesterday, because the RAS has been widely quoted, including by the Director of NASA, as supporting human space flight. You will recall the RAS commissioned a report on the scientific benefits of human space flight which came to the conclusion there were some kinds of science that probably could only be done by human beings — deep drilling on Mars, for example. However, that is not the same thing as saying that we should now switch from our current space-science programme, which is entirely robotic, to one that is based on human space flight. We will be consulting the membership of the RAS on this very issue, and we will put out a statement and ask you whether you support it, and also start a forum on the web page. But I think our view is that within the current constraints of a fixed budget for space science, then scientifically the robotic programme is overwhelmingly better value for money. There may be other reasons for involvement in a human space-flight programme — for example, medical, technological, and outreach and education benefits. But in terms of the purely scientific arguments and in the context of a fixed budget, then it is harder to see that we should go for it — at least as astronomers.

Dr. Fong. I can entirely sympathize with that view, only to say that I am not talking about programmatic sign-up here. I am talking about strategic funds, and although it is possible to focus on the scientific view and value for money in the short term, that *is* a short-term view; our supply of scientists is in desperate trouble and nothing we have done so far has changed that, so I think that deserves an opportunity.

The President. I've given you the last word; thank you very much again for your interesting talk. [Applause.]

In the first century BC, Hero of Alexandria was famous for his automata and machines. A surviving geared device is the Antikythera Mechanism, which is supposedly from Rhodes, of which the Greek poet Pindar wrote: "The animated figures stand, Adorning every public street. And seem to breathe in stone, or move their marble feet." Mike Edmunds and Tony Freeth are going to tell us what this Mechanism was for.

Professor M. G. Edmunds. In 1900 a shipwreck was discovered off the Mediterranean island of Antikythera. The wreck probably occurred around 70 BC, and some of the very first underwater archaeology recovered magnificent cast-bronze figures and other ancient Greek artefacts. Now in the National Archaeological Museum in Athens, the hoard included the remains of a bronze mechanism, originally about the size of a shoe-box, which has subsequently been shown to contain thirty gear wheels. This 'Antikythera Mechanism' is considerably more complex that any other known mechanism from the subsequent millennium, until one reaches the era of the medieval cathedral clocks. Excellent previous work by Price, Bromley and Wright has shown convincingly that the Mechanism is astronomical in purpose, and today we can report the results of a new international investigation involving the UK, Greece, and the USA. Known as 'The Antikythera Mechanism Research Project' and funded principally by the Leverhulme Trust, we have used modern imaging techniques to increase greatly the knowledge of the Greek texts engraved on the Mechanism, and (extending previous research) to establish the function of the gearing and scales. Our initial results have just been published in the journal *Nature* (2006 November 30), provoking considerable public interest around the world. The Mechanism probably originates from Rhodes, and the epigraphy of its inscriptions implies a construction date around 140–100 BC. A few literary references exist to mechanical devices that displayed celestial phenomena, and Cicero reports having seen something of the sort in Rhodes in the 1st Century BC. Other artefacts on the wreck imply that the lost ship may well have called in at Rhodes on its way trading from the Eastern Mediterranean to Rome. The actual primary purpose of the device — *i.e.*, why it was built — remains uncertain, although at least it is now known what it could actually do. The astronomy encoded in the gears and scales is perfectly consistent with the epoch of its construction, and is very largely based on Babylonian period relationships. In particular it shows the 19-year Metonic Cycle (and its near-multiple the Callippic Cycle) relating synodic lunar months and years, and the Saros Cycle of solar and lunar eclipses (with its multiple, the Exeligmos Cycle). It had long been known that the Mechanism displayed the position of the Sun and Moon in the Zodiac, but our new investigations show that the sophistication and design of the lunar gearing is astounding, and certainly challenges conservative assumptions about technological capabilities of the ancient Greeks. This raises major questions about what other mechanisms, apparently now lost, the ancient Greeks may have built. The two main investigative techniques we have used are (*i*) a visual surface-imaging technique invented by Malzbender at Hewlett-Packard, and (*ii*) three-dimensional microfocus X-ray computed tomography (essentially high-resolution body-scanning) developed by X-Tek Systems Ltd in the UK for industrial examination of components such as turbine blades and printed circuits. Both industrial partners have been superb, giving freely of their time in pursuit of our research aspirations. The logistics of the research were non-trivial, including the necessity of transporting the seven-and-a-half-ton X-ray machine from Hertfordshire and into the basement of the National Museum in Athens! The quality of imaging has been very encouraging, and my colleague Dr. Tony Freeth will outline the structure of the Mechanism.

Dr. A. Freeth. The overall architecture of the Antikythera Mechanism was described by Price in his seminal paper, *Gears from the Greeks*. I will talk first about the two major dials on the back face of the Mechanism. Our reading of the phrase "spiral divided into 235 sections" confirmed Wright's observation that the back dials have a spiral form, and we have discovered the remains of a 'pointer–follower' which would travel along a spiral slot, indicating exactly which turn of the spiral dial should be read by the pointer. The upper back dial shows the 235 synodic months of the Metonic Cycle arranged in a five-turn spiral, with an inset subsidiary dial recording the four segments of the Callippic Cycle. The gear train to drive these dials is relatively straightforward, although four of the necessary gears are missing. The train does, however, include a gear with 53 teeth, a somewhat unusual number, but one that turns out to be critical in subsequent interpretation. The lower back dial has a total of 223 divisions, in a four-turn spiral, and some divisions are marked with what we call 'glyphs' — groups of about six Greek characters and symbols. Three of these glyphs are clearly visible on the surface of the surviving fragments, and we discovered a further 13 through the X-ray tomography. By matching the position of these glyphs to ancient eclipse records it is clear that what is shown by the dial is the Saros eclipse cycle — a prediction of the likely occurrence of solar or lunar eclipses based on previous records, and well-known to the Babylonians. The glyphs may also indicate the anticipated time of eclipse. The gearing to this dial shares the first four gears with the gear train to the Metonic Dial (including the 53-toothed gear) and thereafter includes a large 'turntable' gear, which we conjecture was driven by a hypothetical gear on a (now) broken-off shaft. The large gear also has two further gears mounted on it (*i.e.*, epicyclic gearing). These turn on slightly eccentric axes and a pin in one of these gears drives a slot in the other, inducing a small periodic variation in the motion of the gear with the slot. This remarkable mechanism is combined with other gearing to reproduce the first lunar anomaly — the variation in rate of lunar motion across the sky due to the ellipticity of the lunar orbit. By mounting these gears epicyclically on the large turntable gear, the brilliant designer has accomplished the feat of introducing the periodic variation of the Moon's motion, but at the correct period of the anomalistic, rather than the sidereal, month. It is the rotation of the large turntable gear at the rate of the precession of the lunar apses that achieves this — and in ensuring this correct rate the 53-tooth gear is necessary. It is tempting (but probably over-claiming!) to associate the Mechanism with Hipparchus, who was active in Rhodes at the beginning of the period of dating of the Mechanism and who is reported to have developed a theory of the first anomaly. The texts on the Mechanism suggest that there may also have been a display of some planetary positions — Venus and Mercury are certainly mentioned, although the gearing is lost. We do not yet know whether the superior planets were displayed, and much remains to be done on interpretation of the texts. Overall, the design of the Mechanism, with its interlaced gear trains and epicyclic gears, is really quite extraordinary. Our new model of the Mechanism for the first time explains the functions of all (except one) of the surviving gears and reveals a coherence, economy, and unity of design. It explains the tooth counts in all these gears and shows how their essential astronomical prime factors can be derived from two ancient cycles of the Solar System — the Metonic Cycle and the Saros Cycle.

The President. Well, this is clearly an absolutely stunning achievement of Alexandrian astronomy, and also of the authors of this talk to unravel it from the remains that we saw. We have time for one or two quick questions.

Professor I. Roxburgh. Could I ask, do you know of any comparable technological achievements in other civilizations?

Dr. Freeth. Not from the same period, no. The classic example that's quoted is Su Sung's astronomical clock, which I think is about 1000 AD, so there's a huge gap in the evidence. An example that bridges it is the Byzantine sundial calendar containing just a few gears that Michael Wright studied when he was at the London Science Museum.

Dr. N. Kollerstrom. I think it's wonderful the way that you put this apparatus at the same time and place as Hipparchus, the great astronomer of Rhodes, by pushing it back a century. May I say that the Greek astronomers certainly had known about the Metonic Cycle, but it's not generally believed they used the Saros cycle; for example, Ptolemy's *Almagest* doesn't say anything about the Saros in relation to eclipse prediction.

Dr. Freeth. I'm not an historian of science but I understand that the ancient Greeks were certainly aware of the Saros Cycle and used it, but in terms of the dating we haven't really pushed it back 100 years. Price said 87 BC in what is now I think considered a spurious dating argument. Our link with Hipparchus is frankly very speculative, so you can't really make a direct connection. Obviously we all love to make these links, but it's probably a bit after he died. It could be within his lifetime, but I don't think we want to jump to that as a conclusion. But certainly, it would have been in the shadow of Hipparchus.

Professor N. O. Weiss. If they were capable of producing something as marvellous as this, they must surely have started by making much simpler devices. Is there any evidence of such things?

Professor Edmunds. No. [Laughter.] There are references in literature, particularly Cicero and some writings of Archimedes, to show that they did build things like this. Nothing actually refers to this particular mechanism, but they did build orrery-type mechanisms. This is the only artefact that survives.

Dr. Freeth. Cicero wrote that he'd seen an instrument that Posidonius had made, which with every turn of the handle showed the position of the Sun, Moon, and five planets over a day and night. It's often been linked to this mechanism, but certainly if you were looking at the back dials you'd turn it 24 hours and absolutely nothing happens because of the long time periods. In fact, you could consider it as the most boring astronomical display in history when you use it in that way. So I think myself that it was used as a prediction machine, for looking a few years into the future.

The President. Basically it is the first computer, then. With great regret I think I'm going to have to stop this discussion, but thank you very much for a fascinating talk. [Applause.]

The President. Our next talk is the Gerald Whitrow Lecture. I'm sure several people here will remember Gerald Whitrow with great affection. Before we start the lecture I'd like to thank Mrs. Whitrow; unfortunately she can't be with us today, but her representative Trevor Stuart is here, I see, and I hope you'll let Mrs. Whitrow know that we all wish her a very speedy recovery. Our Whitrow Lecturer is Professor Carlos Frenk from the University of Durham and his title is 'Our implausible Universe', and I hope he's going to tell us whether it's the Universe that is implausible or the model! [Laughter.]

Professor C. Frenk. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics.*]

The President. So I think it's both the Universe and the model that are implausible! That's the conclusion! Questions?

Rev. G. Barber. We've just heard about the Antikythera Mechanism, which was a wonderful computer and obviously an astounding achievement at the time. The problem was, of course, it was working on the wrong paradigm. Your computer simulations are again wonderful in terms of the picture they present but might it be possible they are working on the wrong paradigm, given that we cannot find the Higgs boson or the inflaton to cause inflation, or the dark-matter particle or dark energy?

Professor Frenk. I think that it is entirely possible that everything I have been telling you is only a part of the picture and that something is fundamentally missing. However, what I suspect is that whatever the next version of the truth turns out to be, it has got to look a hell of a lot like this one, because you saw the amazing agreement with the microwave-background measurements and the power spectrum of the galaxy distribution. It would be a slightly perverse coincidence if we got lots of really wrong hypotheses and ended up with the right answer. Of course it is not out of the question, but I think the most likely possibility is that there is a deeper theory that we don't yet know, of which what I've been telling you about now is a sort of approximation or a kind of low-level manifestation of this higher theory. Maybe the reason why it looks so implausible and ugly in some ways is because we don't yet understand that deeper theory. You could have said the same thing, for example, about the Solar System before we had Newton's laws. It must have seemed ugly and implausible for the Greeks and those who came later and shared their aesthetic ideas about the perfection of the sphere, for example, to see that the orbits of the planets were ellipses. Today, we know that ellipses are not ugly and implausible; they are expected and beautiful. Why? Because Newton's theory tells us that is the natural order for the planets. There may well be a deeper theory which will then make this implausible Universe look not implausible and inelegant but quite the contrary.

Rev. Barber. The point being, of course, that the epicyles give the right answer, as far as I can tell.

The President. I think you made your point!

Mr. Stone. Just one comment: you talk about the implausibility of all this and we wonder if we can understand it and what other versions there might be. This reminds me of the *Hitch-Hiker's Guide to the Galaxy*'s great answer to the Universe being 42, so what is the question? Well my suggestion for that is — how many times does God have to recreate the Universe until He gets it right?

Professor Frenk. According to String Theory it is $10^{10^{120}}$ times!

The President. Ian, last question.

Professor Roxburgh. As one listens to modern cosmologists talking from other areas of astronomy you get the impression that they add bits in because there's something they can't explain, so you have to have inflation to explain the horizon problem and so on. I want to force you to say what you can predict that we can test; for example, the gravitational radiation background. Could it be measured and could it either substantiate or contradict the model?

Professor Frenk. I think we must agree that science is a dialectic process that evolves. You do not want to have a theory that is frozen and which then just dies when it disagrees with the facts. Science is a process whereby, as you encounter a theory that does not work well, you try to improve it. In that context, the theory of inflation in the cold-dark-matter model made a very important prediction, the prediction of those acoustic oscillations that we saw. These are predictions that date to the 1980s and were corroborated by the CMB data; there are other further predictions that can be made and you alluded to some of them. I think that it is fair to say that although they provide very strong support for inflation, they don't really prove it completely. There are other possibilities that could have ended up with the same result. But inflation predicts other things, like, as you said, a background of gravitational waves which are really only, as far as we know, produced by inflation, and they may be detectable in the next generation, or maybe the generation after the next, of gravitational-wave detectors. So that would be, I think, further confirmation. I feel you should be impressed that many of the features that I showed were actually predicted in the 1980s; the acoustic oscillations is one, the power spectrum of the galaxy distribution is another, and the theory really has not changed that much. The main ideas of inflation and cold dark matter were there in the 1980s, and the only thing that really has changed are the parameters, if you like, or the composition of the Universe, with the major change being the introduction of the dark energy. Other than that, it is not really the case that cosmologists have been massaging the theory as the data have come along. The main ideas have been there for a long time and been tested in a way that I think is pretty compelling.

The President. I think we should stop there and thank Carlos for a wonderful lecture. [Applause.] The next A & G meeting will be on 2007 January 12. I wish you all a happy Christmas and a prosperous New Year.

ORBITS OF HYADES MULTIPLE-LINED SPECTROSCOPIC BINARIES

PAPER 6: THE DOUBLE-LINED SYSTEM HD 28545

By J. Tomkin
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New observations of HD 28545 made at McDonald Observatory reveal the spectrum of the secondary for the first time and provide an improved spectroscopic orbit for this Hyades binary. The G8 V spectral type of the primary combined with the mass ratio ($m_1/m_2 = 1{\cdot}415 \pm 0{\cdot}004$) indicate a late-K spectral type for the secondary. The linear size of the relative orbit is $a \sin i = (155{\cdot}1 \pm 0{\cdot}3) \times 10^6$ km. The corresponding angular separation on the sky of 20·1 mas is comfortably within the grasp of modern optical interferometers.

Introduction

Hyades binaries with well-determined visual and double-lined spectroscopic orbits can provide accurate distances to the systems and masses of their component stars. Here we report the results of a new look at the Hyades spectroscopic binary HD 28545 (BD + 15° 638, cluster designation vB 182). Basic data for HD 28545 are: α (2000) $4^h\ 30^m\ 34{\cdot}9^s$, δ (2000) $+15°\ 44'\ 2''$; spectral type G8 V; $V = 8{\cdot}94$, $(B-V) = 0{\cdot}82$. The Hyades membership of HD 28545 is well established. Griffin & Gunn[1], who in 1981 reported their discovery that HD 28545 is a spectroscopic binary and the first determination of its orbit, reviewed the proper-motion evidence available at the time, which is consistent with membership, and in addition found that its systemic velocity is also consistent with membership. Since then, the thorough investigation of Hyades proper motions by Schwan[2] has further confirmed HD 28545 as a cluster member. HD 28545 is not in the *Hipparcos Catalogue*[3]. Its absence cannot be explained on grounds of faintness — many stars, including Hyades stars, fainter than HD 28545 are in the *Catalogue* — and appears to be nothing more than the luck of the draw.

