THE OBSERVATORY

Vol. 129 — 2009 DECEMBER — No. 1213

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2009 May 8th at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

A. M. Cruise, *Vice-President*
in the Chair

The Vice-President. The President has been called away to another function and I'm standing in for him this afternoon: my name is Mike Cruise and I'm the Vice President. We begin with a talk by Allan Chapman, on 'Thomas Harriot and his Welsh friends: the birth of telescopic astronomy in the British Isles.'

Dr. A. Chapman. The date 2009 July 26 is the 400th anniversary of the first properly dated, drawn, and documented observation of an astronomical body using a telescope, made by Thomas Harriot at Syon Park, near Kew, London. He beat Galileo by four months. I say that not for any nationalistic reason, but simply that it was a plain fact. Harriot did not claim great kudos — in fact his friends had to chivvy him into making any kind of claim whatsoever; and he admired Galileo profoundly, and later admitted being inspired by him. Nevertheless, it was an Englishman who on that date first saw through an astronomical telescope and recorded the 5-day-old Moon, at about 7 p.m. in the evening.

Who was Thomas Harriot? He was a well-known English mathematician, but his work as an astronomer was fairly obscure, or at least kept largely to the measurement of planetary positions. He did very little work in the way of astronomy apart from a remarkable four-year corridor — 1609–1613 — when he heard of a 'Dutch trunke', as he called it, in the days when the telescope had not yet been given a name. He acquired one from somewhere unspecified and his friend and assistant Christopher Tooke made him several replicas. The set started with a low magnification of 6×, and the most powerful worked at between 35 and 40×, and all of this within a fairly short period. We have to remember that there was nothing particularly difficult in making a telescope in 1609 — spectacles had been around for centuries: they'd been known since at least the 13th Century, and there would have been very few cities in Europe where you could not have had a pair of spectacles made. And the technologies for making spectacles and telescopes were very, very close. Once the optical principle had been grasped, telescopes quickly proliferated across Europe.

However, I think there are good reasons to suggest why there was not a Tudor telescope: Harriot had been a personal friend of Digges, Bourne, and all the people who would have no doubt been connected with a so-called Tudor telescope in the 1570s and 80s; yet when he first saw the Moon through his Dutch trunke in 1609, he was quite amazed. In other words, it is clear he was not already accustomed to some kind of 'far-seeing glass' in his experience thus far.

Harriot was a mathematician. He is famous as a geometer, and as a pioneer of algebraic mathematics. His pupils, like Nathaniel Torporley, published a number of works after his death, but none, apart from celestial mechanics, pertaining to astronomy. His telescopic work was not known until 1784, when von Zach, the German mathematician, was going through his papers and discovered the lunar drawings. Then Stephen Peter Rigaud, Savilian Professor of Astronomy at Oxford in 1832, whilst working on a larger project of a great compilation of the works of Rev. James Bradley, also went through the Harriot papers, and realized there was something important, and published the work which von Zach found but had published only in a German journal. So it was not until the 1830s that Harriot's significance as an astronomer began to be more widely appreciated.

We know very little about Harriot's background. He was born in 1560 and bred in Oxford, and described by the famous Oxford gossip collector, Anthony Wood, as "tumbling out of his Mother's Womb into the Lap of the Oxonian Muses". He entered St. Mary's Hall at age 17, a college now owned by Oriel, and then took the normal arts degree of the day, which would have included the quadrivium — astronomy, arithmetic, geometry, and mathematics — and then entered the service of another Oriel old boy, Sir Walter Raleigh, eight years older than Harriot, and already famous.

Through his association with Raleigh, Harriot makes his proper connection with the men who could have been the ancestors of the so-called Tudor telescope. In the circle around Raleigh are some of the most extraordinary figures in Elizabethan England: mathematicians, such as Thomas Digges, his father, Leonard Digges, William Bourne, instrument maker, and Dr. John Dee, mathematician, astrologer, physician, alchemist, and private advisor to Queen Elizabeth I on all things technical. Harriot was moving in fairly high-profile society, and I'm pretty sure Harriot must at least have been in the Queen's company at some point.

In 1585–86 he spent a year in Virginia, the newly named colony, as surveyor and philosopher on the Raleigh-inspired expedition. He made major contributions to what we would call ethnology: he took a serious interest in the local Algonquian Indians, writing their language down in what Aubrey called, rather charmingly, the American language. He was interested in their ethics, their religion, their natural philosophy. He was perhaps responsible for introducing a new word to the English language: shortly before they were due to come back in 1586 June, a violent storm swept up the eastern seaboard. The local population said it was caused by their local storm God, Huracan, perhaps the origin of the word 'hurricane'. I would also be so bold as to claim that while he was in Virginia, Thomas Harriot became the first person to lecture on 'modern' science and technology on the North American Continent, for he tells us that he showed and explained compasses, magnets, burning glasses, lenses, a mechanical clock, and other devices to a group of Indians.

When he came back to England, he became a well-endowed private mathematician in the entourage of Sir Walter Raleigh. When Raleigh lost favour with the Queen, in the 1580s, Harriot was taken up by Lord Percy, 9th Earl of Northumberland, one of the richest men in England, and known as the 'Wizard Earl' because of his fascination with what was called natural philosophy.

He had money settled on him by Raleigh, and by Percy, an income in the region of 200 pounds a year for which he had to do very little other than be very clever (about twice the income of the warden of an Oxford college), and this was perhaps one reason for his not being too keen on becoming too prominent when he made the first recorded telescopic drawing of the Moon.

He worked as a mathematician and a correspondent for the Earl of Northumberland, one of his 'Three Magi', the Earl's entourage of philosophers. In 1605, Lord Percy suffered a serious reversal: Percy had a cousin involved in the Gunpowder Plot, and was arrested and put in the Tower. Harriot ended up spending three weeks in the gatehouse prison being examined to see if he was connected with the plot. This was another reason why he had no concern to draw attention to himself when he first made the lunar observations. You imagine: you are very well placed indeed, you are enjoying in modern terms a tax-free income of £150 000 a year, you have a grace-and-favour house in Syon Park, and probably another one in Threadneedle Street in London; your two closest friends are in the Tower of London for high political offences. Do you want to draw attention to yourself? [Laughter.]

We do not know where he got his 'Dutch trunke' from, but presumably Holland. We do know that Tooke made several more, and Harriot made his monumental drawing of the Moon during the summer of 1609. If one inspects one of Harriot's first-ever drawings of the Moon from telescopic observation, one notes that it is not of the whole Moon, but a sketch of the terminator region near Theophilus and Mare Fecunditatis. One can compare this with a drawing made two years later, and one notes an extraordinary improvement in the accuracy of his cartography, and of the whole Moon, where many details can be correlated with known features. All of these documents are available for inspection in the West Sussex archives in Chichester and are also on the Internet. Harriot obtained a copy of Galileo's publication of 1610 March, and this seems to have spurred him on to do further lunar work.

Where does the Welshness come into all of this? One of Harriot's main correspondents for many years before the telescope was Sir William Lower of Trefenty in Carmarthenshire, Wales. Lower was a Cornish MP who married Lady Penelope Perrot, a Welsh heiress, and took his country seat in South Wales. In his letters to Harriot, Sir William speaks of "our Trafentine Philosophers", or in other words, scientists: he speaks of at least three — the Elder and the Younger Mr. Protheroe, father and son, and a Mr. Vaughan, but he gives no Christian names, which makes it hard to identify them. We know they were highly educated men, because they were already poring over a copy of the newly-pressed *Astronomia Nova*, Kepler's Latin treatise on his first two laws of planetary motion, one of the most advanced maths books ever produced up to that time; and they were wrestling over how the planets could move in ellipses. They were in the thick of this when they received telescopes from Harriot. We are dealing here with a remarkably advanced Welsh community of philosophers.

When they read Galileo, they started to take it further. Galileo came to the telescope with a very focussed agenda: Copernicanism. The telescope showed the Universe to be very different from that perceived with the naked eye. These bodies through the telescope, for instance, were found to be not points of light, but spheres. Galileo draws lots of evidences in his *Sidereus Nuncius* of 1610 March, based on his own observations of 1609 December – 1610 January. Harriot and his friends were fascinated by Galileo's discoveries. They were themselves active Copernicans, and as a result, they were spurred further to do work on astronomy, and Harriot produced his map of the Moon which, alas, would remain unpublished until the 1960s.

Harriot's map was the best of its time — it was not until Hevelius in Danzig, 40 years later, using much better telescopes, that a better map was produced. One can only see a limited part of the Moon with a telescope of the type used by Harriot, which explains why some of the features in his initial sketches are not quite in the right places; but his map is astonishingly accurate. If one compares it with Galileo's drawings in *Sidereus Nuncius*, Galileo's are nowhere near of the same quality. Galileo captured the general look of the Moon through the telescope — an artist's response to a rough planet, as opposed to the smooth planets of Ptolemy's Solar System. Harriot — cartographer, mathematician, and Virginian surveyor — provided a much more scientific map of the Moon. His manuscripts also contain a number of smaller sketches, such as of the crater Theophilus, and a series of Sun sketches, showing sunspots. He may already have heard of observations of sunspots by Galileo, but he was able to make a better determination of the period of the solar rotation than Galileo, and came up with a figure within a few hours of today's accepted figure. To Copernicans, the demonstration of the rotation of the Sun was so important because Ptolemy, Aristotle, and the classical mathematicians who had seemed to substantiate the geocentric theory, held that the Sun was a perfect sphere, immutable, and should not have an axial rotation. Then behold, the Sun is blotted and it rotates. And while this in itself does not prove the Copernican theory, it none the less shows that geocentricists had got something badly wrong.

Although Harriot made accurate measurements of the positions of celestial objects, he seems to go no farther with his telescopic observations. By 1613, when he seems to have made his last major telescopic observation, he had gone about as far as he could with a telescope of 40 times magnifying power, and he returns to his more beloved areas of pure mathematics. There was no reason for him to draw attention to himself, there was nothing he could have got from further public attention, and Lower himself says in his letters, "Let your countrie & friends injoye the comforts they would have in the time and greate honor you would purchase your selfe by publishing some of your choice workes". Harriot never did.

By 1614, Harriot had developed a spot in his right nostril, and he was examined in the following year by Sir Theodore de Mayerne, the eminent Swiss physician who was in England to see King James I; it shows something of the circles in which he moved that he was able to get this leading international doctor to examine him. Indeed, Harriot and Mayerne seem to have become friends. But the speck grew bigger; of course there was nothing that could be done, and he died on 1621 July 2, of what was almost certainly nasal cancer. He died in a house in Threadneedle Street, where the Bank of England currently stands.

Harriot was a major English mathematician, regarded in international circles as a great mathematician; but he was also the first person to look at an astronomical body through the telescope, draw it, record it, and communicate it to others, the 'Trefenty philosophers' in Wales.

The Vice-President. Thank you very much indeed, Allan; that was fascinating. There is time for questions.

Ms. Teresa Grafton. What was the Anthony Wood quotation?

Dr. Chapman. That was from Wood's *Athenae Oxonienses* edition of 1721. A wonderful quote!

Professor E. R. Priest. Without giving another talk, could you briefly say why you don't believe in the Tudor telescope?

Dr. Chapman. First of all, if there had been one around that worked, Harriot would have seen it — I have no doubt about that, considering the social circle he would have moved in; but there is no suggestion in any of his papers that he had come across one before. Also, the very language of modern optics is really formed after the telescope, so phrases like 'reflection angle' and 'refraction angle' tend to be found. Now when you look at the Tudor material, it's rather ambiguous: there is talk of 'glasses'. Now what is a glass? We think of a lens, but there is a reference which says "the best glasses are of steel". You have to bear in mind, too, George Herbert wrote in his poem, *The Elixir*, in about 1630: "A man that looks on glass, on it may stay his eye". Does this mean a glass you look into and reflect out of, or a glass you look through? I would suggest that the language of the rather vague descriptions of the Tudor telescope is riddled with ambiguity. Indeed, an excellent contemporary example of this linguistic ambiguity can be found in the passage in the Authorized Version of the Bible of 1611, *Exodus* 38:8, where the women of Israel donate their "looking glasses" to be melted down to make brass fittings for an altar; Egyptian bronze mirrors, no doubt.

Another very strong piece of evidence is that Thomas Digges was muster-master general for Kent, a very high office in the defence of the shores of Kent. He published a little booklet, of which there is a later copy in the Bodleian Library, called *England's True Defensitive* — how to defend England from invasion. He mentions all the normal stuff like pikes, muskets, and gunpowder, but he never mentions seeing the enemy from afar. Now some people have suggested this information was suppressed; but this was at the very highest levels, so it's a bit like saying Winston Churchill tried to suppress radar in 1940, and as we know, you cannot suppress something so fundamental. Things simply got out eventually. Bringing all these reasons together would suggest to me that, yes, there was an optical device — there was a whole fascination with what Tudor people called 'dioptrica' (seeing all sorts of images and colours and shapes) — but nothing you would call a telescope in the modern scientific sense.

Rev. G. Barber. If the telescopes were being made in Holland, were there any Dutch observers who used a telescope?

Dr. Chapman. Not directly, as far as we can tell, but we do have a rather curious comment from the summer or spring of 1609 by one of the people of the court of Louis XII, famous as a sort of 'gossip-record mentioner'; and he mentioned these Dutch glasses that you could see things out of, because he allegedly gave one to the British ambassador in Paris. After all, there were astronomers all over Europe, and of course Galileo makes use of it straight away. But the first major usage of the telescope was military–commercial — we often forget that Galileo first uses the telescope commercially. He had the telescope in 1609 June; he didn't use it for astronomy until five months later. He was trying first to sell it to the Republic of Venice to get himself a pay rise at his professorship in Padua; only when he had milked it commercially did he then start looking at the stars with it.

Also, Holland at the time was in the thick of a war of liberation from Spain, and most of the early references after 1608, when the telescope is first mentioned legally, are to military purposes. Hans Lipperhey, the first attempted patentee of the telescope, goes to the Estates General on October 2, tries to sell it to Prince Maurice of the Netherlands and to get a commission to make telescopes for the Dutch army and the Dutch navy. So the whole drive is essentially for military–commercial devices. Harriot, it seems, is the first one who has the space, the leisure, and the lack of financial initiative of necessity to observe the sky.

Professor J. D. Barrow. Harriot is generally attributed with the discovery of Snell's law of refraction several decades before Snell, although I guess it was known in ancient Islamic times as well. Did this discovery play any rôle in his telescopic work?