Griffin & Gunn's[1] observations, which were photoelectric radial velocities measured with the 200-inch *Hale* telescope and the Cambridge 36-inch reflector, showed that the orbital period ($P = 358{\cdot}4$ d) is very close to a year and that the orbit is quite eccentric ($e = 0{\cdot}364$), but did not detect the secondary spectrum. The mass function they determined ($f(m) = 0{\cdot}0791\ M_\odot$) was large enough, however, to suggest that the secondary could not be a lot fainter than the primary and that a closer look at higher signal-to-noise ratio might well reveal its spectrum. Here we report new McDonald observations of HD 28545 which do, indeed, detect the spectrum of the secondary and allow the determination of an accurate double-lined spectroscopic orbit for the first time.

Observations, radial velocities, and orbit

The observations were made on the 2·7-m and 2·1-m telescopes at McDonald Observatory between 1995 and 2005. The first 16 observations were made with the 2·7-m telescope and the last 17 were made with the 2·1-m. The observations with the 2·7-m telescope were at the coudé focus and used the *2dcoudé* echelle spectrometer[4] in a mode giving 60 000 resolving power, while those with the 2·1-m telescope were at the Cassegrain focus and used the *Sandiford* Cassegrain echelle spectrograph[5], which also gives a resolving power of 60 000. More details about the observations, the reduction of the data, and the velocity-measurement procedure have been given in earlier papers in this series[6,7].

Weak secondary lines are clearly present in these spectra. Fig. 1 shows a V I line at 6039 Å in one of the 2·7-m-telescope observations of HD 28545; a well-defined primary component and its weaker red-shifted secondary counterpart are evident. Table I gives the dates, heliocentric Julian dates, and heliocentric radial velocities of the observations.

A solution of the new McDonald velocities and the Griffin & Gunn[1] velocities combined was done in order to determine the orbital elements. The weights used in this solution for the primary and secondary velocities and for the different telescopes are given in Table I. Two early observations made by Wilson[8] in 1942 and 1943 with a one-prism spectrograph attached to the 60-inch reflector at Mount Wilson were included in the solution, but were unweighted. (The individual velocities and dates for those two observations were later reported by Abt[9].) As a preliminary step, the Griffin & Gunn velocities were adjusted so as to put

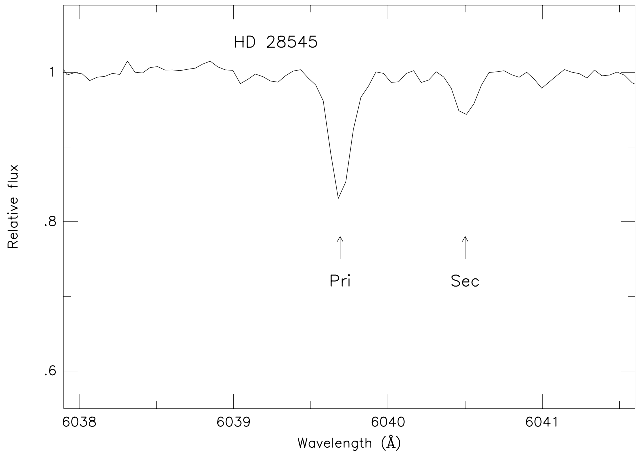

FIG. 1

The primary and secondary components of a V I line (rest wavelength 6039·736 Å) in the spectrum of HD 28545 observed with the 2·7-m telescope on 1995 September 30.

TABLE I

Radial velocities of HD 28545

Date (UT)	*Tel.*	*HJD* −2 400 000	*Phase*	*Velocity* Pri km s⁻¹	*Velocity* Sec km s⁻¹	*(O−C)* Pri km s⁻¹	*(O−C)* Sec km s⁻¹
1942 Dec 31	MtW	30724·63	0·271	37·00	—	−0·56	—
1943 Dec 5	MtW	31063·86	0·217	36·00	—	1·99	—
1972 Jan 6	Pal	41322·76	0·835	47·12	—	−0·04	—
1973 Jan 13	Pal	41695·78	0·875	44·22	—	0·13	—
1973 Dec 15	Pal	42031·81	0·812	48·12	—	−0·14	—
1973 Dec 16	Pal	42032·82	0·815	47·72	—	−0·42	—
1974 Feb 2	Cam	42081·38	0·951	33·19	—	−0·24	—
1974 Feb 7	Cam	42086·39	0·965	29·59	—	−1·33	—
1974 Sep 22	Cam	42312·63	0·596	51·09	—	1·85	—
1974 Nov 23	Pal	42374·89	0·770	49·12	—	−0·46	—
1974 Nov 24	Pal	42375·82	0·772	49·02	—	−0·51	—
1975 Jan 12	Cam	42425·44	0·911	39·99	—	0·06	—
1975 Feb 27	Cam	42471·35	0·039	22·19	—	−0·08	—
1975 Sep 4	Cam	42659·65	0·564	46·59	—	−2·05	—
1975 Oct 2	Cam	42687·57	0·642	49·99	—	0·11	—
1975 Nov 4	Cam	42720·61	0·734	49·09	—	−1·00	—
1975 Dec 12	Pal	42758·81	0·840	46·02	—	−0·79	—
1976 Mar 5	Cam	42843·33	0·076	26·39	—	3·42	—
1977 Sep 4	Cam	43390·67	0·603	47·99	—	−1·37	—
1977 Sep 18	Cam	43404·66	0·642	49·69	—	−0·19	—
1977 Nov 4	Cam	43451·52	0·773	49·39	—	−0·12	—
1977 Dec 4	Cam	43481·54	0·857	41·89	—	−3·80	—
1978 Jan 24	Cam	43533·43	0·001	27·09	—	1·98	—
1978 Sep 23	Cam	43774·67	0·674	49·99	—	−0·15	—
1978 Oct 4	Cam	43785·63	0·705	49·49	—	−0·72	—
1978 Oct 7	Cam	43788·61	0·713	50·69	—	0·49	—
1978 Nov 9	Cam	43821·57	0·805	49·29	—	0·73	—
1978 Nov 20	Cam	43832·61	0·836	47·49	—	0·40	—
1978 Dec 9	Pal	43851·82	0·889	41·92	—	−0·67	—

TABLE I (*concluded*)

Date (UT)	*Tel.*	*HJD* −2 400 000	*Phase*	*Velocity* *Pri* km s⁻¹	*Sec* km s⁻¹	*(O−C)* *Pri* km s⁻¹	*Sec* km s⁻¹
1979 Jan 9	Cam	43883·41	0·978	29·19	—	0·51	—
1979 Jan 11	Cam	43885·44	0·983	27·19	—	−0·55	—
1979 Feb 28	Cam	43933·36	0·117	25·49	—	−0·37	—
1979 Mar 4	Cam	43937·35	0·128	26·59	—	−0·21	—
1979 Mar 9	Cam	43942·31	0·142	29·49	—	1·51	—
1979 Mar 12	Cam	43945·34	0·150	28·99	—	0·29	—
1979 Mar 17	Cam	43950·30	0·164	28·29	—	−1·58	—
1979 Nov 5	Pal	44182·90	0·813	47·32	—	−0·92	—
1979 Nov 19	Cam	44197·49	0·854	43·99	—	−1·92	—
1979 Nov 28	Cam	44205·61	0·876	43·39	—	−0·58	—
1979 Dec 25	Cam	44233·48	0·954	33·59	—	0·76	—
1980 Jan 1	Cam	44239·50	0·971	29·79	—	−0·04	—
1980 Jan 5	Cam	44243·51	0·982	28·59	—	0·66	—
1980 Jan 13	Cam	44251·50	0·004	25·89	—	1·16	—
1980 Jan 20	Cam	44259·39	0·026	22·69	—	−0·08	—
1980 Jan 22	Cam	44261·42	0·032	21·99	—	−0·50	—
1980 Jan 26	Cam	44265·39	0·043	21·69	—	−0·50	—
1980 Mar 9	Cam	44308·34	0·163	29·69	—	−0·08	—
1980 Mar 22	Cam	44321·30	0·199	31·69	—	−0·98	—
1995 Sep 30	2·7	49990·898	0·015	23·68	63·73	0·03	0·04
1995 Oct 1	2·7	49991·883	0·017	23·40	63·97	−0·01	−0·06
1995 Oct 12	2·7	50002·941	0·048	22·14	65·71	−0·02	−0·09
1995 Oct 15	2·7	50005·934	0·057	22·21	65·85	−0·03	0·16
1995 Dec 2	2·7	50053·873	0·190	32·03	51·96	0·04	0·06
1995 Dec 4	2·7	50055·821	0·196	32·35	51·19	−0·06	−0·11
1996 Jan 4	2·7	50086·724	0·282	38·42		—	—
1996 Jan 6	2·7	50088·780	0·288	38·79		—	—
1996 Feb 6	2·7	50119·631	0·374	42·77	36·84	0·01	0·18
1997 Aug 29	2·7	50689·981	0·965	30·91	53·27	−0·02	−0·12
1997 Aug 31	2·7	50691·965	0·970	29·97	54·96	0·02	0·18
1997 Dec 14	2·7	50796·768	0·263	37·03	44·97	−0·02	0·24
1997 Dec 15	2·7	50797·776	0·265	37·36	44·84	0·13	0·35
1998 Oct 2	2·7	51088·922	0·078	23·06	64·50	0·03	−0·06
1998 Oct 5	2·7	51091·955	0·086	23·50	63·88	−0·04	0·03
1999 Mar 12	2·7	51249·586	0·526	47·81	29·57	0·03	0·01
2000 Feb 3	2·1	51577·717	0·441	45·21	32·66	−0·10	−0·39
2000 Oct 26	2·1	51843·859	0·183	31·39	52·56	−0·06	−0·10
2000 Nov 13	2·1	51861·858	0·234	35·16	46·91	−0·02	−0·48
2000 Dec 7	2·1	51885·766	0·300	39·35		—	—
2000 Dec 9	2·1	51887·774	0·306	39·50		—	—
2001 Jan 11	2·1	51920·717	0·398	43·65	35·10	−0·09	−0·17
2001 Jan 12	2·1	51921·694	0·401	43·83	35·22	−0·02	0·10
2001 Oct 30	2·1	52212·990	0·213	33·59	49·08	−0·14	−0·36
2001 Nov 1	2·1	52214·924	0·219	33·98	48·61	−0·14	−0·28
2001 Nov 25	2·1	52238·875	0·285	38·54	42·94	0·13	0·13
2002 Feb 1	2·1	52306·724	0·475	46·40	31·74	0·02	0·20
2002 Oct 19	2·1	52566·924	0·200	32·88	50·78	0·10	0·00
2002 Nov 29	2·1	52607·781	0·314	39·88		—	—
2003 Mar 21	2·1	52719·640	0·626	49·69	27·16	−0·01	0·31
2003 Apr 20	2·1	52749·600	0·710	50·19	25·94	−0·02	−0·18
2003 Sep 6	2·1	52888·960	0·099	24·55	62·79	0·12	0·20
2005 Apr 27	2·1	53487·589	0·769	49·69	—	0·09	—

Telescope code: MtW = Mt Wilson; Pal = Palomar 200-inch;
Cam = Cambridge 36-inch; 2·7 = McDonald 2·7-m; 2·1 = McDonald 2·1-m.

The primary velocities from the 2·7-m, 2·1-m, Palomar, and Cambridge telescopes have weights 1·0, 0·5, 0·01, and 0·005, respectively; the secondary velocities from the 2·7-m and 2·1-m telescopes have weights 0·1 and 0·05, respectively; the Mt. Wilson velocities are unweighted.

them on the same scale as the new velocities. These adjustments, which have been explained previously[6], were −1·08 and −0·81 kms^{-1} for the Palomar and Cambridge velocities, respectively. The adjusted velocities were used in the orbit solution and are also the ones given in Table I. Table II gives the orbital elements from this solution and Fig. 2 shows the observed velocities and the calculated radial-velocity curve.

As already noted, the orbital period (358 d) of HD 28545 is very close to a year. This coincidence means that the gap in coverage of the orbital phase, corresponding to when HD 28545 is behind the Sun, persists from year to year with the result that the observations of both Griffin & Gunn and this investigation suffer from gaps in their phase coverage. However, the two sets of observations are ~ 22 years apart so their gaps are at different phases — that of Griffin & Gunn is from 0·20 – 0·55, while this investigation's is from 0·77 – 0·96 — so the combined observations complement each other nicely, as can be seen in Fig. 2. As an item of trivia, we also remark that with a view to narrowing the gap in phase coverage, the last of the present observations was made on 2005 April 27 at dusk at an heroic 3·0 airmass.

TABLE II

Orbital elements of HD 28545

P (days)	= 358·484 ± 0·013	T (HJD)	= 2450702·66 ± 0·16
γ (km s^{-1})	= 40·233 ± 0·016	q $(= m_1/m_2)$	= 1·415 ± 0·004
K_1 (km s^{-1})	= 14·03 ± 0·02	$m_1 \sin^3 i$ $(M_\odot)$	= 0·679 ± 0·004
K_2 (km s^{-1})	= 19·85 ± 0·05	$m_2 \sin^3 i$ $(M_\odot)$	= 0·480 ± 0·002
e	= 0·3703 ± 0·0013	$a_1 \sin i$ (10^6 km)	= 64·24 ± 0·11
ω (degrees)	= 141·1 ± 0·2	$a_2 \sin i$ (10^6 km)	= 90·87 ± 0·25

R.m.s. residual (unit weight) = 0·058 km s^{-1}

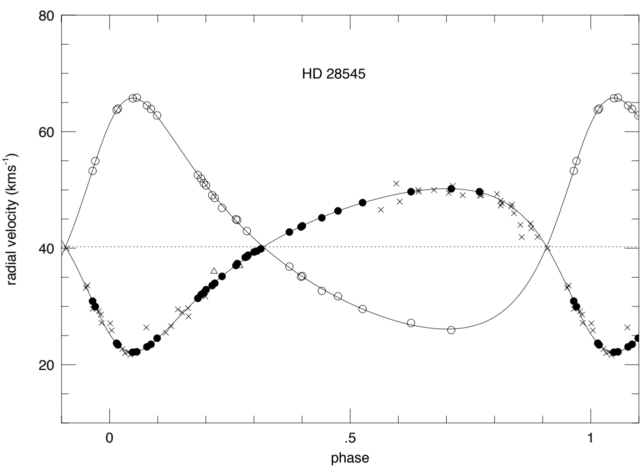

FIG. 2

The primary and secondary radial velocities of HD 28545 and the calculated radial-velocity curves. Filled and empty circles are McDonald primary and secondary velocities, respectively; crosses are Griffin & Gunn[1] velocities (primary only); triangles are Wilson[8] velocities (primary only).

Discussion

The radial velocities, which are well described by the orbit solution, show no sign of any variation in addition to those caused by motion in the spectroscopic binary orbit; in particular there is no evidence of any long-term trend in the radial velocities such as might be caused by a distant, faint stellar companion accompanying the spectroscopic-binary pair. This lack of evidence of a distant companion is consistent with what Patience *et al.*[10] found in their speckle-interferometry survey of multiplicity in the Hyades: their observations of HD 28545, in which the spectroscopic binary is a single source, show no sign of a companion.