Dr. Chapman. Not directly that I'm aware of, but certainly there was this tremendous growth of optical culture across Europe, a whole body of people going back to people like Roger Bacon in the 12th Century. Snell's law was known but I don't think it had a part in this. According to the legend of the invention of the telescope, which is purely supposition as we have no further substantiation, it was children playing with lenses; and Lipperhey suddenly realized that that was useful, and that if you put them in a tube you can get something from the government for them! That seemed to be his motivation, and patriotically too of course, given the war in Holland. I don't think it came out of any higher theoretical drive. But we know that after the failure of the Dutch Estates General to give a patent to Lipperhey, the device quickly went public. We know that by Christmas 1608 they were on sale publicly in Frankfurt, simple devices with glasses at opposite ends of a tube, so the devices seem to have been very common. I don't think there was any prior theoretical stimulus, however.

Dr. G. Q. G. Stanley. In the portrait you showed of Thomas Harriot, what is he holding in his left hand?

Dr. Chapman. A pomander, an orange skin peeled very carefully, stuffed with aromatic spices, and stitched up. This was a highly odiferous age [laughter], especially in time of plague. London was in the grip of plague in 1603, one of the worst plagues before 1665, and it was widely believed that plague was communicated, in the Hippocratic medical theories of the day, by unseemly stinks. And one of the ways to drive off plague was to have a pomander or some kind of powerful aromatic and carry it around with you as a sort of antiseptic — if you didn't smell the nasty smells then you wouldn't take the fatal diseases. They were a common device.

The Vice-President. Thank you very much indeed, Allan. [Applause.] Our next talk is by David Strauss from Kalamazoo College, Michigan, and his title is 'Percival Lowell's long journey to Mars, 1883–1894.'

Professor D. Strauss. Percival Lowell's interest in extraterrestrial life grew out of his claustrophobic Boston youth and his belief in Herbert Spencer's philosophy of the cosmos. Bored by the routines of business and society and distraught by a failed engagement, Lowell plotted his escape. He embarked on a new career as a traveller and writer to cure his malaise by broadening his perspective. During his journeys, Lowell sought an intimate engagement with peoples and cultures different from his own, both to learn about them and to reflect on the nature of his own culture. He travelled for good, not for goods.

Though he was no stranger to extended stays overseas, Lowell began travelling in earnest with his first visit to Japan in 1883. His parents, descended from Boston's cotton aristocracy, raised their children with a respect for learning and culture. During a two-year sojourn in Europe, Lowell attended boarding schools as preparation for Harvard College and careers devoted more to cultural than business activities. Even so, he valued travel in East Asia far more than European sojourns, because the 'oddities' of the 'Orient' forced westerners "to criticize, examine, and realize [their] own way of doing things ... ." On his five journeys to Asia in eleven years, Lowell regretted the scarcity of unmapped terrain and so wrote instead about unusual cultural practices he encountered.

Among other achievements, he was one of the first westerners to write a book on Korean culture based on direct observations of that country (1886). In *The Soul of the Far East* (1888), Lowell constructed the first systematic account of East Asian culture based on an investigation of language, family structure, gardens, and art. Despite this impressive record, his writings reinforced the western stereotype of Asians as imitators, who lacked the mental capacity to engage in scientific activity, the foundation of western progress. Accordingly, Lowell saw no point in continuing his Asian travels.

Lowell's 1892 viewing of Giovanni Schiaparelli's 1877 map of Mars at the Harvard College Observatory convinced him to undertake a telescopic investigation of the geometrical markings on the planet's surface in order to confirm the existence of intelligent life. New to astronomy, he relied on two assistants who had served at Harvard College's station in Arequipa, Peru. In 1894 W. H. Pickering and A. E. Douglass advised Lowell to locate the observatory in Flagstaff, Arizona, in a high plateau area featuring dry air like Arequipa's. At the new observatory, astronomers would "sally forth into the untrod wilderness", an environment that was "fitting portal to communion with another world". Through the telescope, the astronomer would visit other worlds, exactly as Lowell's hero, Schiaparelli, had done. For discovering the canals of Mars, Lowell lauded his mentor as the 'Christopher Columbus' of astronomy.

The new project would again remove Lowell from Boston, while increasing prospects of learning about its society through contrasts with a distant civilization. The telescopic explorations of Mars, far more than travel to Japan, would provide "a sense of the possibilities of life for intelligent beings in the universe" and yield a "cosmoplanetary breadth of view".

Lowell's confidence that intelligent life was a strong possibility in the Solar System was informed by Herbert Spencer's system of cosmic evolution. In contrast to Darwin's limited application of evolution through natural selection to the development of organisms, Spencer and Lowell believed that the entire history of the Solar System could be "spun out of the original, homogeneous nebula".

The consequences of this belief were clear. Since the Sun and each planet in the Solar System developed from that original nebula, their constituents were similar, though present in different proportions. Furthermore, each planetary body was subject to the same governing forces, including the gradual cooling of the original gaseous mass which shaped the planet's surface. Of course, bodies of different sizes cooled at different rates, a fact that contributed to creating distinctive surface configurations. During the cooling process, moreover, each planet of sufficient size developed warm oceans conducive to nurturing simple organisms. Relying on the work of zoologists, T. H. Huxley and Ernst Haeckel, Lowell argued that these organisms developed from the original constituents of the nebula once combined in the proper proportions. From simple organisms complex creatures, including intelligent life, emerged as the environment changed.

Lowell was thus engaged in thinking scientifically and in an interdisciplinary fashion about the origins and development of life. Combining insights from biology and astronomy, he explored the conditions which were necessary to support life on any planet. In this way he modelled the approach of recent exobiologists.

By insisting on the possibility of extraterrestrial life, Lowell also fired the imagination of future astronauts. However, his speculations about the character of Martian life sabotaged his plan to learn by contrasting divergent civilizations. The Martians, who were saving a drying planet from extinction by designing a gigantic hydrographic system, resembled the builders of the Suez and Panama canals, who dealt with similar technical issues.

In fact, Lowell wavered in his quest to achieve a cosmoplanetary perspective. Despite his pretensions, he kept a house on Beacon Hill and enjoyed the company of his closest friends from Harvard. To be sure, he gave his sizable fortune to the distant Flagstaff Observatory, but his will also provided that a member of the family be chosen as its trustee. And, although Lowell arranged his own burial at the Observatory, a plaque affixed to a chunk of Arizona petrified wood announced the Flagstaff grave from within the family's cemetery plot in Cambridge, Massachusetts. Even Lowell's image of Mars as a techno-paradise was a projection of engineering solutions to Earth's problems; and his arrangement for governing the Observatory assured that other Bostonians would continue his efforts to civilize the Arizona wilderness. Lowell may have travelled to Japan, Mars, and Flagstaff, but, in important ways, he always returned to Boston.

Professor B. W. Jones. When Antoniadi, in the early 20th Century, more or less gave convincing evidence that canals on Mars did not exist, Lowell neverthless persisted in his belief until 1916, when he died. What counter-arguments did he give against the very convincing evidence that canals didn't exist?

Professor Strauss. Well, every time somebody challenged Lowell he was quick to emphasize the fact that the atmosphere at the Flagstaff Observatory was absolutely unique and that if astronomers wanted to prove their case they would have at least to come to Flagstaff and see what they could see out of the 24-inch Clark telescope. Second, he always talked about the special kind of eyesight that he was gifted with, which enabled him to see details on distant heavenly objects, so he had a line that was well developed! I don't know how many others believed what he was saying, that the canals might exist despite all the evidence.

Professor Jones. Did he also continue to refine his drawings of the canals on Mars right up until his death?

Professor Strauss. The best oppositions of Mars were in 1907 and 1909; between 1909 and 1916 the oppositions were not as favourable, and my impression is that he did a lot less observing. He did some observing and there were a few new canals discovered, but not many.