The determination of the spectroscopic orbits of the primary and secondary allows an estimate of the linear size of the relative orbit, apart from the usual veil of uncertainty due to the unknown value of sin i. With the aid of the $a_1 \sin i$ and $a_2 \sin i$ in Table II one finds $a \sin i = (155 \cdot 1 \pm 0 \cdot 3) \times 10^6$ km. At the distance of HD 28545, which Patience *et al.*[10] estimate to be 51·5 pc, this corresponds to an angular size of 20·1 mas, so the system is a good candidate for resolution as a visual binary by optical interferometry — either from the ground or from space. The combination of a spectral-type-based mass for the primary and the mass ratio (Table II) allows one to estimate the mass of the secondary and, thus, its spectral type. The G8 V spectral type of the primary and the spectral type *versus* mass calibration of Drilling & Landolt[11] lead to an estimated mass for the primary of $0 \cdot 84\ M_\odot$, which with the mass ratio of $m_1/m_2 = 1 \cdot 415$ gives an estimated mass and spectral type for the secondary of $0 \cdot 59\ M_\odot$ and K7.

In conclusion, our current knowledge of HD 28545 reveals a spectroscopic binary composed of a G8 V star and a fainter companion of approximately K7 spectral type; the two stars orbit each other in an elliptical orbit ($e = 0 \cdot 370$) with a period (358·5 d) very close to one year. The angular separation between the primary and secondary makes the system a good target for optical interferometry. Neither the radial velocities nor direct imaging show any sign of the presence of additional stars in the system.

Acknowledgements

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 194: HD 113997, HD 114931, HD 115588, AND HD 116880, WITH A PRELIMINARY DISCUSSION OF HD 113995

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This paper presents results on five spectroscopic binaries that are all in the vicinity of the North Galactic Pole. The four stars which form its principal subject are all too faint to feature in the *Hipparcos* catalogue, and very little is known about them apart from what is published in a paper not retrievable through *Simbad* plus what is presented here. They all have orbits of modest eccentricity. HD 113997 is an early-K giant and has a period close to 2000 days; the other three stars are dwarfs of types F9, G3, and K1, respectively, and have periods of about 2100, 300, and 120 days. The mass function of HD 115588 is so large that the companion must be more massive than the star whose velocity is observed; the favoured explanation is that the secondary is itself a double star consisting of two late-main-sequence objects which, though too faint to give individually detectable signatures in radial-velocity traces, may yet contribute to the unusually ragged orbital residuals and perhaps cause the system's colours and other photometric properties to mimic those of a star rather later than the real type of the primary.

HD 113995 is somewhat brighter ($8^{m}\cdot4$) than the stars whose orbits are given here, and was on the *Hipparcos* programme; its type is K4 III. No orbit can be given, but the star is found to be a very unequal double-lined binary in a high-eccentricity orbit with a period of indeterminate length probably longer than 40 years; a periastron passage was witnessed in 1996.

Introduction

From time to time, papers in this series have presented orbits of spectroscopic binaries that were discovered in the Cambridge survey of the radial velocities of all the late-type *HD* stars within 15° of the North Galactic Pole (NGP). The survey was amalgamated with an equally comprehensive photometric (*BV, DDO*) one undertaken by Dr. K. M. Yoss and was published[1] about ten years ago. Out of approximately 125 spectroscopic binaries (almost all of them new discoveries) that were identified in the survey, only about half have so far had their orbits published. Nearly all the others (approximately 60, not counting those whose orbits are given below), remain under observation, but some of them promise to have periods so long that the solution of their orbits must be left to posterity. One such object is HD 113995, for which a preliminary discussion is given here.

Orbits are first given for four binaries whose periods are relatively short and whose orbit determinations present no particular difficulty, unless it be that the stars concerned are rather faint (unusually faint, in fact, for the *Henry Draper Catalogue* — Miss Cannon must have been provided with a particularly good plate of that part of the sky!).

All four stars whose orbits are given here are in the southern part of Coma Berenices; the first three are all within about 2° of α Comae, while HD 116880 is about 5° north-following that star. HD 113997 is the only one of the four bright enough to be plotted in *Uranometria 2000*[2]. There is very little astrophysical information in the literature about any of the four. Most of what *is* available is in the survey paper[1], which is not retrievable through *Simbad*; for the relevant stars it is presented anew in Table I, which shows the (V, B) photometry, the spectral type and absolute magnitude derived from DDO-style[3] photometry, and the resulting estimate of the distance. The sum total of the information held by *Simbad* on the stars, apart from V and B magnitudes which appear to have come from *Tycho* photometry and are not to be expected to be of accuracy comparable with those in Table I, are photometry of HD 113997 by Häggkvist[4] ($V = 9^m \cdot 29$, $(B-V) = 1^m \cdot 10$, $(U-B) = 1^m \cdot 00$), a photometric estimate of $M_V = 0^m \cdot 737$ for that star, leading to a z distance of 510·9 pc, by Soubiran *et al.*[5], and photometry of HD 114931 by Oja[6], who gave $V = 10^m \cdot 35$, $(B-V) = 0^m \cdot 53$, $(U-B) = 0^m \cdot 01$.

TABLE I

NGP Survey[1] *results for the four stars*

Star	V m	$(B-V)$ m	*Type*	M_V m	z pc
HD 113997	9·29	1·10	K1 II–III	+0·2	643
HD 114931	10·35	0·53	F9 V	+4·2	165
HD 115588	9·71	0·65	G3 V	+4·8	94
HD 116880	9·81	0·84	K1 V	+6·1	54

Radial velocities and orbits

The only radial velocity that has been published for any of the four stars whose orbits are given here appears to be the $-38 \cdot 00$ km s^{-1} given by Soubiran *et al.*[5] for HD 113997; no date is given, so it cannot be incorporated into the discussion below.

By coincidence, the first observation of each of the four stars was made in the same year, 1973; in the case of the faintest star, HD 114931, it was made by the writer in collaboration with Dr. J. E. Gunn with the 200-inch Palomar telescope, but in the other three cases it was made by Mr. G. A. Radford (then a student) with the original radial-velocity spectrometer in Cambridge. The survey programme occupied a long time, and it was not until 1987 or 1988 that the binary natures of the stars were fully established and systematic radial-velocity monitoring was instituted. As far as the radial-velocity observations can show, all four objects are single-lined; the likelihood is that the secondaries are main-sequence objects a lot fainter than their respective primaries. Table II gives a breakdown of the numbers of measurements made with each of the six photoelectric radial-velocity spectrometers that have been used in the programme. The second column of the table gives the references where descriptions of the various instruments are to be found. The ESO instrument is a '*Coravel*' almost identical with the OHP[10] one; the Cambridge one is also a *Coravel*, which in many ways is very similar to the OHP one but has some differences and has not yet been the subject of a published description.

TABLE II

Sources of radial-velocity measurements of the four stars

Source	*Ref.*	*HD 113997*	*HD 114931*	*HD 115588*	*HD 116880*	*Totals*
Cambridge (old)	7	12	5	11	5	33
Palomar	8	—	2	1	—	3
DAO	9	1	1	1	1	4
OHP	10	26	23	29	23	101
ESO	—	1	2	2	2	7
Cambridge (new)	—	27	31	22	20	100
Totals	—	67	64	66	51	248

The observations are listed, star by star, in Tables III–VI. The Haute-Provence and ESO observations, initially reduced to the post-2000 zero point[11], have had the adjustment of $+0{\cdot}8$ km s^{-1} that has become routine in this series of papers applied to them in an effort to bring them onto the scale generally used here. In the case of HD 113997, where there is quite a good number of observations made with the original spectrometer, with which the Cambridge scale was set up[12], there is good systematic agreement between those observations and the adjusted Haute-Provence ones; for the other stars, all that can be said is that there are no significant discrepancies between the two sources. The Cambridge *Coravel* measurements of the K giant HD 113997 are also in systematic agreement; for the other, bluer stars, empirical adjustments of $-1{\cdot}0$, $-0{\cdot}4$, and $-0{\cdot}4$ km s^{-1} have, respectively, been made to such measurements.

TABLE III

Radial-velocity observations of HD 113997

Except as noted, the sources of the observations are as follows:
1973–1991 — original Cambridge spectrometer;
1992–1996 — Haute-Provence Coravel; 1997–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	*(O–C)* km s^{-1}
1973 Mar. 30·08	41771·08	−36·2	0·327	−0·4
1978 Mar. 31·03	43598·03	−33·9	1·245	+0·1
1987 Mar. 3·18*	46857·18	−30·2	2·881	−0·1
1988 Jan. 31·50†	47191·50	−29·8	3·049	−0·4
Mar. 11·15*	231·15	−29·7	·069	0·0
17·01*	237·01	−30·7	·072	−0·9
Apr. 13·97	264·97	−29·6	·086	+0·4
May 19·95	300·95	−29·8	·104	+0·6
June 6·91	318·91	−30·3	·113	+0·3
Nov. 6·22*	471·22	−32·4	·190	+0·2
1989 Feb. 11·19	47568·19	−33·5	3·238	+0·3
Mar. 25·11	610·11	−33·2	·260	+1·2

TABLE III (*continued*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O−C)* *km s⁻¹*
1989 Apr. 28·06*	47644·06	−35·1	3·277	−0·3
May 27·96	673·96	−35·6	·292	−0·5
1990 Jan. 31·16*	47922·16	−36·6	3·416	+0·3
Feb. 12·37‡	934·37	−37·3	·422	−0·3
Mar. 29·00	979·00	−37·5	·445	−0·4
Apr. 30·00	48011·00	−37·7	·461	−0·6
Dec. 27·23	252·23	−36·1	·582	+0·5
1991 Jan. 27·12*	48283·12	−36·6	3·597	−0·2
May 24·99	400·99	−35·6	·657	−0·2
1992 Jan. 19·26	48640·26	−32·9	3·777	−0·3
Apr. 27·09	739·09	−31·5	·826	−0·1
June 26·97	799·97	−30·0	·857	+0·6
Dec. 19·25	975·25	−28·6	·945	+0·6
1993 Feb. 12·21	49030·21	−28·6	3·973	+0·4
Mar. 25·05	071·05	−28·7	·993	+0·3
July 6·91	174·91	−29·7	4·045	−0·3
Dec. 28·25	349·25	−32·0	·133	−0·9
1994 Feb. 16·16	49399·16	−32·1	4·158	−0·4
May 1·06	473·06	−33·1	·195	−0·4
July 29·84	562·84	−33·6	·240	+0·3
Dec. 13·20	699·20	−35·9	·309	−0·5
1995 Jan. 3·20	49720·20	−35·4	4·319	+0·2
June 1·97	869·97	−36·1	·394	+0·6
Dec. 27·21	50078·21	−37·6	·499	−0·4
1996 Mar. 31·05	50173·05	−37·7	4·547	−0·8
1997 Mar. 6·16	50513·16	−34·1	4·717	0·0
Apr. 11·10	549·10	−33·7	·735	0·0
May 7·03	575·03	−34·3	·748	−0·9
July 27·85*	656·85	−32·9	·789	−0·6
1998 May 2·03*	50935·03	−29·9	4·929	−0·5
July 12·92*	51006·92	−28·6	·965	+0·4
1999 Dec. 29·26	51541·26	−33·7	5·234	0·0
2000 Apr. 6·04	51640·04	−34·4	5·283	+0·5
May 30·99	694·99	−36·6	·311	−1·1
2001 Jan. 7·26	51916·26	−36·4	5·422	+0·6
Feb. 17·17	957·17	−37·5	·442	−0·4
May 30·97	52059·97	−36·9	·494	+0·3
2002 Feb. 4·25	52309·25	−35·7	5·619	+0·4
Mar. 1·13	334·13	−35·2	·632	+0·7
Apr. 4·08	368·08	−35·7	·649	−0·1
May 7·97	401·97	−35·0	·666	+0·3
July 13·91	468·91	−33·7	·699	+0·8
Dec. 9·27	617·27	−32·7	·774	0·0
2003 Feb. 18·15	52688·15	−31·5	5·809	+0·3
Apr. 19·02	748·02	−31·1	·840	0·0
Oct. 22·91	934·91	−30·2	·933	−0·9

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2004 Jan. 15·24	53019·24	−28·7	5·976	+0·3
Mar. 17·14	081·14	−28·8	6·007	+0·2
May 7·02	132·02	−29·4	·032	−0·2
Dec. 27·29	366·29	−31·4	·150	+0·1
2005 Mar. 12·18	53441·18	−32·6	6·188	−0·1
May 7·96	497·96	−33·0	·216	+0·3
2006 Mar. 1·13	53795·13	−36·1	6·365	+0·3
Apr. 6·02	831·02	−36·0	·383	+0·6
June 11·95	897·95	−36·9	·417	0·0

*Observed with Haute-Provence *Coravel*.
†Observed with DAO 48-inch telescope.
‡Observed with ESO *Coravel*.

TABLE IV

Radial-velocity observations of HD 114931

Except as noted, the sources of the observations are as follows:
1973–1998 — Haute-Provence Coravel (weighted 1/2 in orbital solution);
2000–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
1973 June 12·20*	41845·20	+7·1	0·236	−0·5
1978 May 23·34*	43651·34	+10·5	1·079	−0·6
1984 Apr. 28·01†	45818·01	+11·4	2·089	+0·5
1987 Mar. 3·21	46857·21	+1·7	2·574	−0·9
1988 Feb. 1·50‡	47192·50	+1·0	2·731	+0·4
Mar. 12·08	232·08	+0·7	·749	+0·4
17·03	237·03	+0·4	·751	+0·1
Apr. 13·98†	264·98	+0·3	·764	+0·2
1989 Feb. 24·27§	47581·27	−0·2	2·912	+0·7
Mar. 25·12†	610·12	−0·5	·925	+0·1
Apr. 29·06	645·06	+0·1	·942	+0·1
May 2·99	648·99	−1·1	·943	−1·2
27·97†	673·97	+0·7	·955	−0·1
1990 Jan. 31·17	47922·17	+10·8	3·071	−0·4
Feb. 12·37§	934·37	+11·3	·077	+0·2
1991 Jan. 27·13	48283·13	+8·1	3·239	+0·6
Feb. 6·19	293·19	+6·0	·244	−1·4
May 9·99†	385·99	+8·0	·287	+1·4
1992 Apr. 27·11	48739·11	+5·7	3·452	+1·6
June 26·98	799·98	+2·7	·480	−1·1
1993 Feb. 15·17	49033·17	+2·4	3·589	0·0
Mar. 25·06	071·06	+1·7	·607	−0·5
July 11·93	179·93	+1·0	·658	−0·5

TABLE IV (*concluded*)

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O−C) km s⁻¹
1994	Jan.	3·21	49355·21	+1·7	3·739	+1·2
	Feb.	21·14	404·14	+0·7	·762	+0·5
	May	1·07	473·07	−1·8	·794	−1·6
	Aug.	4·88	568·88	−1·8	·839	−1·1
1995	Jan.	3·22	49720·22	−2·8	3·910	−1·9
	June	1·98	869·98	+4·2	·980	+0·7
1996	Mar.	31·06	50173·06	+11·0	4·121	+0·8
1997	Apr.	11·11¶	50549·11	+5·7	4·296	−0·8
	July	25·86	654·86	+6·5	·346	+0·8
1998	May	2·04	50935·04	+3·4	4·476	−0·4
	July	12·94	51006·94	+4·0	·510	+0·6
2000	Mar.	4·18	51607·18	−0·6	4·790	−0·4
	Apr.	7·09	641·09	−0·4	·806	0·0
2001	Jan.	14·22	51923·22	+0·1	4·937	+0·3
	Feb.	14·15	954·15	−0·4	·952	−1·0
		27·20	967·20	+0·9	·958	−0·2
	Mar.	9·05	977·05	+3·4	·962	+1·9
	May	5·04	52034·04	+4·1	·989	−0·8
		29·01	058·01	+7·4	5·000	+0·7
	June	7·96	067·96	+5·9	·005	−1·5
		27·96	087·96	+9·3	·014	+0·6
	July	4·94	094·94	+9·8	·017	+0·8
		10·92	100·92	+8·9	·020	−0·4
2002	Jan.	31·23	52305·23	+11·1	5·115	+0·8
	Feb.	21·21	326·21	+9·0	·125	−1·1
	Apr.	7·09	371·09	+9·6	·146	0·0
	May	22·96	416·96	+9·5	·168	+0·5
	Dec.	9·27	617·27	+6·7	·261	−0·4
2003	Jan.	11·26	52650·26	+6·3	5·276	−0·5
	Mar.	3·14	701·14	+6·4	·300	0·0
	Apr.	29·08	758·08	+5·8	·327	−0·2
	May	20·02	779·02	+6·1	·336	+0·3
	June	20·95	810·95	+5·4	·351	−0·2
2004	Apr.	15·07	53110·07	+4·1	5·491	+0·5
	June	21·96	177·96	+2·5	·523	−0·7
2005	Jan.	23·23	53393·23	+1·5	5·623	−0·4
	May	7·97	497·97	+2·2	·672	+0·9
2006	Mar.	1·14	53795·14	+0·4	5·810	+0·8
	Apr.	6·04	831·04	−0·6	·827	0·0
	May	29·96	884·96	−0·9	·852	0·0
	June	28·94	914·94	−1·2	·866	−0·2

*Observed with Palomar 200-inch telescope.
†Observed with original Cambridge spectrometer.
‡Observed with DAO 48-inch telescope.
§Observed with ESO *Coravel*.
¶Observed with Cambridge *Coravel*.

TABLE V

Radial-velocity observations of HD 115588

Except as noted, the sources of the observations are as follows:
1973–1991 — original Cambridge spectrometer (weighted 1/2 in orbital solution);
1992–1996 — Haute-Provence Coravel; 1997–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s^{-1}*	*Phase*	*(O – C)* *km s^{-1}*
1973 Mar. 31·08	41772·08	−14·4	0·309	+0·6
1978 May 23·36*	43651·36	−18·3	6·282	−0·4
1986 Apr. 11·03†	46531·03	−4·4	15·435	+0·9
May 15·94	565·94	−0·4	·546	+1·2
1987 Jan. 31·21	46826·21	−9·6	16·373	−0·3
Feb. 21·16	847·16	−5·5:	·440	−0·5
Mar. 3·12†	857·12	−4·7	·472	−1·2
May 7·93	922·93	−3·4	·681	+0·5
1988 Jan. 31·46‡	47191·46	−1·8	17·534	−0·1
Mar. 12·10†	232·10	−2·2	·663	+0·9
17·03†	237·03	−2·3	·679	+1·5
Apr. 13·05	264·05	−9·6	·765	+0·6
May 29·99	310·99	−33·4	·914	−1·1
1989 Feb. 24·27§	47581·27	−10·9	18·773	+0·2
Mar. 25·13†	610·13	−23·5	·865	+0·4
Apr. 28·07†	644·07	−40·3	·973	+0·7
May 2·99†	648·99	−41·9	·989	+0·7
27·97	673·97	−41·7	19·068	+1·9
1990 Jan. 31·17†	47922·17	−23·6	19·857	−1·0
Feb. 12·38§	934·38	−30·0	·896	−0·9
Mar. 29·03	979·03	−44·1	20·038	+0·5
Apr. 30·98	48011·98	−38·2	·142	−2·2
1991 Jan. 26·18†	48282·18	−45·1	21·001	−1·5
29·19†	285·19	−43·3	·011	+0·8
Feb. 4·13†	291·13	−45·0	·030	−0·4
May 10·00	386·00	−12·3	·331	+0·6
1992 Jan. 20·22	48641·22	−35·4	22·142	+0·6
Apr. 24·06	736·06	−5·3	·444	−0·5
June 25·96	798·96	−3·1	·644	−0·7
1993 Feb. 18·13	49036·13	−7·2	23·398	+0·3
Mar. 18·14	064·14	−3·0	·487	0·0
25·07	071·07	−0·9	·509	+1·4
July 7·95	175·95	−19·5	·842	+0·7
1994 Jan. 3·22	49355·22	−8·2	24·412	−1·6
Feb. 21·14	404·14	−1·0	·567	+0·4
May 1·07	473·07	−13·3	·786	−0·7
Dec. 13·22	699·22	−3·0	25·505	−0·6
1995 Jan. 3·23	49720·23	−0·8	25·572	+0·6
June 6·98	874·98	−45·3	26·064	−1·5
1996 Mar. 31·08	50173·08	−45·0	27·011	−0·9
1997 Mar. 27·04	50534·04	−32·9	28·159	+0·9
29·11	536·11	−32·9	·165	0·0

TABLE V (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O−C)* *km s⁻¹*
1997 Apr. 1·11	50539·11	−32·1	28·175	−0·5
8·04	546·04	−28·9	·197	−0·3
11·11	549·11	−27·1	·207	+0·2
14·97	552·97	−25·1	·219	+0·5
18·08	556·08	−25·3	·229	−1·0
25·05†	563·05	−20·9	·251	+0·6
28·08†	566·08	−19·3	·261	+1·1
May 1·02	569·02	−20·2	·270	−0·9
July 25·86†	654·86	−2·3	·543	−0·7
1998 July 9·96†	51003·96	−3·5	29·652	−0·8
2000 Apr. 7·12	51641·12	−4·2	31·678	−0·5
2001 Feb. 27·20	51967·20	−7·1	32·714	−1·3
2002 Feb. 27·14	52332·14	−24·3	33·874	+1·1
Apr. 4·11	368·11	−41·1	·988	+1·5
May 7·93	401·93	−42·7	34·096	−1·3
16·01	410·01	−37·8	·121	+0·8
2003 Jan. 27·18	52666·18	−36·5	34·936	−0·6
Mar. 19·04	717·04	−40·3	35·097	+0·9
May 18·02	777·02	−17·2	·288	0·0
2004 Mar. 30·11	53094·11	−17·2	36·296	−0·8
Apr. 20·06	115·06	−8·7	·362	+1·4
July 2·94	188·94	−1·5	·597	0·0
2006 May 31·02	53886·02	−15·8	38·813	+0·2
June 11·95	897·95	−21·5	·851	+0·1

*Observed with Palomar 200-inch telescope.
†Observed with Haute-Provence *Coravel*.
‡Observed with DAO 48-inch telescope.
§Observed with ESO *Coravel*.

TABLE VI

Radial-velocity observations of HD 116880

Except as noted, the sources of the observations are as follows:
1973–1996 — Haute-Provence Coravel; 1997–2006 — Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O−C)* *km s⁻¹*
1973 Mar. 30·13*	41771·13	−7·4	0·963	+0·9
1987 Mar. 1·06	46855·06	+6·1	43·198	+0·7
1988 Feb. 1·53†	47192·53	−3·1	46·001	+0·1
Mar. 13·13	233·13	−3·3	·339	−0·1
1989 Mar. 27·08	47612·08	−13·8	49·487	+0·3
Apr. 30·00	646·00	−24·6	·769	+0·1
May 1·96	647·96	−24·5	·785	−0·2
June 1·93*	678·93	+5·1	50·042	+3·6
1990 Jan. 27·16	47918·16	+1·0	52·029	+0·8
31·18	922·18	+3·4	·063	0·0

TABLE VI (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1990 Feb. 12·39‡	47934·39	+6·1	52·164	−0·3
14·39‡	936·39	+6·3	·181	+0·3
Mar. 29·05*	979·05	−18·3	·535	−1·0
Apr. 5·01*	986·01	−21·6	·593	−1·0
May 1·00*	48012·00	−25·7	·809	−2·2
1991 Jan. 26·19	48282·19	+1·8	55·054	−0·8
Feb. 4·14	291·14	+6·5	·128	+0·1
1992 Jan. 20·23	48641·23	+0·5	58·036	−0·4
Apr. 24·07	736·07	−22·6	·824	+0·1
June 25·97	798·97	−4·6	59·347	−0·8
1993 Feb. 11·19	49029·19	+2·6	61·259	+0·3
18·15	036·15	−2·2	·317	−0·5
Mar. 18·14	064·14	−18·4	·550	−0·2
July 7·96	175·96	−12·8	62·479	+0·8
1994 Jan. 8·18	49360·18	−1·7	64·009	+0·5
Feb. 21·15	404·15	−5·8	·374	+0·1
May 2·06	474·06	−10·3	·955	−0·9
1995 Jan. 3·25	49720·25	−2·5	67·000	+0·8
June 6·02	874·02	+0·8	68·278	−0·3
1996 Mar. 31·10	50173·10	−25·0	70·762	−0·2
1997 Mar. 27·05	50534·05	−24·4	73·761	+0·4
Apr. 8·05	546·05	−20·2	·861	0·0
11·12	549·12	−17·4	·886	+0·4
14·98	552·98	−14·6	·918	−0·4
18·08	556·08	−11·3	·944	−0·4
1998 July 7·93§	51001·93	−23·7	77·648	−0·7
2000 Jan. 9·26	51552·26	+4·1	82·220	−0·4
Mar. 25·12	628·12	−20·7	·850	+0·4
Apr. 22·03	656·03	+4·5	83·082	−0·2
2001 Jan. 7·29	51916·29	+3·1	85·244	−0·1
Mar. 3·19	971·19	−24·0	·700	+0·5
2002 Feb. 27·15	52332·15	−24·3	88·699	+0·2
May 29·02	423·02	−11·5	89·454	+0·3
2003 Mar. 23·11	52721·11	−12·8	91·930	−0·1
May 20·03	779·03	−8·8	92·411	−0·1
24·03	783·03	−10·2	·444	+0·9
June 14·97	804·97	−23·1	·627	−0·9
2004 Apr. 15·08	53110·08	+6·4	95·161	0·0
22·10	117·10	+4·7	·220	+0·2
June 4·98	160·98	−20·1	·584	0·0
2006 May 21·99	53876·99	−17·5	101·532	−0·4

*Observed with original Cambridge spectrometer; zero-weighted in orbital solution.
†Observed with DAO 48-inch telescope.
‡Observed with ESO *Coravel*.
§Observed with Haute-Provence *Coravel*.

For HD 113997 all sources have been weighted equally in the solution of the orbit. For HD 114931 the Haute-Provence data, which have some bad residuals possibly arising in part from inadequate integrations attributable to reprehensible impatience on the part of the observer, have merited weight 1/2. In the case of HD 115588 it is the 'original Cambridge' observations that have warranted down-weighting to 1/2; all three major sources of velocities seem unusually ragged on that star. Finally, for HD 116880 there are only five 'original Cambridge' measures, two of which give very bad residuals (the worst one has been re-checked *ab initio* from the original chart record but no mistake could be identified), and the unusually draconian decision has been taken to reject the whole lot rather than to pick and choose between them; all the other sources have received equal weights. With the zero-points and weightings just described, the velocities readily yielded the orbital elements that are set out in Table VII; the velocity curves together with the underlying data are plotted in Figs. 1–4.

TABLE VII

Orbital elements for the four stars

Element	*HD 113997*	*HD 114931*	*HD 115588*	*HD 116880*
P (days)	1991 ± 5	2144 ± 7	314·61 ± 0·05	120·373 ± 0·009
T (MJD)	49085 ± 88	49914 ± 11	49540·3 ± 1·9	50081·3 ± 0·7
γ (km s^{-1})	−33·41 ± 0·06	+4·22 ± 0·10	−18·91 ± 0·11	−10·13 ± 0·08
K (km s^{-1})	4·09 ± 0·09	6·14 ± 0·18	21·63 ± 0·17	15·72 ± 0·10
e	0·080 ± 0·021	0·551 ± 0·018	0·201 ± 0·007	0·162 ± 0·007
ω (degrees)	6 ± 16	284·9 ± 3·3	161·2 ± 2·4	291·8 ± 2·3
$a_1 \sin i$ (Gm)	111·6 ± 2·5	151 ± 5	91·7 ± 0·7	25·67 ± 0·17
$f(m)$ ($M_\odot$)	0·0140 ± 0·0009	0·0299 ± 0·0029	0·311 ± 0·007	0·0466 ± 0·0009
R.m.s. residual (wt. 1) (km s^{-1})	0·48	0·67	0·83	0·45

Discussion

For HD 113997 the observations span six circuits of the 5 1/2-year orbit, defining the period with an uncertainty of only 5 days; the odd half-year in the period means that the observing seasons in one cycle make good the inter-season gaps in the adjacent ones, facilitating good phase coverage. At first sight the velocity curve looks like a sine wave, owing to its low eccentricity and the fact that $\omega \sim 0°$. A sine wave does in fact fit tolerably, but raises the sum of squares of the residuals of the 68 observations from 15·70 to 18·90 (km s^{-1})^2. Bassett's[13] second test (whose application in such a context was last explained reasonably fully in Paper 176[14]) yields from those figures a variance ratio $F_{2,62} \sim 6·32$, which is easily significant at 1% ($F \sim 4·97$) although it does not reach the 0·1% level (7·76), so the eccentric solution is surely to be preferred.

HD 114931 has a period slightly longer than that of HD 113997; it is a specially faint star for the *Henry Draper Catalogue* and gives poor dips on radial-velocity traces owing to its relatively early type, and its velocities are therefore rather ragged, but the period determination is not bad (standard deviation 7 days) — the higher eccentricity, which creates a phase of relatively rapid change of velocity, largely makes up for the imprecision of the data.

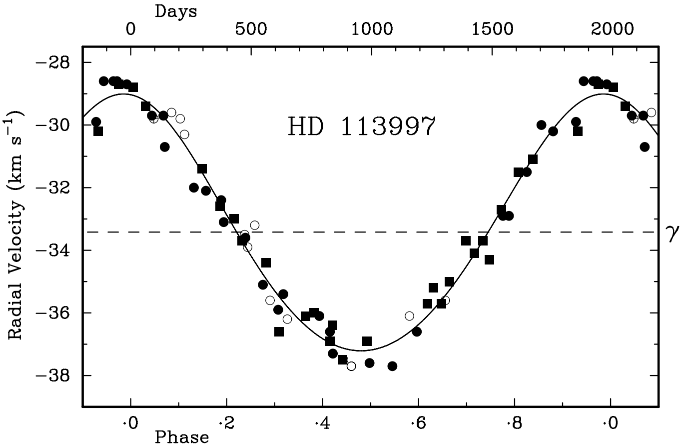

FIG. 1

The observed radial velocities of HD 113997 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Filled symbols represent *Coravel* observations, circles for OHP (and also for one ESO velocity), and squares for Cambridge. Open circles indicate measurements made with the original radial-velocity spectrometer at Cambridge; one such circle with a cross in it (half-hidden near phase 100 days) identifies a DAO measure. All sources of velocities were weighted equally in the solution of the orbit.

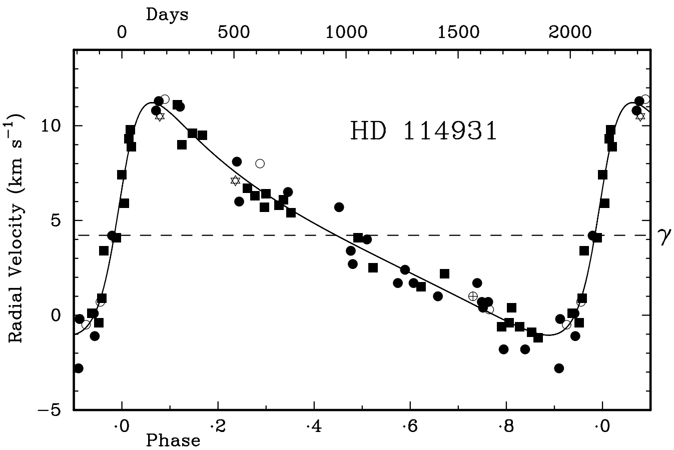

FIG. 2

As Fig. 1, but for HD 114931. This diagram includes two measurements, identified by being plotted as open stars, made with the 200-inch telescope at Palomar. The different data sources were weighted equally in the orbital solution, save that the OHP velocities received weight $^1/_2$.