Mr. I. Ridpath. My understanding is that the seeing on Mars Hill isn't particularly good and that might be why he saw canals where other people saw disjointed blobs, but was Mars Hill his original prime site or did he have somewhere else in mind?

Professor Strauss. Not at all. When he sent A. E. Douglass, his assistant, out to Arizona, they planned together with Pickering an itinerary which Douglass would follow to test out various sites mainly in southern Arizona, which seemed more favourable because that was a drier area. Flagstaff, even though it is elevated, gets a fair amount of rain in different seasons. They tried various of these southern sites without success and Lowell got very impatient about it: the opposition was coming, and he came home from Japan in November of 1893 and the opposition was in November of 1894, and they wanted to get out there earlier than that to take advantage of the months leading up to the opposition. He didn't have a lot of time. So when the first few sites didn't work out and they got reports that Flagstaff was all right — but not enthusiastic reports — Lowell decided to put the observatory at Flagstaff. I think they came to regret that very soon thereafter — there was a lot of rain in the first year and they were unable to observe throughout the winter; and they picked up the observatory and moved it to Mexico in 1896. I think Flagstaff was a dubious choice and I don't think it's that highly regarded these days.

Professor D. Lynden-Bell. I understand that when he employed the Sliphers, one of the requirements was that your eyesight was good enough that you could see the canals on Mars! [Laughter.] Nevertheless, Lowell was also extremely enthusiastic when Vesto Slipher discovered the retreat of the nebulae and then discovered the rotation of the nebulae, and so Lowell wasn't just a 'man on

Mars'. Vesto Slipher became head of the Observatory when Lowell died, and was certainly a great man and the founder of the whole science of active galactic nuclei because he saw the broad lines in NGC 1068 and said they were so broad, he couldn't believe they were Doppler.

Professor Strauss. I never had heard the story told quite that way, about Lowell requiring that men could see the canals on Mars, but certainly V. M. Slipher's brother, E. C. Slipher, who was a photographer, continued to believe in the canals well into the mid-20th Century — he was still living and observing at that point.

Mr. L. Widdell. In general, would you classify Lowell as a scientist, philosopher, Renaissance man, or just touched in the head a little? [Laughter.]

Professor Strauss. All of the above? I think it's an interesting question. In my book I claim him as a polymath. I think he fits into the category of the Victorian polymath and I think he saw himself that way. But he saw what we might think of as various careers — a businessman, a traveller, a writer, and an astronomer — as part and parcel of each other. It's interesting to note that the Lowell Observatory was possible largely because Lowell was a good fund-manager and he developed a large fortune which went into the endowment of the Observatory. He was very gifted in making decisions about which telescopes to buy, and so on.

In criticizing Lowell for the failure of his projects, we should also remember that one of the major contributions he made was to create a solid observatory that's still well respected, even though the site is perhaps not so wonderful, and to encourage his assistants. He expected during the oppositions for them to be devoted to Mars; but he was very supportive of Slipher's work, and it's important to mention that the little respect that he did get from professional astronomers came as a result of the fact that Slipher was associated with the Lowell Observatory.

Dr. Chapman. One might suggest that in a way Lowell is an American version of many of the kind of people from whom this Society was founded, what I call 'grand amateurs' — those who made their money through finance or through marriage or through industry, had an excellent education behind them, and then perhaps in their 30s or 40s moved into very serious astronomy. Henry Draper was another American who ended up that way, so there was very much a parallel on opposite sides of the Atlantic, while there wasn't a similar parallel in, say, France and Germany where there was much more of a focussed university system. But I think America and Great Britain have this very close 'grand amateur' tradition, and Lowell was probably the last major grand amateur.

Professor Strauss. I agree with that completely, and I think another good example of an American astronomer who fits this category of the Victorian polymath was Simon Newcomb, who had a whole range of occupations and abilities, very much like Lowell.

Dr. Chapman. What I think, too, about Japan is that it's rather curious that Lowell had views about Japan as being non-progressive, because at that time quite a lot of Japanese young men were actually studying in Europe, particularly in engineering and medicine. There was the great Japanese bacteriologist Kitasato, who, along with a Frenchman called Yersin, when bubonic plague was raging in Indo-China in around 1900, discovered that the plague was actually communicated by a bacterium, and if you could break the bacterial vector then you could stop the disease. So there you have a Japanese bacteriologist who had studied with Pasteur and Koch in Europe, and it's rather curious that Lowell has this rather limited view of Japan.

Professor Strauss. I've thought a lot about that and I think I understand what was going on. Lowell was actually in Japan when the first constitution was promulgated, another example of Westernization, and when the railroads were being built, and in his book, *Noto*, he talks about travelling on the Japanese railroad and he's obviously very upset by this; I think he was upset because Japan was in fact showing it could be a modern country, and I think Lowell wanted to believe that the only way it could become a modern country was simply to imitate. So even though the Japanese were producing the technology of the West, he felt they weren't capable of any kind of original thinking; or so he supposed.

The Vice-President. Let's thank our speaker again. [Applause.] At this time of year it's good to remember the life and work of Michael Penston, by being privileged to hear from the recipient of the Michael Penston Astronomy-thesis Prize. This year, the recipient, Joern Geisbuesch from Cambridge, is going to talk on 'Cosmology with Sunyaev–Zel'dovich cluster surveys'.

Dr. J. Geisbuesch. Let me start by giving you an overview of the present state of cosmology. Due to the availability of new data, cosmology has advanced a lot in the last decade. These data (supernovae Ia, cosmic microwave background (CMB), galaxy distributions, *etc.*) seem to prefer a flat model with a present-day dominant cosmological constant and a matter-energy density of about 25% of the critical one, of which about a fifth is in baryonic matter. Furthermore, the root-mean-square of the mass-fluctuation amplitude on scales of $8h^{-1}$ Mpc, which measures the normalization of the matter power spectrum, is somewhat less than unity in this so-called consensus model. Clusters can help us to test, question, and further constrain this favoured model. The cluster-redshift number count depends strongly on the growth of structure in the Universe, the visible cosmological volume per unit sky area, and the large-scale distribution and overall occurrence of matter. All these properties are governed by the Universe's cosmology. The number–redshift distribution of clusters in the Universe is described by the cluster mass function, whose form has been theoretically predicted by Press–Schechter theory and related approaches and numerically confirmed and improved by cosmological simulations. We can thus calculate the cluster number above a limiting mass at a redshift for any assumed model and statistically compare the result to observations to obtain constraints on cosmological-model parameters. From such computations we find that galaxy-cluster-redshift number counts and their spatial clustering are very sensitive to the at-present still-weakly-constrained cosmological parameters of the matter variance and the equation of state of dark energy.

Observationally, clusters are detectable by several of their constituents: dark matter, galaxies, and their hot ionized plasma. Apart from detections in the optical, infra-red, and X-ray wavelength régimes, a very useful way to detect clusters is the thermal Sunyaev–Zel'dovich (SZ) effect caused by the distortion of the CMB spectrum *via* Compton scattering of CMB photons off the hot intra-cluster electron gas. The induced shift of photon energies leads to a decrement in CMB maps below ~ 220 GHz and an increment above this so-called crossing frequency. Since the SZ effect of clusters is almost redshift independent, it can yield an approximately mass-limited cluster sample. In recent years a large number of ground-based SZ survey instruments, such as the *Arcminute MicroKelvin Imager* (*AMI*), the *Atacama Pathfinder Experiment*, the *South Pole Telescope*, and the *Atacama Cosmology Telescope*, have become operational and have started surveying altogether several thousands of square degrees of sky for galaxy clusters. First results of blind SZ-cluster detections have appeared in the last months. Also the *Planck* satellite is shortly due for launch, which has — apart from measuring the primordial CMB — the detection of clusters *via* their SZ effect as a major science goal. [The *Planck* satellite was launched successfully on 2009 May 14.]