Of course the much shorter periods of the other two stars are determined very much more accurately. It may be of interest to rehearse explicitly — as we proceed to do here — the manner in which the period and eccentricity of an orbit affect

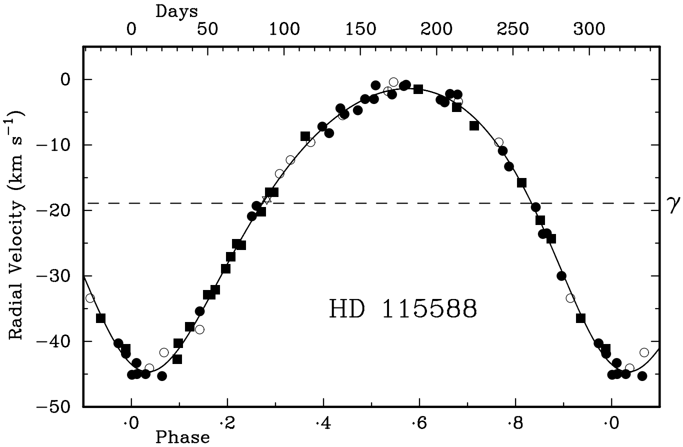

FIG. 3

As Figs. 1 and 2, but for HD 115588. The 'original Cambridge' data were half-weighted in the solution, all the other sources having unit weight.

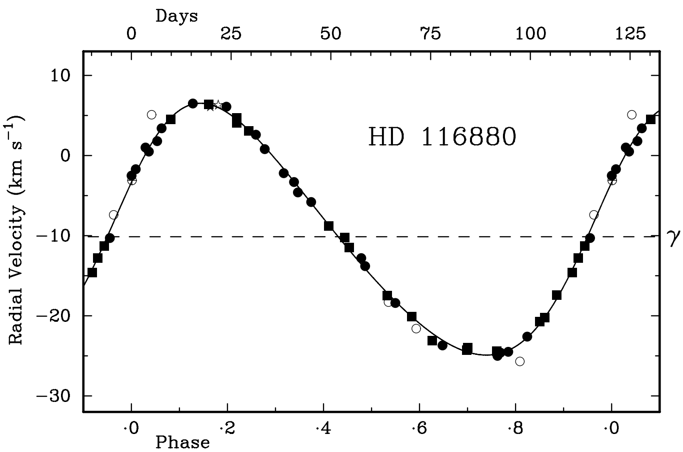

FIG. 4

As Fig. 3, but for HD 116880 and with the 'original Cambridge' data zero-weighted.

the precision with which its period is determined. Consider two stars with orbits of similar form and amplitude and having periods in the ratio of n to 1, and receiving mutually analogous sets of radial-velocity observations over the same total interval of time. The one with the shorter period will change its velocity n times more rapidly than the other, and hence each observation establishes its phasing n times more accurately. Also, it will be seen round n times as many cycles as the other and its period will therefore be determined n times as accurately in comparison with the length of that period, or n^2 times as accurately in terms of days. We have then to recall that Kepler's laws show that velocity amplitudes go as the inverse cube root of the period, so in analogous stellar systems the mean rate of change of velocity in the system of shorter period can therefore actually be expected to be not n times faster than in the long-period case (as we initially took it to be on the basis of equal velocity amplitudes) but $n^{4/3}$ times as great, leading finally to a difference in period-accuracy of $n^{7/3}$. That makes it easy to see why, for example, the period of HD 116880 is determined about 600 times more accurately (0·008 days) than that of HD 113997 (5 days), since the periods differ by a factor of about 17, and $17^{7/3} \sim 700$. The high power of the period makes it easy to understand how, for example, the periods of pulsars can soon be determined to precisions of femtoseconds.

The benefit of a high eccentricity, as well as a short orbital period, in assisting an accurate period determination is not to be under-estimated. Near the periastron passage there will be phases at which the velocity changes much more rapidly if the orbital eccentricity is high than if it is low. Radial-velocity measurements at phases of rapid change define those phases with accuracies directly proportional (*i.e.*, uncertainties inversely proportional) to the rate of change. By way of a concrete example, we may consider an orbit whose longitude of periastron is 90° — the velocity curve consists of a relatively gentle rise and a steep fall, with the maximum rate of change just halfway down the descent. In comparison with the slope of the velocity variation in a circular orbit ($e = 0$), the maximum rate is increased by factors of 2, 5, and 10 at eccentricities of about 0·3, 0·6, and 0·74, and continues to rise more and more rapidly at still higher eccentricities; it is already up to about 43 at $e = 0{\cdot}9$.

No evidence has been seen of a second dip in the radial-velocity trace of any of the four stars. That is a very usual situation in the cases of giant stars such as HD 113997, but main-sequence objects such as the other three discussed here are apt to appear double-lined if the masses are not too disparate. We could estimate the masses of the primary stars in HD 114931, 115588, and 116880, whose types (see above) are about F9, G3, and K1, at about 1·1, 1·0, and 0·8 $M_\odot$, respectively. Then from the mass functions we can estimate the minimum masses of the companions, which come out at about 0·4, 1·1, and 0·4 $M_\odot$. In the cases of HD 114931 and 116880, those values offer no contradiction of the hypothesis that the companions are simply too faint in relation to their primaries to be detectable, but (as is in fact obvious at sight from the extraordinary mass function of 0·31 $M_\odot$) the result for HD 115588 is bound to cause some excitement.

For that star, the secondary has to be, as a minimum, more massive than the primary! It clearly cannot be just another main-sequence star. It might be a white dwarf, but in that case it would necessitate some special pleading to explain how the system came to be left, after the red-giant evolution of the companion, in an orbit with significant eccentricity despite a period of less than a year and a separation of little more than 1 AU. A more attractive option is to postulate, as has been necessary in certain previous instances in this series of papers, that the companion is itself a binary system. There would be no difficulty in accepting that the system includes a pair of main-sequence stars, not too unequal, with masses each not much more than half the overall minimum, *viz.*, not much above 0·55 $M_\odot$, constituting the secondary object — secondary in terms of luminosity though not of mass. Such a secondary would not necessarily make the luminosity or colour indices of the total system particularly unusual. Although no extra dip has been explicitly seen in radial-velocity traces, despite several deliberate efforts involving unusually long integrations and appropriate scan widths near times of nodal passages, we might notice that the r.m.s. residual of the velocities is worse for HD 115588 than for any but the most difficult objects. For example, it is worse than the value given here for HD 114931, a star that is only half as bright and, apparently being of earlier type, ought to give a shallower dip. Actually the dip given by HD 115588 is much the same as that of HD 114931; a possible interpretation is that the two stars are in fact of much the same type but the former is made to appear later in its colours and other photometric indices by the admixture of the light of two much later (though fainter) stars. Analogously, the excess scatter in the radial velocities could be a result of the blending with the principal dip of the exiguous feature(s) stemming from one or both of the putative sub-components. Indeed, the fact that the author's observing list in recent years has had an admonitory note against HD 115588 inviting extra integration time, but without obvious benefit in terms of reduced residuals, could be seen as a pointer in that direction.

HD 113995

41 Comae Berenices served as the initial fundamental reference star for photoelectrically determined radial velocities when such observations were first begun[7] in Cambridge in 1966. It was selected because it seemed at the time to satisfy the criteria[12] adopted for the choice of standard stars, and it happened to be just about at opposition at the time of year (late winter) when the observations began. Quite by chance it is very close to the NGP ($b \sim 86^{\circ}\cdot 5$) and so formed a very convenient reference star, within its limitations, when the NGP survey was undertaken. Its *HD* number is 113996. In view of the writer's concern with spectroscopic binaries, it seemed rather amusing when, later on, the succeeding star, HD 113997, turned out to be such a system, and even more so when 41 Com's other neighbour, HD 113995, proved to be not merely a binary but actually double-lined! Since it seems most unlikely that the present author will ever be able to delineate the orbit of HD 113995, he takes the opportunity presented by this Paper 194, which gives the orbit of HD 113997, to offer a progress report. HD 113995 is brighter than the four stars treated above, and (unlike them) it was observed by *Hipparcos*, which determined its parallax to be $0''\cdot 00248 \pm 0''\cdot 00119$. That value corresponds to a distance modulus of $8^{m}\cdot 0$, but since it is scarcely more than twice its standard error all that it really does is to demonstrate that the star is far above the main sequence and therefore is in all probability a normal giant (supergiants not being expected at high galactic latitudes).

In 1964 Harris & Upgren[15] gave the *UBV* magnitudes of HD 113995 as $V = 8^{m}\cdot 40$, $(B-V) = 1^{m}\cdot 37$, $(U-B) = 1^{m}\cdot 54$. (*Simbad*, remarkably, attributes to that paper colour indices given to three decimal places, but the paper gives only two.) Several years previously, Stock & Wehlau[16] had thanked D. L. Harris for the use of unpublished photoelectric photometry, and had plotted his $(B-V)$ colours against their own photographically determined $(B-I)$; the point representing HD 113995 can be identified uniquely and its $(B-V)$ read off the graph as $1^{m}\cdot 37$. Stock & Wehlau gave the type of the star, on the basis of spectra obtained with the 4° objective prism on the '*Cleveland Schmidt*'* as K2 III–IV. Still in the 1960s, two members[18] of the Haute-Provence objective-prism syndicate classified

*Yet another name for the Warner & Swasey/Case/Burrell Schmidt that was described in a major footnote in Paper 167[17].

it as K2 III and gave its radial velocity as -9 km s^{-1}, with 'quality B' (meaning 'probable error' between 2·5 and 5 km s^{-1}), as the mean of six measurements. Thirty-four years later, the syndicate published[19] a systematic correction of -4 km s^{-1} for all stars measured in the relevant area (Selected Area[20] 57), bringing to -13 km s^{-1} the corrected velocity for HD 113995. Heard[21] gave a spectral type of K3 III and a mean velocity of $-23·9$ km s^{-1}, with a 'probable error' of 1·7 km s^{-1}, from four slit spectra obtained at 66 Å mm^{-1} with a Cassegrain spectrograph on the DDO 74-inch reflector. Tchipashvili[22] included the star (as 28° 283, with a type of K5 III) in a very large programme of photographic photometry and objective-prism spectroscopy in the NGP field, and Schild[23], who obtained a slit spectrogram at the McDonald 82-inch with a prism spectrograph giving 87 Å mm^{-1} at Hδ, listed its type as K4 III.

Between 1982 and 1997 Yoss and his collaborators included HD 113995 in no fewer than three investigations that they made of stars in the NGP field; in each case they used DDO-style narrow-band-filter photometry[3] to estimate spectral types and luminosities for the stars on their programmes. First Hartkopf & Yoss[24] listed the 'DDO type' of HD 113995 as K4 III, the absolute magnitude as $+1^{m}·4$, and the distance as 251 kpc*. They gave the *W* velocity (or z-motion, perpendicular to the plane of the Galaxy), as $-10·5$ km s^{-1}, implying a radial velocity of about -16 km s^{-1}. Later, Yoss, Neese & Hartkopf[25] offered two radial-velocity measurements and slightly revised the absolute magnitude to $+1^{m}·3$ and correspondingly the distance to 262 kpc*, and the *W* motion to -8 km s^{-1}; and finally Yoss & Griffin[1] gave the DDO type again as K4 III, M_V $+1^{m}·3$, distance 262 pc, radial velocity $-9·2 \pm 0·8$ km s^{-1} as the mean of five measurements, and *W* motion -1 km s^{-1}.

A few other mentions of HD 113995 are to be found in the literature. Latham *et al.*[26], who had been using mutually similar spectrometers[27] on three different telescopes to make a search for wide binaries in the NGP field (but did not consider HD 113995 to be a member of such a system), gave a radial velocity of $-11·99 \pm 0·20$ km s^{-1} as the mean of no fewer than 13 measurements spread over more than 8 years, but at the time they did not notice any variation; they also gave a distance of 262 pc and a type, classified from a 39-Å mm^{-1} plate taken with the Kitt Peak coudé feed, of K4 III. Janulis[28] used Vilnius photometry[29] in an investigation of Selected Area[20] 57, in which he listed HD 113995 as no. 17, and found the type to be K3 III, $M_V = +0^{m}·6$, and the distance to be 340 pc. Finally, Famaey *et al.*[30] included HD 113995 in a large table concerning late-type giants and gave for it a mean radial velocity of $-10·00 \pm 0·30$ km s^{-1} and a distance of 403·8 ± 100·7 pc; their velocity was probably derived partly, possibly wholly, from the present writer's own observations on the data base of measurements made with the OHP *Coravel*.

A radial-velocity measurement of HD 113995 was made in Cambridge in 1970, and another in 1973. The next one was not obtained until 1989, with the Haute-Provence *Coravel*; another was taken with that instrument in 1992, and a third in 1994, at which time there was found to have been a clear change in the velocity. The star was then observed more attentively, and proved to be double-lined: the velocity excursion was towards negative velocities, and there was seen to be a weak secondary dip blended in the red wing of the principal one. The episode did not last long in relation to the overall time-scale of HD 113995's activities, and by 1997 it was over and the velocity had returned nearly to its previous level. Since

*There is presumably a misprint in the column heading in the relevant table.

then the star has of course been carefully watched, but there has been no sign of any further change. The measurements are set out in Table VIII, and are plotted directly against time in Fig. 5.

TABLE VIII

Radial-velocity measurements of HD 113995

UT Date	*Velocity km s⁻¹*	*UT Date*	*Velocity km s⁻¹*	*UT Date*	*Velocity km s⁻¹*
1970 Mar. 31·11⋆	−9·2	1997 Mar. 3·11‡	−14·0	2002 Feb. 27·11‡	−10·3
		Apr. 10·06‡	−13·9	Apr. 7·07‡	−10·1
1973 May 23·07⋆	−9·6	May 5·06‡	−14·2	May 4·06‡	−10·3
		July 24·86†	−13·1	July 13·91‡	−10·3
1989 May 3·03†	−11·9	Sept. 9·79†	−13·1		
		Dec. 24·23†	−12·9	2003 Feb. 18·14‡	−10·3
1992 Apr. 30·05†	−13·0			May 10·00‡	−9·5
		1998 Apr. 12·03†	−12·1		
1994 May 4·07†	−15·7	July 9·91†	−11·9	2004 Apr. 23·04‡	−9·6
Dec. 13·20†	−16·7				
		1999 Dec. 27·28‡	−10·9	2005 May 9·01‡	−9·4
1995 Jan. 3·20†	−15·8			July 17·91‡	−9·4
June 6·95†	−18·1	2000 Apr. 6·03‡	−10·8		
		June 19·92‡	−10·3	2006 Jan. 29·23‡	−9·9
1996 Jan. 1·22†	−17·4			June 9·96‡	−9·5
Apr. 2·10†	−18·8	2001 Jan. 16·24‡	−11·1		
		Mar. 3·14‡	−10·4	2007 Feb 15·18‡	−10·1

Sources: ⋆Original Cambridge spectrometer †OHP *Coravel* ‡Cambridge *Coravel*

It is difficult to believe, when one looks at Fig. 5, that there is room for another event similar to the one so clearly shown in the 1990s to have occurred in the unobserved interval around 1980, or that the first (1970) observation could have been made any earlier in orbital phase than would correspond to about 2004, so the minimum period may be put at about 34 years (about 12400 days). It could be longer than that, to any degree. If only we knew the orbital period we would be able to determine all the other elements of the orbit quite reliably. The mean velocity reported by Heard[21] from the DDO is plenty low enough to have marked a previous periastron passage; Dr. C. T. Bolton has very kindly retrieved the dates and individual values of the four velocities, which were obtained in 1955–1959, but their scatter is such that one could not claim with any confidence that they really document a periastron.

Orbits based on different periods naturally have a strong family resemblance to one another, the main differences being in eccentricity, which reflects the proportion of the total period that is occupied by the velocity excursion marking the periastron passage. Thus the epoch of periastron remains always close to MJD 50130 (mid-February 1996) with an uncertainty of about six weeks, the longitude of periastron varies scarcely at all from 197° ± 4°, and the velocity (semi-) amplitude is always a little over 4 km s^{-1}. The eccentricity ranges from 0·69 for the minimum period to 0·77 at 20 000 days, 0·87 at 50 000 and 0·92 at 100 000 days; it can be represented within two or three thousandths by the empirical relationship (which nevertheless looks as if it might owe something to Kepler's Laws) $(1 - e) = 170/P^{2/3}$. There are no significant period-related differences in the good-

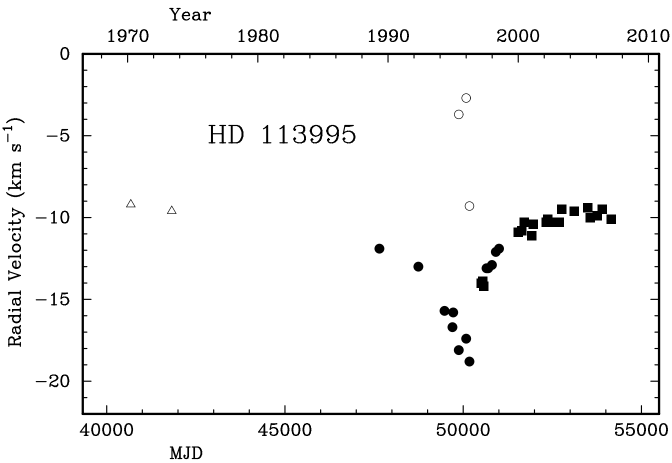

FIG. 5

Radial-velocity measurements of HD 113995, plotted directly against time. The two triangles in the early years represent velocities obtained with the original Cambridge spectrometer; all the other points were obtained with *Coravel* instruments, circles for OHP and squares for Cambridge. The open circles indicate velocities derived for the secondary from non-optimal reductions of three of the traces obtained near the nodal passage of 1995/6; regrettably they cannot be used to obtain a reliable estimate of the mass ratio.

ness of fit of the orbit to the data points, but very long periods are statistically unlikely since the probability of a periastron passage being observed within the duration of the observations would be small. The star needs to be monitored systematically, not necessarily more often than once per observing season, until the next periastron passage is witnessed.

Fig. 5 shows, in addition to the radial-velocity measurements of the primary star, three points that represent velocities of the secondary star near the time of nodal passage in 1996. Although in a general way they indicate that the velocity of the secondary varied in anti-phase with that of the primary, the reductions of the relevant records, which were obtained with the OHP *Coravel*, are not satisfactory. Good double-lined reductions of blended traces are not obtainable from such records without manual intervention. Unfortunately it is no longer possible to obtain new reductions of OHP data, which are stored at the Geneva Observatory in a data base to which the writer has no access. For the same reason it is not possible to present a figure illustrating the nature of radial-velocity traces obtained at the nodal phase. It can, however, be reported qualitatively that the secondary appeared as a weak feature blended into the red wing of the primary dip. It seems unlikely that it would be another late-type giant, or rather sub-giant, much fainter than the primary; most likely it is a star in the middle of the main sequence, somewhere within the F types, making the system rather similar to that of, *e.g.*, β LMi, which showed a somewhat analogous line profile during a nodal passage some years ago.

Dr. D. W. Latham, who refereed this paper, has been kind enough to inform the writer of his renewed interest in HD 113995, of which he has many radial-velocity measurements that begin at about the time of nodal passage; naturally they confirm the variation seen since then in Fig. 5. New reductions of the 13 earlier velocities, which had been obtained during the interval MJD 45000–48000 and reported[26] simply as a mean value, indicate that they do confirm the slow decline that is implied for that epoch by Fig. 5. Dr. Latham is also able to derive information about the secondary star, and will present his findings independently in due course.

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CORRESPONDENCE

To the Editors of 'The Observatory'

Mercury's False Dawn Revisited

My letter[1] on the double sunrise on Mercury generated some interest, but it fell to the sharp eye of a former Editor of this magazine, Professor Roger Griffin, to whom I am most grateful, to spot a few mistakes. Rather than embarrass myself by listing them, it seems wisest to start afresh with a detailed numerical calculation. The basic cause of the phenomenon of the reversal of motion of the Sun in Mercury's sky was correct, namely that, briefly near the perihelion of Mercury's orbit, Mercury's angular speed around the Sun is greater than its rotational speed around its axis, and at that time the Sun moves across the sky from west to east instead of the usual east to west. The quantitative details in my letter and the formula (which was "not difficult to show") for the true anomaly where reversal of apparent motion occurs were not correct.

The calculation is somewhat simplified by the circumstance that Mercury's obliquity is very close to zero. I do the calculations here for a point on Mercury's equator. The orbital angular speed ω of Mercury around the Sun when it is at a distance r from the Sun is given by

$$\omega = \frac{\sqrt{GMl}}{r^2} . \qquad (1)$$

Here a and l are the semi major axis and semi latus rectum of the orbit, G is the gravitational constant, and M the mass of the Sun. For Mercury,

$$\sqrt{GMl} = 2 \cdot 7168 \times 10^9 \text{ km}^2 \text{ s}^{-1}. \qquad (2)$$

We can find r as a function of time from the usual equations of planetary motion:

$$M = \frac{2\pi}{P} (t-T), \qquad (3)$$

$$E = M + e \sin E, \qquad (4)$$

$$\cos v = \frac{\cos E - e}{1 - e \cos E}, \qquad (5)$$

$$r = \frac{a(1-e^2)}{1 + e \cos v}. \qquad (6)$$

If r is calculated in km and substituted into equation (1) with (2), we obtain the orbital angular speed in rad s^{-1}, which can readily be converted into arcmin min^{-1}. The spin angular speed of Mercury, Ω, is almost (apart from a very small physical libration) constant at $0 \cdot 2558$ arcmin min^{-1}, and the angular motion from east to west across the equatorial hermean sky is $\Omega - \omega$. Fig. 1 shows this motion from seven (terrestrial mean solar) days before perihelion to seven mean solar days after perihelion. Positive values indicate when the Sun is moving in the familiar east-to-west motion across the sky; negative values indicate when the motion is reversed. It should be borne in mind that the total length of a solar day on Mercury is 176 (terrestrial mean solar) days, or two hermean years (*sic*).

Reversal of the motion occurs for a period of 8·06 days, starting when the true anomaly is $-25^\circ\cdot 6$. Numerical integration (by both Simpson's rule and Gaussian quadrature as a check for mistakes) of the data shown in Fig. 1 shows that the Sun moves eastwards during this period through an angular distance of 66·6 arcminutes. The angular diameter of the Sun varies from 100·9 arcmin at the start and end of reversal, to 104·1 arcmin at perihelion. This shows that, if the Sun has just completely risen by the time that reversal starts, it will only partially set before rising again in earnest. For an observer on Mercury's equator somewhat further to the west, however, the Sun will have only partially risen before reversal begins, and then the Sun will completely set before rising for a second time.

Yours faithfully,
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2007 February 19

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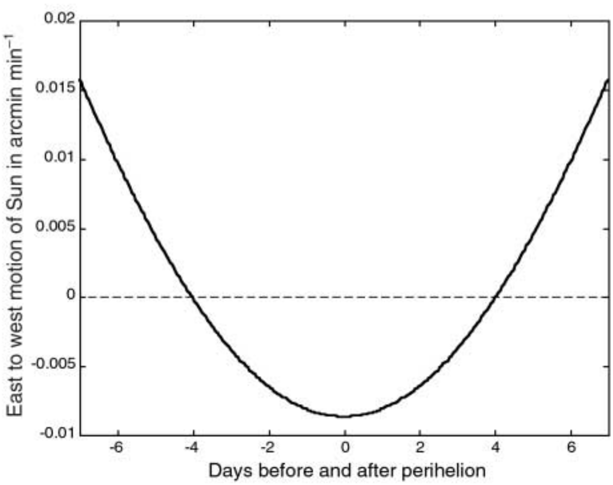

FIG. 1
Angular motion of the Sun across the sky for an observer on Mercury's equator.

Astrobiology

In a recent book[1] on astrobiology, Steven Dick & James Strick point to perturbing possibilities that could make the new multidisciplinary science become obsolete. Already one of these possibilities is in the air — namely, the proposed fifty-percent cut in the NASA astrobiology budget for 2007. Despite this, it is heartening that, through different sources of funding, the SETI Institute at Mountain View, California, has formed the Carl Sagan Centre for the Study of Life in the Universe. Projects at the Centre will focus on the Drake Equation, which was in earlier times also called "the Sagan–Drake Equation". The equation is an attempt to estimate the probable number of technological civilizations developing in the Galaxy.

Failure of imagination, according to Dick & Strick, is another possibility that could make astrobiology become obsolete; behind it looms also the possible failure to answer core questions of the origin of life. There is nothing new about this. Maybe the classical approach — to conceptualize 'living systems' in our own context — is part of the problem. It draws only on carbon-chemistry life forms and may never settle the problem of what other 'living systems' could have developed independently in the Milky Way.

An open mind as to what alien life-forms might have evolved is undoubtedly necessary for research and study of other planets and components of the Solar System as well of other stellar systems or cosmic phenomena. To set limits is analogous to idealism.

Clearly, to find the pieces and forms of life, astrobiology must continue. Its 21st-Century research tracks are important for expansion of life from Earth into space as well as for addressing the fundamental question: Are we alone in the cosmos?

Yours faithfully,
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REVIEWS

A Multinational History of Strasbourg Astronomical Observatory, edited by A. Heck (Springer, Dordrecht), 2005. Pp. 310, 24·5 × 16·5 cm. Price £81 (hardbound; ISBN 1 402 03643 4).

In this well-documented book, André Heck and his fourteen co-authors are retracing a unique and exemplary chapter of astronomical history, that of the Strasbourg Observatory, in the heart of Alsace. It was German at its foundation in 1872, then became French in 1918, German a second time in 1940, and finally French in 1945. At each of these three changes, the Director and the astronomers were displaced, going to the west or to the east (and sometimes to the south: at the end of the Second World War, Hellerich, the last German Director, was prisoner for a short time in southern France), alternately, to be replaced by a new director from the other side of the border, and new astronomers. Such moves have stimulated in Strasbourg the emergence of a European conscience, and were indeed instrumental in pushing the Strasbourg Observatory towards playing an international rôle, which has grown stronger and stronger year after year. In the post-war period, it was there that the recommendation of the International Astronomical Union, at its meeting at Brighton (1970), was implemented in 1972: a Centre for Stellar Data (Centre de Données Stellaires, or CDS) was created and evolved recently into a very active International Centre for Astronomical Data, where the fields of interest go far beyond the stellar realm, and encompass galaxies of all sorts.

Contributions by G. Wolfschmidt, H. W. Duerbeck, and W. C. Seitter elaborate upon the achievements and personalities of the two German periods. The achievements of the German astronomers of Strasbourg, generally unknown in France, were indeed quite notable. This is perhaps one the most attractive features of this book. The Observatory was founded in 1872, built and equipped from 1877 to 1880, and inaugurated in 1881. Actually, by 1914, the equipment was already very good for the period. The first Director, August Winnecke (1835–97), was general secretary of the *Astronomische Gesellschaft*, the powerful German association. He was also an excellent observer of comets and nebulae. The second Director, Ernst Becker (1843–1912), was a devoted observer of circumpolar stars, in order to study the motion of the pole. Walter Friedrich Wislicenus (1859–1905) was also a very interesting personality; he created a yearly bibliography of astronomy, which became, in 1899, the well-known and much used *Astronomisches Jahresbericht*; the successor to this series, from 1968 to 2000, was *Astronomy and Astrophysics Abstracts*, in English, but still published in Germany, in Heidelberg. Today, bibliographical studies are using the broader opportunities provided by informatics. One of the most remarkable scientists who worked at Strasbourg was Carl Wirtz (1876–1939), whose pioneering research on spiral nebulae influenced deeply the forthcoming studies by Shapley, Curtis, Slipher, and Hubble. His measurement of the diameter of Neptune remained a reference for a long time. During the second German period, almost all the instruments having been taken to Paris and Bordeaux, the Observatory's activity was mainly one of restoration, and not much research was actually achieved.

The two French periods are described by Suzanne Débarbat in her contribution. She describes the rôle of the several well-known astronomers of the pre-war period who later became pillars of French astronomy, namely Ernest Esclangon, creator of the 'speaking clock'; the first French Director, André Danjon

(1890–1967), the builder and photometrist; and Paul Muller, the double-star expert (to whom P. Bacchus dedicates a chapter). During the German occupation of France, the French team was displaced to Clermont-Ferrand and elsewhere in France, without many possibilities for research, and a German team took over.

In the post-war period, under the energetic direction of the Director, but without André Danjon, who did not come back to Strasbourg, the Observatory became a centre for the development of concepts and aims of modern instrumentation. It was also a fertile nest for the birth of many new talents, such as the youngest members of the Strasbourgeois group, Pierre Lacroûte (first Director after the war) and Pierre Bacchus, the initiators of the *Hipparcos* satellite. The astronomers who went through Strasbourg, although not all Alsatians (such as Couder and Lallemand), together with several young Alsatian astronomers (such as Fehrenbach, who later became Director of both the observatory in Marseilles and the Observatoire de Haute-Provence), were later active in many observatories in France.

The *Hipparcos* astrometric satellite of the European Space Agency (operational during the period 1989–1993) was indeed a remarkable achievement. It brought a huge harvest of stellar data (position, brightness, and parallax of hundreds of thousands of stars), 25 times more accurate than previous catalogues built with classical astrometric instruments. This Odyssey is described in the chapter written by Jean Kovalevsky, himself one of the main contributors to the success of the *Hipparcos* programme.

The editor (and co-author) of the book has been himself, after Jean Jung and Carlos Jaschek, the Director of the CDS, which continues now to act as an international centre for astronomical data, and is aiming at the creation, on a world scale, of the 'virtual observatory'.

This book is altogether a fine survey of a bipolar observatory, as described by André Heck in the first chapter of the book. It is indeed a very rare, if not unique, occurrence in the western world. One is fascinated by each team's apparent ignorance of those who preceded or succeeded. It would have been fascinating to know what they thought of each other. — JEAN-CLAUDE PECKER.

Andreas Cellarius: Harmonia Macrocosmica, by R. van Gent (Taschen, Köln), 2006. Pp. 240, 53·5 × 32·5 cm. Price £69·99/$125 (hardbound; ISBN 3 822 85290 2).

Those extremely lucky readers who own a copy of the original *Atlas Coelestis seu Harmonia Macrocosmica* (Johannes Janssonius, Amsterdam, 1660) need read no further. Their book, with its twenty-nine, massive, copper-plate, hand-water-coloured celestial maps and diagrams is worth considerably more than a typical astronomer's annual salary. Even the later reprints from the original plates, run off by Valk & Schenk in 1708, sell for around £2000 each. So this new, bargain, full-sized, annotated reproduction of the University of Amsterdam's copy of the original star atlas is really welcome.

The originator of this reproduction, Dr. Robert H. van Gent, is a Utrecht astronomer and historian of astronomy who now specialises in the history of Dutch celestial cartography. He introduces the book by providing a first-class overview of the history of celestial maps and charts, and then regales us with detailed notes on each of the plates in the original atlas. These notes are in English, German, and French in the book under review. And if you can wait until 2007

March the notes will be in Italian, Spanish, and Portuguese in a second edition. (In the original, the commentaries were in Latin.)

Andreas Cellarius (1596–1665) was born in Neuhausen, Germany, and then emigrated to Holland. In 1637 he was appointed as the rector (headmaster) of the Latin School in the Ceciliaklooster, Hoorn. Cellarius wrote three books. His first was on city fortifications and defence systems. Then he produced a book about Poland. His final work was a celestial supplement to a general world atlas being published by Johannes Janssonius. It is this atlas that showed Cellarius to be one of the greatest celestial cartographers of the Renaissance.