However, even though — as described before — the thermal SZ effect has a spectrally unique and well-understood signature, it is generally hidden in the observed sky images due to the presence of other competing components. There are several such components which can constitute significant contaminants in the observation of the effect. These are, for example, the primordial CMB itself, synchrotron, free–free, and dust emission from point sources, and our Galaxy, the Milky Way — just to name the most obvious. Some of the mentioned experiments have multiple wavebands, others only observe at a single frequency band. Moreover, instrumental effects such as detector noise and resolution affect the observations as well. Purpose-built SZ instruments operate at selected wavebands adequate to the SZ spectral dependence and have resolutions well matched to the angular sizes of galaxy clusters to overcome contaminating effects. Nevertheless, it is still a major effort to separate out the SZ effect and detect clusters. For this purpose, we have developed algorithms to extract the SZ cluster signal. In particular, we make use of Bayesian data-analysis methods, such as Markov Chain Monte Carlo and nested sampling techniques. Also, matched filtering and combined methods, including, *e.g.*, wavelet-based techniques on spectrally separated SZ component maps, have been successfully applied. These detection algorithms and methods have been tested and optimized on synthetic sky maps based on realistic cosmological simulations of the microwave sky. Benchmarks for ranking SZ-cluster extraction methods are the completeness (true cluster detections compared to total number of clusters in the field) and purity (true cluster detections compared to total number of detections) of the recovered cluster sample. Many advanced methods have been found after optimization to achieve comparable high-quality performances. For follow-up observations a high purity is especially of importance.

Furthermore, given the instrumental design and the detection algorithm, we can evaluate from these synthetic observations the cluster-redshift selection function and predict the total cluster number count of a survey strategy in an assumed cosmological model. The selection obviously depends on the instrumental set-up and the cluster-extraction-algorithm performance. With some prior information about the Universe's cosmology at hand, one can then optimize survey strategies in order to improve model constraints. For example, in the case of *AMI*, we find that, for a consensus model, surveying a sky patch of a few hundred square degrees per annum (observation time) is the most optimal strategy since it maximizes the number of detectable clusters and thus yields the tightest constraints. While approximately up to a hundred clusters can be found by such an *AMI* SZ survey, in the case of the *Planck* All-Sky Survey actually up to 2000 clusters are expected to be detectable in a consensus model. As a result *AMI* is able to yield good independent constraints on the mass variance, while a *Planck* cluster sample can tightly constrain this parameter and also obtain strong constraints on the nature of dark energy, *i.e.*, the equation-of-state parameter. Note that, apart from the assumption of reasonable prior knowledge of the Hubble parameter — as has been gained, for example, from the Hubble Key Science Project — and the accessibility of photometric cluster-redshift follow-up, these constraints are in any case independent of results obtained from other cosmological data sets.

Therefore, in conclusion, let me point out that clusters are a complementary and powerful tool for studying the cosmology of our Universe, since their number and physics are cosmology-sensitive. Moreover, SZ surveys especially provide — due to their cluster selection — a useful means to detect and study clusters over a large range of redshifts. Hence, many SZ-cluster surveys are up and running and are about to publish cosmologically relevant results.

The Vice-President. Questions for Joern?

Professor O. Lahav. It was popular for a while to use the Sunyaev–Zel'dovich effect to measure the Hubble constant. I think it became less believable because it tended to give values smaller than 70 km s^{-1} Mpc^{-1}. I wonder if you could use your simulations to comment on whether this method is still competitive?

Dr. Geisbuesch. If you know the morphology of the SZ clusters, such that they are fairly relaxed spherical systems and you have a large morphologically unbiassed sample, then you should be able to use this method — in short, you have to make assumptions about the third projected dimension to get a constraint on the Hubble parameter. Moreover, it has also been shown that cluster physics may — also in a systematic way — affect the estimate. To my knowledge there are some people who have in mind to use this method on up-coming data obtained by new instruments. The available sample of suitable clusters will be much larger than the ones which have been previously utilized to derive constraints. Let me also point out that hydrodynamical simulations have been used to investigate systematic biasses of this method and to explore why the previous SZ estimates of H_0 were lower than the value favoured today. For example, it has been found that — apart from the cluster geometry — assumptions about the temperature profile play a rôle as well. If one takes systematic effects into account the values obtained are consistent with present-day estimates from other data sets.

The Vice-President. You showed simulations of *AMI versus Planck*. *Planck's* got five times worse resolution so should do a worse job on clusters, but it seemed to be doing a much better job.

Dr. Geisbuesch. Well, the *Planck* survey area is much larger than the *AMI* survey patch. Actually, the *Planck* survey covers the entire sky. That way you pick up more clusters even though *Planck* observations are less sensitive to low-mass clusters. Furthermore, the *Planck* cluster sample will not extend to high redshifts — *Planck* will barely detect any clusters above a redshift of 1, and will only detect the really massive clusters which have formed relatively recently in the Universe.

The Vice-President. And are the *AMI* data going to be available soon? It has been funded since about 1985.

Dr. Geisbuesch. I don't know exactly when the data will become available. The instrument actually started being built at the beginning of the millennium. I should also point out that I am not much involved in SZ observations any more. At present, I work mainly on *Square Kilometer Array* (*SKA*) science within the *SKA* Design Study collaboration. A comment I should make is that SZ cluster observations are also important for my present work. Currently, my research focusses on the magnetic fields of galaxy clusters. At low radio frequencies, at which the *SKA* and its pathfinder instruments will be operating, cosmic magnetic fields in galaxy clusters can be detected by Faraday-rotation measures. Faraday rotation of the polarization plane occurs when polarized synchrotron emission traverses a magneto-ionic plasma. However, we have to disentangle the contribution from the ionized-gas density, which affects the rotation-measure value as well, from the amplitude of the regular magnetic field along the line-of-sight. For this purpose, we can use cluster SZ and X-ray observations, which yield information about the cluster thermal electron gas.

The Vice-President. Thank you very much, Joern. [Applause.]

Dr. Geisbuesch. I would also like to thank my PhD supervisor, and the RAS for awarding me the Michael Penston Prize.

The Vice-President. The next monthly A&G open meeting will be held on 2009 October 9, and the President and I wish you a pleasant summer.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 209: TWENTY SHORT-PERIOD ACTIVE-CHROMOSPHERE STARS

By R. F. Griffin
Cambridge Observatories

The recent publication of the *CABS3* catalogue[1] of active-chromosphere binaries has resulted in the author being alerted to the lack of orbits for a number of those interesting objects. That is partly remedied here. The orbits have short periods, and it has therefore been possible to determine them much more quickly (though admittedly less thoroughly in some cases) than most of those treated in this series of papers. Noteworthy cases are those of HD 31738, whose period of less than half a day is far shorter than any previously found in this series (and has eluded previous observers although it is traceable in measurements made 30 years ago), and HD 192785, which has a mass ratio of nearly 8 to 1 and appears still to be transferring mass now.