The plates in the reproduction under review are absolutely stunning. Benedikt Taschen, who managed the project and produced the book, is to be congratulated on the care that has been taken with the sharpness of the printing and vivid colour rendition. No expense seems to have been spared. Even the quality of the paper is worthy of praise. And the subject matter is of great interest. The original atlas was produced at an exciting epoch in the history of our subject. Three interpretations of the cosmos were still being debated. We are thus presented with plates illustrating the old geocentric views of Ptolemy, and others showing the new ideas of Copernicus and the 'compromise version' of Tycho Brahe. All the plates are in a rich Baroque style with classical and historical figures and astronomical vignettes gracing the banners and corners. The eight northern- and southern-hemisphere celestial star maps are stunningly beautiful. Cellarius even included those two 'Christian' depictions of the heavens in which Julius Schiller (*Coelum stellatum christianum*, 1627) filled the northern and southern sky with New and Old Testament figures, reserving the zodiac for eleven disciples and St. Matthius.

This reproduction of *Harmonia Macrocosmica* not only presents us with full-sized copies of the original plates, but also provides many additional pages showing magnified versions of specific features. I cannot recommend this book too strongly. I was amazed at the quality and the reasonable price. It is an absolute treasure. — DAVID W. HUGHES.

The Birth of Stars and Planets, by J. Bally & B. Reipurth (Cambridge University Press), 2006. Pp. 295, 28·5 × 25 cm. Price £25 (hardbound; ISBN 0 521 80105 2).

There has been something of a rash of star-formation textbooks recently, with a broad-ranging text by Stalla & Pahler, a monograph by Smith, the usual run of conference proceedings volumes, and *Protostars & Planets V* due out soon. So where does this book fit into the overall scheme of things? Well, this is somewhat unusual in that it is an attempt at a popular textbook on star formation. The stated aim of the authors is to communicate to "readers with an interest in understanding the Universe". This is a very laudable goal. Communicating the ideas of contemporary research to the public is something we all try to do in our different ways.

The book is written in an informal, chatty, and largely approachable style, and is liberally sprinkled with some of the most beautiful images our subject has to offer. There are nine chapters on star formation, four on planet formation, and three on star formation in other galaxies. There is then a final chapter on astrobiology — a subject that some of its target readership may find rather novel. Consequently the book covers all of the topics that might be expected. Naturally, given the specific interests of the authors, the discussion of jets, outflows, and Herbig-Haro objects is well to the fore, and this section provides some of the most dramatic pictures in the book.

Many technical words and phrases are footnoted, with extensive explanations in appendices at the back of the book. This is designed so as not to disrupt the flow of the book. The authors also state that they have not included any diagrams in the main body of the book, but banished them to the appendices also. The stated aim of that policy is not to put off the potential reader. However, it occasionally leads to slight anomalies, such as when reading the part of the text describing the Hertzsprung–Russell diagram it is necessary to flick forward more than 200 pages in order to see a version of that diagram. But this is no more than a minor quibble.

By and large the authors treat each aspect of the subject in careful and correct detail. Occasionally I feel that they have become a little carried away with the level of detail into which they delve. For example, the discussion of molecular dissociation at the tip of a bow-shock compared to the potential survival of molecules in the wings of a bow-shock is a detailed diagnostic that perhaps the general reader does not need to know to understand the formation of stars. However, it is of course possible for a reader simply to move on if they reach a part they don't understand. Conversely, there were times when I wished that some of our undergraduate students would read parts of the book to obtain a qualitative understanding of topics they sometimes do not grasp from our more orthodox, quantitative approach. Consequently I will be recommending it as background reading for our first-year students.

My only other minor quibble is that some labelling on the pictures themselves would have been helpful. For example, there are many instances where images are shown of the same region at different wavelengths, but there is no indication to tie down a particular feature in one wavelength to its counterpart at a different wavelength, since the image scales and sizes are usually different. In the case of the many pictures of Orion at various wavelengths it would have been easy simply to mark the familiar constellation stars on all images for ease of reference. Presumably the authors felt this would have spoilt the pictures.

Nevertheless, the book appears to work on many levels: the quality of the pictures allows it to pose as a coffee-table book (something the authors point out that they do not want it to be!); the approachable style of the text means that the keen amateur will learn something of this subject; and the level of detail and rigorous explanation would also allow it to be used by students embarking on their first course in astronomy to get a good background overview before they become embroiled in the detailed mathematics of the subject. All in all the authors are to be congratulated on producing a very useful addition to the growing library of books on star formation. — DEREK WARD-THOMPSON.

The Physics of the Cosmic Microwave Background, by P. D. Naselsky, D. I. Novikov & I. D. Novikov (Cambridge University Press), 2006. Pp. 255, 25·5 × 18 cm. Price £65/$125 (hardbound; ISBN 0 521 85550 0).

Recent observations of the cosmic microwave background (CMB) radiation have led to a number of spectacular breakthoughs in our understanding of the origin and evolution of structure in the Universe. The authors of this book have played an important rôle in establishing this new 'standard model' of cosmology, both in the development of theoretical concepts and in the analysis of the observational data. This book is thus a timely and authoritative account of the physics of the CMB and the cosmological constraints that can be derived from existing CMB observations and those planned over the next few years.

Following a thorough and accessible introduction to the observational foundations of modern cosmology, the authors provide a comprehensive account of radiative transfer in a homogeneous Universe, in particular the interaction of photons with an electron plasma and the ionization history of the Universe. These topics are covered in impressive depth while retaining clear physical insight. Particularly pleasing is the subsequent detailed account of the evolution of perturbations from both the Newtonian and General Relativistic viewpoints. This includes a clear discussion of scalar, vector, and tensor modes and an accessible approach to sophisticated topics such as gauge freedoms, radiative transfer through an inhomogeneous medium, and Sakharov oscillations. The last topic naturally leads on to an excellent pedagogical chapter that forms the heart of the book, namely the physics underlying the generation of primary anisotropies in the CMB. Beginning with a wry and entertaining account of the history of this topic, the chapter goes on to describe in full detail the various important physical mechanisms for generating CMB temperature fluctuations, closing with an informative account of the dependence of the CMB temperature power spectrum on cosmological parameters. The topic of polarization of the CMB is wisely left to a separate chapter, in which a very careful treatment is presented, starting from basic concepts but quickly introducing the electric and magnetic decompositions and their predicted power spectra. A novel geometric interpretation of polarization on the sphere is of particular interest here. The final theoretical chapter of the book focusses on the statistical properties of random fields, and particularly Gaussian random fields, including a clear account of correlation structures and local topological measures. Searching for non-Gaussianity is mentioned, but only in the context of Minkowski functionals, and one minor criticism is that perhaps some discussion of other techniques such as wavelets would have been appropriate here. Turning to observations, a valuable summary of the results derived from the first-year *WMAP* satellite data is presented, which is followed by a mouth-watering chapter looking forward to what we hope to learn from the forthcoming *Planck* satellite, due for launch in 2008. A brief conclusions chapter rounds off what is certain to be a very widely used book, and one that should prove to be an excellent resource for anyone interested in the exciting area of CMB science. — MIKE HOBSON.

The History of Meteoritics and Key Meteorite Collections: Fireballs, Falls and Finds, edited by G. J. H. McCall, A. J. Bowden & R. J. Howarth (The Geological Society, London), 2006. Pp. 513, 25·5 × 17·8 cm. Price £95 (hardbound; ISBN 1 862 39194 7).

In the two hundred years before 1972, meteorites were being recovered at the rate of about ten per year. As about a tonne of extraterrestrial material arrives at the Earth's surface each day, this was a rather paltry number. After the retardation and ablation that they suffer on passing through the atmosphere, the meteoritic end-product free-falls relatively gently to the ground at a velocity of about 200 km hr^{-1}. Masses ranging from a few grams up to 60 tonnes or more are found. Many of the incoming bodies also break up into a multitude of fragments.

The vast majority of meteorites are collected with care and taken lovingly to the local natural history museum or university geology department. In the old days there were exceptions to this rule. The French sometimes chained meteorites to the ground where they fell, in case they decided to depart from Earth as swiftly as they arrived. And in China many meteorites were ground up and ingested as a medicine. But the surviving stones and irons can form the hub of an extremely impressive meteorite museum. The visitor is confronted with showcase after showcase full of extraterrestrial rocks, the displays often enlivened by the occasional huge iron meteorite resting on a central pedestal.

In 1972, 2100 distinct meteorites were known to science. But the subject has not stood still. Today the number is an impressive 30 000 thanks mainly to searches of blue-ice regions in Antarctica and sandy deserts such as the Nullarbor, Sahara, Arabian Peninsula, and Roosevelt County, New Mexico.

Curating, collecting, and the astronomical and geological significance of meteorites are the main themes of the book under review. The publication was spawned by a one-day meeting of the History of Geology Group in 2003 December. After much subsequent hard work the end result is a collection of twenty-four absolutely delightful, up-to-date, well-referenced, and well-refereed papers. One minute you are wandering the corridors of the Muséum national d'Histoire naturelle in Paris, and a few pages later you are whisked off to the wonders of the meteorite collections in the Smithsonian (Washington), Western Australian Museum (Perth), Natural History Museum (London), Vatican Museum (Rome), Museum für Naturkunde (Berlin), American Museum of Natural History (New York), Russian Academy of Science (Moscow), and Natural History Museum (Vienna). The illustrations are marvellous and many. Etchings and photographs of venerable researchers and curators vie with today's generation of active scientists peeping over huge meteorites in railway trucks, tweezering meteorites from desert sands, and gazing at meteorites from tracked ice-mobiles on Antarctic glaciers.

Such joys as meteorites on postage stamps, cartoons of the British Museum 'stealing' meteorites from the Irish, meteorite falls depicted on the Icon of St. Prokopiy, Dürer's Melencolia I, and Sebastian Brant's broadsheet compete with vivid modern images of chondrules, tektites, calcium–aluminium-rich inclusions, thin sections, Widmannstätten patterns, fusion crusts, and strewn fields. The book also moves from the museum into the research laboratory and contains a dozen detailed papers on such topics as desert meteorites, meteorite-age determination, the meteorite's influence on the selection of Solar-System-origin hypotheses, meteorite impacts and Earth cratering, meteorite precursor orbits, chondrules, inclusions, tektites, Martian meteorites, and asteroidal parents.

This book is a refreshing amalgam of the past and the present, an enticing insight into the work of the collectors and researchers, and, moreover, an absolute joy to read and look at. — DAVID W. HUGHES.

Astrophysics of Gaseous Nebulae and Active Galactic Nuclei, 2nd Edition, by D. E. Osterbrock & G. J. Ferland (University Science Books, Sausalito, California), 2006. Pp. 461, $24 \times 19 \cdot 5$ cm. Price £34·99 (hardbound; ISBN 1 891 38934 3).

Some of us are old enough to remember the first edition of Don Osterbrock's classic textbook on gaseous nebulae*. On instructions from my then supervisor I borrowed it from the library and started reading. That text, written by an acknowledged expert, introduced me to the complex world of photoionization, charge exchange, and all the other processes I needed to know about in order to try and understand weird-looking spectra. I'm still trying and I still turn to Osterbrock when I feel myself not understanding a basic concept or two.

*The Editors regret to record the death of Professor Osterbrock; an obituary appears on p. 203.

From the first, Osterbrock wrote the book not only as an introduction to the physics but crucially to provide guidance on how to interpret spectra. The study of gaseous nebulae was originally dominated by H II regions, supernovae, and planetary nebulae. The rôle of spectroscopy in modern astrophysics has increased in importance steadily over the years, and in particular has moved out of the Galaxy to the most distant objects, some of which are only seen in emission. In many of these objects, and indeed many of the more traditional ones, it has been realized that the classic nebular-ionization structure is an over-simplification. This is particularly true where optical depths can be large. To expand on some of these concepts, Osterbrock updated his book in 1989 to include AGN. This hefty tome is the third edition of the original book, although it is referred to as the second edition of the updated volume — the 'AGN2'.

The new edition has gained an additional author in Gary Ferland. Ferland is an appropriate choice as originator of perhaps the most-used photoionization code around — CLOUDY. Their combined observational and technical experience is probably second to none and inspires a confidence which is not misplaced. Avoiding the temptation to get bogged down in too much technical detail, the book still reads like a volume written to enable you to get on with the astrophysics. The older material has been revisited and updated, although of course the basic theory has not changed. Nevertheless, the authors make clear that even apparently simple structures such as H II regions, now observed across the electromagnetic spectrum, still defy the most complex computer models. Where new material has been added it complements the old. This includes, for example, a brief but illuminating chapter on high-energy spectroscopy.

As ever the text tries to explain in words concepts which can baffle the inexperienced and experienced alike. As a result the new blends in smoothly and this remains a classic well-written text. You will not find all the details here — that would require a shelf of books — but you will find sufficient to clear the mind a little and force you to think about astrophysics at a deeper level.

At a mere £35 for a hardback this is a bargain and has to be recommended reading for any student or pundit alike. — PAUL O'BRIEN.

The Herschel Partnership, by M. Hoskin (Science History Publications, Cambridge), 2003. Pp. 182, 24 × 15·5 cm. Price £25/$40/€40 (hardbound; ISBN 0 905 19305 9).

On a recent visit to Bath, my wife and I spent a pleasant hour or so re-acquainting ourselves with the charmingly atmospheric Herschel Museum at 19 New King Street, and by way of a souvenir (and Christmas present for me!) we purchased (in addition to a delightful CD of William Herschel's symphonies) a book that the *Magazine* didn't get for review when it appeared in 2003. In *The Herschel Partnership*, Michael Hoskin presents us with a fascinating study of Caroline Herschel that, by dint of diligent research among extant letters and diaries, reveals in depth a woman who played a crucial part in the work that turned William Herschel into a world celebrity and a hero of British observational astronomy.

Caroline and William were just two of numerous siblings of Isaac Herschel (a musician in the Hanoverian Foot Guard) and his rather dull and even brutish wife Anna. While the boys of the family (at least those who survived into adulthood) could expect professional work (especially following in their father's musical footsteps), the girls were not so fortunate, and Caroline soon became a household drudge under her unsympathetic mother. But these were turbulent times, with Prussia and France in a drawn-out war, and William escaped to England to pursue his musical career in Bath. Eventually, he was able to rescue Caroline and thus begin an important partnership, first in music and then in the subject that progressively absorbed all William's energies — astronomy, and, with occasional help from his brother Alexander, telescope making.

Caroline clearly idolized William, for not only was he her rescuer, he was also her tutor, first in music and later in the tasks of a (very competent) scientific assistant. It soon becomes very clear that, while William provided the genius and the drive, Caroline was essential to the organization and documentation of the work in hand, and Herschel's legacy would probably have been far less without her dedication. After the discovery of Uranus from the house in New King Street in 1781, which resulted in the award of a £200 salary from the King, William moved closer to Windsor to become a 'Royal Astronomer', expected to entertain the King's family and friends with occasional night viewing. Caroline of course came too, and developed into a useful observational astronomer in her own right, especially as a comet huntress.

With the construction of the 40-foot telescope, an ultimately somewhat disappointing instrument, William turned from observing to a more reflective consideration of what his observations meant — and perhaps a less taxing life: he acquired a wife and, importantly, a son, John, who was to put on the observational mantle and extend William's work on the 'construction of the heavens', double stars, and nebulae to the southern skies. Meanwhile, Caroline continued her devoted service to her brother but was probably less happy in this new situation. In fact, her writings suggest that she had a rather 'crotchety', negative outlook on life, which only got worse with time, especially in the years after William died, in 1822, when she returned to Hanover to live with her brother, Dietrich, and his family. That said, she endured for many more years and visitors such as John Herschel evidently thought that she was generally in fine fettle in her 'retirement'. She died at the ripe old age of 97 in 1848.

This is an important volume in the social history of astronomy, revealing, as it does through her own words and deeds, the character of a woman who contributed enormously to astronomy. Gratifyingly, her work was recognized at the time through the award of medals from learned societies and laudatory remarks from major (male) astronomers, and she became probably the first female astronomer to receive a salary (from the King). This is a fine piece of scholarship at a very reasonable price. — DAVID STICKLAND.

[On learning of this review, Dr. Hoskin kindly sent a companion volume examining the autobiographical notes of Caroline Herschel. The Editors thought that this too deserved to be brought to the attention of our readers and they invited Dr. Mary Brück to describe that work in the review that follows. Further details of these and other publications on historical themes may be found on the web site of Science History Publications Ltd.: www.shpltd.co.uk; see also the advertisement at the front of this issue.]