Introduction

This paper differs considerably in character from previous ones in this series. It deals with a relatively large number of stars in a manner that might be considered rather superficial, involving sometimes only a rather small number of observations, and covering a matter of months, rather than the decades that are usually spanned by the author's observations. All those differences have arisen, and may possibly be excused, by the natures of the stars concerned, which have all been identified as having active chromospheres and feature in *A Catalogue of Chromospherically Active Binary Stars, 3rd Edn.* (hereinafter called simply 'the

Catalogue'), by Eker *et al.*, who announced its availability in a paper[1] published in 2008 October. Orbits (47 of which have been provided by the present writer) are already known for some three-quarters of the 409 stars in the Catalogue, but in other cases the duplicity of the entries is inferred from eclipses, discordances in published radial velocities, or on other grounds.

The paper[1] includes a list of 22 entries that were considered to be single-lined spectroscopic binaries but which lacked orbits; one of them (HD 142680) had by chance been on the Cambridge observing programme since early in 2008. It promised to be a non-trivial task to determine which of the other entries were potentially double-lined objects lacking orbits, but Filiz Ak and Eker (two of the authors of the Catalogue) kindly supplied a listing of them at the author's request in 2008 November. Those that promised to be usefully observable with the Cambridge 36-inch reflector and *Coravel* radial-velocity instrument were immediately placed on the observing programme, perhaps in defiance of logical behaviour[2] on the part of the author but with the extenuating circumstance of the expectation of short orbital periods, whose tidal corollary is the rapid rotation which seems to be responsible for the activity of most active binaries. Thus the results presented here represent the fruit of only a very temporary supplementation of the main continuing observing programme. The present paper is an initial crop of such fruit, necessarily limited to systems that have short periods and have been adequately accessible to observation in the few months that have elapsed. Although the observations are in many cases fewer and inevitably less uniformly distributed in phase than is normally the case in papers in this series, the orbits are typically characterized by large amplitudes as well as short periods and so are nevertheless tolerably well determined. The principal motivation for continuing to observe a short-period system would usually be to refine the orbital period, but refinement has been possible already in the great majority of cases by the use of a few published measurements mostly taken about a decade ago.

Another departure from the usual character of these papers is that no introductory material is included to furnish the background of existing knowledge about the relevant objects. It is considered that a good deal of such material is already accessible in the Catalogue, so it is scarcely necessary to provide it here. Instead, the basic information relating to the identities, magnitudes, parallaxes (if known), and spectral types of the objects is listed in Table I; much of it is taken from the *Simbad* bibliographies rather than from the Catalogue, simply to spare the author from having to make choices, which could only be arbitrary, between the multiple and sometimes discordant data faithfully reproduced in the latter. Then the new (and any accessible pre-existing) radial-velocity data and the orbital elements derived from them are presented, together with any discussion that may seem warranted, for each system in turn. The discussion sections provide an opportunity to recall any particularly relevant information that may be in such literature as is not summarized in the Catalogue, and thus may redress to some extent the omission of the normal introductions.

The first column of Table I gives the object's serial number in the Eker *et al.* Catalogue; next comes the *HD* or *HDE* number (or in one case an *Einstein* designation), and then the variable-star designation (if any). For those stars that lack such a designation, their respective constellations are listed, in brackets, just to give an idea of whereabouts in the sky the objects are to be found. *Hipparcos* parallaxes are given where available, followed by the absolute magnitudes that they imply; in cases where the inferred magnitude has an uncertainty much over half a magnitude it is followed by a colon. Then a spectral type, as listed in the Catalogue, is given, followed by the number of recent Cambridge observations. Finally, for convenience, the table and figure numbers relating to each star are indexed.

TABLE I

Basic data for the 20 stars

No.	*HD/HDE*	*VS desig.*	*V* *m*	*(B−V)* *m*	*Parallax* *arc ms*	M_V *m*	*Sp. type*	*N*	*Table*	*Figure*
34	9902	BG Psc	8·71	0·63	—	—	G3 V–IV + G4 V–IV	16	2	1,2
45	16884	(Ari)	8·94	1·37	—	—	K0 III	14	3	3
79	283716	V1110 Tau	10·33	0·8:	—	—	K0 V + K3 V	10	4	4,5
87	31738	V1198 Ori	7·10	0·68	29·85 ± 1·04	+4·5	G6 IV + G1 V	21	5	6,7,8
135	62668	BM Lyn	7·73	1·10	4·97 ± 1·23	+1·2:	K0 III	24	6	9
148	73512	(Cnc)	7·91	0·90	39·28 ± 1·21	+5·9	K0 V	26	7	10,11
172	—	EQ Leo	9·39	1·09	3·19 ± 1·57	+1·7:	K0 III	25	8	12
182	93915	(UMa)	8·11	0·70	22·78 ± 1·00	+5·0	G5 V	24	9	13,14
183	237944	(UMa)	9·36	0·71	10·62 ± 2·11	+4·5	G5 IV	14	10	15,16
214	112099	(Vir)	8·23	0·86	38·12 ± 1·44	+6·1	G5 V	28	11	17
217	112859	BQ CVn	8·09	0·92	5·24 ± 1·16	+1·7	G8 III–IVp	25	12	18,19
218	—	CD CVn	9·39	1·19	3·31 ± 1·21	+2·0:	K0 III	28	14	20
243	127068	HK Boo	8·43	0·89	9·75 ± 1·28	+3·4	G5 IVe	25	15	21,22
265	142680	V383 Ser	8·71	0·95	28·23 ± 1·27	+6·0	K V	39	16	23
273	145230	PX Ser	9·30	0·98	5·83 ± 1·55	+3·1:	K2 V + K5?	23	17	24,25
282	150202	GI Dra	7·97	0·93	3·75 ± 0·69	+0·8	K0 III	34	18	26
318	2E 1848.1	+3305	10·70	0·75	—	—	K0 III–IV	38	19	27,28
347	191179	(Sge)	8·43	0·75	—	—	K0 IV + G2 IV	23	20	29,30
350	192785	(Cyg)	7·75	1·05	—	—	K3 IV + K2 IV	21	21	31,32
355	—	BI Del	10·60	0·89	—	—	K3:	22	22	33,34

The treatment of the individual stars starts with an informal table of the derived orbital elements and then includes the (usually quite short) journal of observations as soon as convenience in pagination permits. The dates (calendar and MJD) in the journals are all corrected to heliocentric timings. In each section there is a diagram to illustrate the orbit, and also, in the cases of double-lined objects, of a sample radial-velocity trace to show its character and in particular the similarity or otherwise of the two 'dips'. A brief discussion follows, referring to the solution and anything that may be concluded from it in the light of whatever else may be known about the system. The radial-velocity traces enable the rotational velocities, too, to be determined: mean values with the formal uncertainties estimated from the mutual agreement of the estimates made from the individual traces are given, but owing to the neglect of non-rotational sources of broadening the reader is cautioned that, however small the formal standard error may be, the true (external) error of the mean value is not to be expected to be better than ±1 km s^{-1}.

Very few radial velocities have been measured previously for the stars discussed here — indeed, that is why the orbits are not already known. In several instances, however, the periods found below are very usefully refined by the inclusion in the data sets of observations (made in pairs a few days apart, one from a red spectrum and the other from a blue) by Strassmeier *et al.*[3]. Wherever such measurements are used, they are always adjusted by +0·8 km s^{-1} in an effort to bring their zero-point closer to that of the Cambridge measures, in comparison with which they are routinely weighted ¼. This information is given here and not repeated for every star concerned.

HD 9902 (BG Psc)

P	=	25·366 ± 0·010 days	$(T)_2$	=	MJD 54814·830 ± 0·028
γ	=	+58·06 ± 0·09 km s^{-1}	$a_1 \sin i$	=	9·11 ± 0·08 Gm
K_1	=	30·32 ± 0·22 km s^{-1}	$a_2 \sin i$	=	9·02 ± 0·09 Gm
K_2	=	30·03 ± 0·28 km s^{-1}	$f(m_1)$	=	0·0469 ± 0·0012 $M_\odot$
q	=	0·990 ± 0·012 (= m_1/m_2)	$f(m_2)$	=	0·0456 ± 0·0014 $M_\odot$
e	=	0·508 ± 0·007	$m_1 \sin^3 i$	=	0·184 ± 0·005 $M_\odot$
ω	=	254·3 ± 0·6 degrees	$m_2 \sin^3 i$	=	0·186 ± 0·004 $M_\odot$

R.m.s. residual (unit weight) = 0·36 km s^{-1}

Fig. 1 reproduces a radial-velocity trace of HD 9902 and shows the appreciable inequality of the dips, both in their areas and in their widths. In the solution of the orbit, shown in Fig. 2, the radial velocities of the component (here designated the secondary) that gives the weaker and wider dip have been attributed half-weight, in order to equalize approximately the variances for the two components. The ratio of dip areas averages 1 to 0·76; the mean $v \sin i$ values are 3·7 ± 0·8 and 11·8 ± 0·6 km s^{-1} for the primary and secondary, respectively. At first sight it is, perhaps, a bit disconcerting that the star that we are calling the secondary is seen from the orbital elements to have marginally the larger mass.

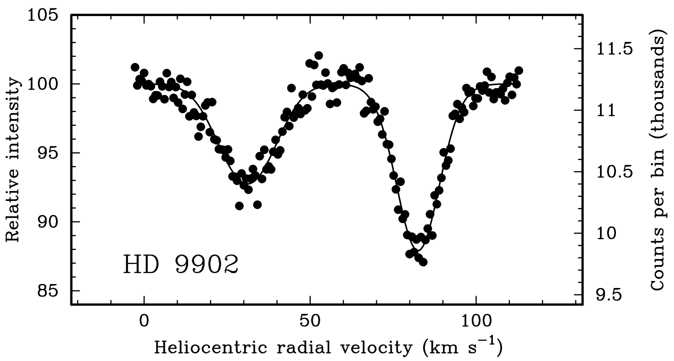

FIG. 1

Radial-velocity trace of HD 9902, obtained with the Cambridge *Coravel* on 2008 November 22 and illustrating the differing characters of the two dips.

In a table referred to by Strassmeier *et al.*[3] there are two radial-velocity observations obtained in 1998; one of them gives values for both components, the other is of a blend. They are consonant with the orbit found from the Cambridge data. It seems dangerous to try to improve that orbit by the addition of just one observation, since the listed uncertainties of 2 km s^{-1} (primary) and 4 km s^{-1} (secondary) are not encouraging. The Strassmeier points were therefore not included in the solution of the orbit, but they are plotted in Fig. 2. Taken at face value and included with full weight in the solution, they would indicate a marginal adjustment of the period from 25·366 to 25·373 days, a change well within the standard deviation of the Cambridge value.

Attention was drawn to the active nature of HD 9902 by its detection as an X-ray source[4] by *Einstein*; following up that detection, Fleming *et al.*[5] discovered it to be a binary system, listing it with the single spectral type of F9 V and single

$v \sin i$ value of 11 km s^{-1}. Favata *et al.*[6] attributed to the components identical spectral types, still F9 V, and identical temperatures of 5580 ± 100 K (which do not by any means agree with the type), and found similar, modest, lithium abundances which they claimed supported the assumption that the components are mutually similar. Tagliaferri *et al.*[7], who published a tracing of the spectrum

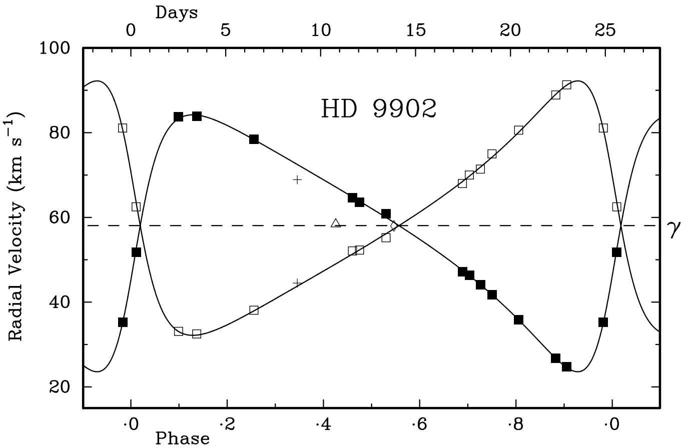

FIG. 2

The observed radial velocities of HD 9902 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Cambridge observations are represented by squares, filled for the primary and open for the secondary. One irresolvable blend, not used in the solution of the orbit, is plotted as an open diamond. Measurements by Strassmeier *et al.*[3] are shown as plusses, or as an open triangle where measured as single-lined. The same conventions are maintained in all the orbit diagrams in this paper, without repetition in every caption; only additions to them, or departures from them, are noted in later captions.

TABLE II

Cambridge radial-velocity observations of HD 9902

Date (UT)		*MJD*	*Velocity*		*Phase*	*(O – C)*	
			Prim. *km s^{-1}*	*Sec.* *km s^{-1}*		*Prim.* *km s^{-1}*	*Sec.* *km s^{-1}*
2008 Nov.	7·96	54777·96	+58·0		0·547	—	—
	11·94	781·94	+46·3	+70·0	·703	0·0	+0·3
	19·01	789·01	35·3	81·1	·982	+0·2	+0·3
	21·96	791·96	83·7	33·1	1·098	+0·4	0·0
	22·92	792·92	83·9	32·5	·136	−0·3	+0·3
	25·94	795·94	78·4	38·1	·255	0·0	+0·1
Dec.	2·91	802·91	60·9	55·2	·530	+0·9	−1·0
	6·94	806·94	47·2	68·0	·689	−0·4	−0·5
	7·89	807·89	44·1	71·4	·726	0·0	−0·5
	9·91	809·91	35·8	80·6	·806	0·0	+0·5
	11·87	811·87	26·7	88·9	·883	0·0	−0·2
	26·88	826·88	63·6	52·3	2·475	−0·2	−0·1
2009 Jan.	2·87	54833·87	41·8	75·0	2·751	+0·1	+0·8
	6·81	837·81	24·8	91·3	·906	+0·2	+0·1
	20·87	851·87	64·6	52·1	3·460	−0·2	+0·7
Feb.	3·82	865·82	+51·8	+62·5	4·010	−0·6	−1·1

of the system in the λ6700-Å Li I region, assessed the types by a process of photometric decomposition and found them to be K0 IV + F6 V, or alternatively G3 V + K0 V; subsequently Cutispoto *et al.*[8] (almost the same syndicate) favoured G9 IV + F5 V. They[8] seemed perturbed that the system showed a photometric variation whose period of 7·6 ± 0·27 days differed from the orbital one. They knew the orbital period because it had been published by Baker *et al.*[9], but those authors gave only the period and the eccentricity (as 25·364 days and 0·518) and no other elements or any indication of the uncertainties. Actually there seems no reason why the photometric period should be seen as posing much of a problem, since it is reasonably close to the rotation period (about 8·8 days) that could be expected on the basis of pseudo-synchronism[10] at the observed orbital eccentricity. The current Catalogue lists the projected rotational velocities of the components as 8 and 3 km s^{-1}. Tagliaferri *et al.*[7] gave them as 14 and 8 km s^{-1}; Cutispoto *et al.*[11], in a third paper from the same syndicate, gave 12 and 7. They altered their opinion of the spectral types to the rather oddly stated ones of G3 V–IV + G4 V–IV that were selected for inclusion in the Catalogue, and *then* they complained that "one of the two stars rotates faster [than the other], which cannot be attributed to a different radius. This is a quite unexpected result and could imply that such star itself is a spectroscopic binary." In still another bite at the cherry, Cutispoto *et al.*[12] again "note that the coolest component (which is probably an SB) rotates faster than the hotter component". It seems to the present writer, however, that there is no occasion to postulate any sub-system, which would have to be a remarkable one in view of the short period of the known binary.