Caroline Herschel's Autobiographies, edited by Michael Hoskin (Science History Publications, Cambridge), 2003. Pp. 147, 24 × 15·5 cm. Price £25 (hardbound; ISBN 0 905 19306 7).

Caroline Herschel's memoirs and correspondence are a major source of information on her own life and on that of her famous brother William. In fact, she wrote two separate versions of her autobiography, the first when she was already in her seventies, the second, less complete, a full twenty years later. They record her upbringing in her native Hanover and her life in England where she arrived at the age of 22 until (in the case of the first version) William's marriage sixteen years later. The texts, from more than one source, have been edited and annotated by Michael Hoskin and are now published together in full for the first time.

Dr. Hoskin has performed his task with immense thoroughness. Caroline's original pagination is retained in the body of the texts, and cross-references in the margins allow the versions to be compared. There are copious explanatory footnotes and elucidations of Caroline's occasional German expressions. The Introduction gives an account of how they came to be written, and is particularly helpful to the reader by explaining the confusing background of war which profoundly affected the entire Herschel household in the first part of Caroline's story. There is also a detailed chronology of family events and a genealogy going back three generations.

Caroline had a phenomenal memory, and her second autobiography, written completely afresh long after the first had left her possession, confirms the first to an uncanny degree. The key events in the Herschel saga are familiar to most of us, but to read Caroline's original words as they flowed uninhibited from her pen, complete with quirky expressions and erratic spelling, brings her personality vividly to light in a way that is lost in popular reverential treatments. One can almost hear her voice. It is striking to find that, after fifty years in England, Caroline had not mastered the language and probably still spoke with a German accent.

A special advantage of having these reminiscences available in their entirety is that the reader gets to know Caroline as a real human character. For example, as Dr. Hoskin points out, she was a complainer, and her slave-like devotion to her famous brother was not entirely unalloyed. Likewise, Caroline's relations with her mother, whom she criticized for discouraging her educational ambitions and for treating her as a servant, were by no means as unsympathetic as is generally imagined. This is particularly evident in the second, softer, autobiography, written at the age of ninety and addressed to her beloved nephew John and his wife. Many examples of interesting little-known facts and fresh glimpses into Caroline's unostentatious life undoubtedly await the keen-eyed reader.

The book is elegantly designed and produced. There are illustrations of Caroline's telescopes and samples of her beautiful handwriting, including the page from her observing book showing the drawings of her first comet.

Caroline Herschel's Autobiographies is an ideal companion volume to *The Herschel Partnership*. It is a pleasure to read simply as a story, and is a compendium of information on Caroline and William's parents, siblings, and forebears. — MARY BRÜCK.

An Introduction to General Relativity and Cosmology, by J. Plebański & A. Krasiński (Cambridge University Press), 2006. Pp. 534, 25 · 5 × 18 cm. Price £45/$80 (hardbound; ISBN 0 521 85623 X).

In the time-honoured tradition of many books from CUP, *An Introduction to General Relativity and Cosmology* cannot really be described as an introduction at all. It is not a book for someone looking for a gentle introduction to the subject, but rather an excellent high-level textbook that includes a number of topics that are not readily to be found elsewhere. I recommend it very highly for students who have studied General Relativity already (perhaps having read a real 'introductory' book), and who would like to gain a deeper mathematical insight into the subject. One of the very nice features of the book is the inclusion of extended derivations, which are set out very clearly and which guide the reader helpfully through advanced topics. In the more mathematical sections (of which there are plenty: gravity really only starts on page 125), the commentaries on some of the theorems give useful insight. The reader will need to be comfortable with mathematics or mathematical physics at advanced undergraduate level to get the most out of the book. The coverage of topics is deliberately inhomogeneous: the standard results are there, but in addition there are some more unusual topics, such as an in-depth discussion of Lemaitre–Tolman(–Bondi) solutions and a rather thorough treatment of the Kerr metric, at a depth which would not be found in most textbooks. For anyone looking for a thorough mathematical treatment of General Relativity, or for a supplement to existing books, this is highly recommended. It is not a standard text by any means, but I would be surprised if there was anyone who didn't find in it something new, interesting, and enlightening. — ALAN HEAVENS.

The Curious History of Relativity: How Einstein's Theory of Gravity was Lost and Found Again, by J. Eisenstaedt (Princeton University Press, Woodstock), 2006. Pp. 384, 23 × 15·5 cm. Price $29·95 (about £15) (hardbound; ISBN 0 691 11865 5).

This book gives a lucid account of the struggle to find the right concepts to understand how the speed of light can be independent of the motion of its source, not just to those at rest in an aether but to all observers whatever their speeds. While Poincaré stated the right principles and Lorentz the right mathematics, the main understanding was due to Einstein, though even he had difficulties in fully appreciating Minkowski's geometry of space-time. This struggle was renewed when Einstein, with Grassman's help, searched for a relativistic theory of gravitation that obeyed the principle of equivalence. Probably the best part of the book is the description of this long and often lonely quest guided by Mach's principle, which was never fully incorporated in the final theory. I found the last part of the book describing Kruskal's extension of Schwarzschild's black-hole solution was not explained in a way that carried conviction to the reader.

The book's thesis that General Relativity was in the doldrums from 1930–1960, with few serious physicists or astronomers following the subject, has the ring of truth, but the whole book is written from a French perspective with rather scant attention to literature published in English. Synge already in 1950 gave the first account that included the full extension of Schwarzschild's metric.

To me, a Cambridge undergraduate who came up in 1952, Bondi's lectures on General Relativity further stimulated the great interest acquired from Eddington's books. Relativity in Cambridge was far from the moribund image portrayed here. Whereas Wheeler and Kruskal are given the credit for reviving the subject, I believe that Kerr's discovery of his metric in 1963, with its wondrous ergosphere still unexplained, coupled with the discovery of quasars that same year, provided other cogent reasons for the renewed interest in the field.

However true the basic thesis, it is bad history to leave out contrary evidence; nothing is said of Eddington's 1924 discovery that $r = 2m$ was not a barrier in infalling coordinates, or of the work of Oppenheimer and collaborators in 1939, nor of Datt in 1938, all of which demonstrated the formation of black holes. From a Cambridge viewpoint it was Sciama's amazing ability to stimulate interest that led some of the ablest, including Ellis, Carter, and Hawking, into this wonderful subject.

The book has been translated from the French, which may account for the strange implication that Newton's work was 150 years after Kepler's. — D. LYNDEN-BELL.

OBITUARIES

Virpi Niemela (1936–2006)

This well-known Finnish-born astronomer has left us. Virpi, born on 1936 December 26, passed away at a hospital in the city of La Plata on 2006 December 18, just a week before her 70th birthday. Her death occurred just four days after the closing session of a very enthusiastic international scientific meeting on massive stars, organized in her honour to celebrate her forthcoming birthday. That meeting, attended by some one hundred astronomers from Argentina and many other parts of the world, took place in Cariló, an exclusive, beautiful, summer-resort town on the Atlantic coast of Argentina, about 360 kilometres from the capital city of Buenos Aires.

Virpi Sinikka Niemela, who lived in Argentina since she was 17, and soon acquired Argentine citizenship, took up the study of astronomy at the La Plata University Observatory, becoming a Licenciate in 1969 and, five years later, in 1974, gaining a PhD. Her PhD dissertation, for which I was her adviser, was devoted to the study of a Wolf-Rayet binary that displays hydrogen lines in its spectrum. Even before this, Virpi had decided to devote her very active and fruitful scientific life to the study of Wolf-Rayet and massive stars, among them quite a number of close-binary objects. Altogether, she was the author or co-author of nearly 160 papers, of which 86 appeared in refereed journals, the remainder being invited talks at international conferences, the majority of which were also refereed. Virpi thus contributed a great deal towards our knowledge and understanding of the kind of astronomical objects on which she worked.

Virpi was always inclined to join forces with other researchers, not only from Argentina but also from much further afield. Thus her curriculum tells us that during her working life Virpi collaborated with some 140 colleagues, only around a third of them working in Argentina, with the most frequent co-authors being Anthony Moffat, Nidia Morrell, and Rodolfo Barbá.

Virpi's interest in the Wolf-Rayet stars started one afternoon in 1970, when I invited her to join forces with me and work together on a short contribution that was published that year in *The Observatory Magazine* (**90**, 198). It was her first paper, and it certainly aroused Virpi's interest in Wolf-Rayet stars! Later, I invited her to study with me a series of spectra of the brightest Wolf-Rayet star in the sky, γ^2 Velorum, that I had taken earlier with the grating spectrograph attached to the 1·50-m reflector at the Bosque Alegre Station of the Córdoba Observatory; this led to our second paper together.

Virpi was a researcher for the Province of Buenos Aires Research Council, in which she reached the highest rank. She first worked in Buenos Aires, at the Institute of Astronomy and Space Physics, part of the National Research Council and the University of Buenos Aires, but later she moved to a professorial research position at the University of La Plata's Faculty of Astronomical and Geophysical Sciences, where, in 2004, she was declared an Emeritus Professor. In 2000, Virpi was elected a member of the National Academy of Exact, Physical and Natural Sciences, an institution that two years earlier had awarded her the Carlos Varsavsky Prize in Astronomy. In 2003, the Konex Foundation of Buenos Aires rewarded her with the Konex Award in Science and Technology and with the Platinum Award in Astronomy. Virpi has been a Visiting Professor or Visiting Astronomer in Brazil, at ESO, in Chile, and in France.

At the Cariló meeting, Virpi learned, with much joy and emotion, that asteroid 5289, discovered at El Leoncito's Carlos U. Cesco Astronomical Station of the San Juan University, had been assigned the name Niemela by the International Astronomical Union, at the request of the discoverers. She also learned, again with much pleasure, that Great Britain's Royal Astronomical Society had appointed her an Associate of the Society. The meeting, the awards, the expressions of friendship and appreciation on the part of the attendees was perhaps, emotionally, too much for her: she had come to the meeting right from the hospital bed, to which she had to return in a critical medical condition when the Cariló conference ended. She became ill in early 2005 when she was planning, very kindly and enthusiastically, to organize a meeting on the Struve–Sahade effect to celebrate my 90th birthday, but she had to abandon the idea on account of the disease that suddenly began to affect her. After the initial medical treatment, Virpi appeared to have overcome her illness, but, early last year, the problem recurred, evidently with more vigour; but even in her sickness, Virpi showed how strong-minded she actually was.

Virpi is survived by two sons and four grandchildren, and all her students and friends all over the world. — JORGE SAHADE.

Donald Osterbrock (1924–2007)

Donald Osterbrock, an eminent astronomer, a leading authority on the history of astronomy, and former director of the University of California's Lick Observatory, died suddenly of a heart attack on 2007 January 11 at the age of 82.

Born and raised in Cincinnati, Osterbrock earned a BS in physics and MS and PhD in astronomy from the University of Chicago, working with S. Chandrasekhar, and then with W. W. Morgan and S. Sharpless on the spiral structure of the Galaxy. After a postdoctoral fellowship at Princeton University and faculty positions at the California Institute of Technology and the University of Wisconsin, Madison, he went to the University of California at Santa Cruz (UCSC) in 1972 and served as Director of Lick Observatory from 1973 to 1981. Osterbrock, a professor emeritus of astronomy and astrophysics at UCSC, had continued to work at Santa Cruz and mentor students after his official retirement in 1992 and was on the campus at the time of his death. His own account of his "fortunate life in astronomy" is to be found in *Annual Review of Astronomy & Astrophysics*, **38**, 1, 2000.

As a researcher, Donald Osterbrock made major contributions in several areas, from understanding the nature of ionized gas around hot stars to discovering and studying new types of active galactic nuclei. He was a pioneer in the use of spectroscopic methods for the study of gaseous nebulae in the cosmos. His books on gaseous nebulae and active galaxies have been standard references for astrophysicists for more than 30 years; with Gary Ferland he produced a completely revised edition of *Astrophysics of Gaseous Nebulae and Active Galactic Nuclei* in 2005.

Osterbrock also published numerous authoritative articles and books on the history of American astronomy, mostly after his retirement, including *James Keeler: Pioneer American Astrophysicist* (1984), *Eye on the Sky: Lick Observatory's First Century* (1988), *Pauper & Prince: Ritchey, Hale, and Big American Telescopes* (1993), *Yerkes Observatory 1892–1950* (1997), and *Walter Baade: A Life in Astrophysics* (2001).

Donald Osterbrock received numerous awards and honours, including lifetime achievement awards from the American Astronomical Society and the Astronomical Society of the Pacific. In 1997, the Royal Astronomical Society awarded him its highest honour, the Gold Medal. The American Astronomical Society and the Antique Telescope Society both honoured him for his contributions to the history of astronomy. An asteroid (6107) was named after him in 1996. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society, and an Associate of the Royal Astronomical Society. He was the author, co-author, or editor of 12 books and published more than 150 research papers. — DAVID STICKLAND.

ERRATA

The vigilant, web-using reader will probably have discovered that the URL given in the caption to Plate 1 at the beginning of the 2007 February issue was incorrect and should have been www.robgendlerastropics.com; the Editors apologise for this oversight.

We also apologise to Dr. Jackman for allowing an incorrect spelling of her name to appear on the 'Contents' page of that same issue.

NEW EXPLANATION FOR GLOBAL WARMING

The Sun sheds 100 million tons of matter per second. — *JBAA*, 116, no.3, June 2006 (advert on back cover).

FAST WORK

The Big Island of Hawaii was created when lava poured from the volcano Kilauea for 10 years during the 1980s and 1990s — *Guide to Mountains* (Philip's, London), 2005, p. 13.

HAS THE SOLAR SYSTEM SHRUNK?

The system was proven each day by observing the very weak signal from the *Pioneer 10* spacecraft, then more than 10 million km from Earth and far beyond Pluto. — *A&G*, **47**, 4·13, 2006 August.

SOUNDS MORE LIKE THE SQUARE MEGAMETRE ARRAY

The whole [Square Kilometre Array] could well be more than 3000 km across so it will need to be located in a large, sparsely populated area. — *A&G*, **47**, 4·16, 2006 August.

TOO HOT FOR COMFORT

... a high-performance telescope to study asteroids near the Sun from a vantage point 17,000 metres away. — *Nature*, **436**, 2005 August 4, p. 619.

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.

LAYOUT: The general format evident in this issue should be followed. All material must be double spaced. Unnecessary vertical spreading of mathematical material should be avoided (*e.g.*, by use of the solidus or negative exponents). Tables should be numbered with roman numerals, and be provided with a brief title. Diagrams should be numbered with arabic numerals, and have a caption which should, if possible, be intelligible without reference to the main body of the text. Lettering should be large enough to remain clear after reduction to the page width of the *Magazine*; figures in 'landscape' format are preferable to 'portrait' where possible.

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and for books:
(No.) Authors, [in Editors (eds.),] Title (Publisher, Place), year[, page].
where the bracketed items are required only when citing an article in a book. Authors are listed with initials followed by surname; where there are four or more authors only the first author '*et al.*' is listed. For example:

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

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

UNITS & NOMENCLATURE: Authors may use whichever units they wish, within reason, but the Editors encourage the use of SI where appropriate. They also endorse IAU recommendations in respect of nomenclature of astronomical objects (see *A&AS*, **52**, no. 4, 1983; **64**, 329, 1986; and **68**, 75, 1987).

SUBMISSION: Material may be submitted as 'hard copy', or (preferably) by electronic mail to the address on the back cover.

Hard copy: *Three* copies should be submitted. Photocopies are acceptable only if they are of high quality.

Email: contributions may be submitted by email, preferably as standard (LA)TEX files. Reference to personal macros must be avoided. Those submitting letters, book reviews, or thesis abstracts are encouraged to use the *Magazine's* LATEX templates, which are available on request. Word files are also welcome provided they conform to the *Magazine*'s style.

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

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

CHECKLIST: Double-spaced? Reference style? Three copies?

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NOTES TO CONTRIBUTORS

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