If we assume that the rotations of both stars are pseudo-synchronized with the orbital revolution, and accept that their types are something like those that Cutispoto *et al.*[8] suggested, it seems understandable that the cool subgiant could be two or three times the size of the F dwarf while still being a little fainter in the violet, as the radial-velocity traces indicate. Its slightly greater mass would explain why it is beginning to evolve towards the giant branch of the H–R diagram before its companion. The values of $m \sin^3 i$ are little more than ⅛ of the mass to be expected of an F5 dwarf, so we could estimate that the orbital inclination is just over 30 degrees and the actual equatorial velocities of the stars are almost twice the observed $v \sin i$ values. Then, on the basis that they rotate pseudo-synchronously (or nearly so), in the observed photometric period — let us take, for the purposes of discussion, rotation periods of 8 days for both stars — the radii come out at about 3·5 and 1·2 $R_\odot$; the latter figure is quite as close to the radius of a mid-F dwarf as we have any right to expect on the basis of a $v \sin i$ estimate with an admitted uncertainty of ±20%, and the former seems reasonable for a late-G or K0 subgiant. The model described for the system seems quite self-consistent, and no enigma warranting any further complication is apparent. The radii of 1·16 and 2·02 $R_\odot$ given in the Catalogue for the cool and hot components, respectively, stand in extraordinary conflict with the 3·5 and 1·2 $R_\odot$ preferred here.

HD 16884

P	=	106·573 ± 0·019 days	$(T_0)_{-1}$	=	MJD 54648·71 ± 0·11
γ	=	3·12 ± 0·11 km s^{-1}	$a_1 \sin i$	=	43·38 ± 0·22 Gm
K	=	29·60 ± 0·15 km s^{-1}	$f(m)$	=	0·287 ± 0·004 $M_\odot$
e	≡	0			
ω	is undefined in a circular orbit		R.m.s. residual (wt. 1)	=	0·38 km s^{-1}

TABLE III

Radial-velocity observations of HD 16884

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
1998 Sept. 13·45*	51069·45	−21·9:	$\overline{35}$·411	+0·2
19·41*	075·41	−25·7	·467	+0·2
1999 Feb. 16·10*	51225·10	+23·9	$\overline{34}$·872	+0·2
2008 Oct. 19·03	54758·03	+32·2	0·026	−0·1
22·08	761·08	+31·0	·054	0·0
Nov. 26·04	796·04	−18·5	·382	+0·3
Dec. 2·99	802·99	−24·6	·448	+0·3
7·02	807·02	−26·8	·485	−0·4
9·94	809·94	−26·1	·513	+0·3
26·96	826·96	−11·6	·673	−0·9
2009 Jan. 2·87	54833·87	+0·7	0·737	−0·1
6·83	837·83	+7·9	·775	+0·2
18·90	849·90	+25·9	·888	+0·2
20·86	851·86	+27·9	·906	+0·2
29·86	860·86	+33·1	·991	+0·4
Feb. 10·83	872·83	+26·2	1·103	−0·5
21·83	883·83	+10·9	·206	−0·2

*Observation by Strassmeier *et al.*[3].

HD 16884, like HD 9902, was brought to attention through the discovery by *Einstein*[13] that it is an X-ray source. A systematic effort[14] to identify the optical counterparts of X-ray sources turned up HD 16884 as such an object, and it was classified from a low-dispersion optical spectrum as K5 V, a type that has been accepted by all subsequent authors, save for Strassmeier *et al.*[3], who referred to a table in which they proposed an absolute magnitude of $+0^{\mathrm{m}}{\cdot}5$; they also listed a $(B-V)$ colour index of $0^{\mathrm{m}}{\cdot}90$ which is entirely incompatible with the $1^{\mathrm{m}}{\cdot}34$ given by *Tycho* or the $1^{\mathrm{m}}{\cdot}37$ in *Simbad*. The only other things to note about HD 16884 are the mutually discordant estimates for $v \sin i$, of 18 km s^{-1} by Favata *et al.*[15] and the mean of 11 by Strassmeier *et al.*[3].

The orbit that has been determined here is circular, and is illustrated by Fig. 3. In addition to the Cambridge radial velocities, there are three available from Strassmeier *et al.*[3], dating from 1998/9. An initial solution of the Cambridge observations alone gave a period of 105·94 ± 0·35 days, from which the Strassmeier *et al.* points were systematically displaced by about –20 days. It was obvious that the incorporation of those points would improve the orbit, at least as far as the period was concerned, and their inclusion resulted in the adopted orbit whose elements are given above. The earliest of the three is listed by its authors with a bad uncertainty and its weight was divided by four in the solution of the orbit. Assessments were made of the significance or otherwise of the eccentricity, following the principle of Bassett's[16] second statistical test which compares the sums of the squares of the deviations when zero eccentricity is forced upon the solution and when e and ω are allowed as free parameters. When the Cambridge observations were used alone, the eccentricity was without significance at even the 10% point, but when the three early measurements were included it was not quite significant at the 5% level. The change arose mainly

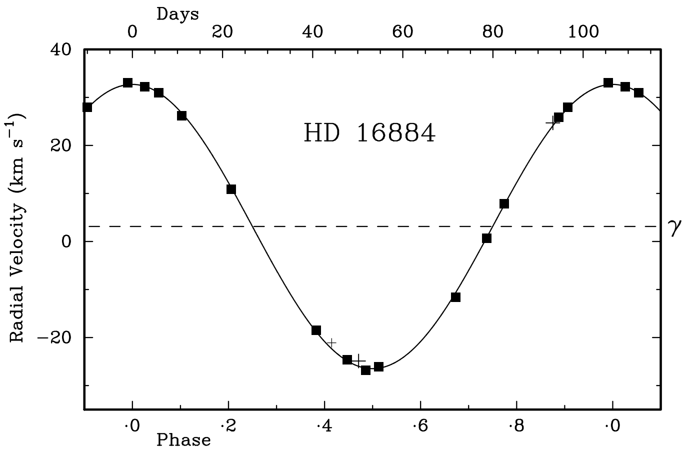

FIG. 3
Orbit of HD 16884.

through the worsening of the fit of the Cambridge velocities to a circular orbit when the Strassmeier observations were added. The decision was readily taken to adopt the circular solution, but it may be mentioned that the eccentric one showed an eccentricity of 0·015 ± 0·005 with $\omega = 282 \pm 17$ degrees.

There can scarcely be a doubt that HD 16884 is actually a giant, not a dwarf, K star; its colour index (*pace* Strassmeier!) then suggests a type of K4 III, but obviously that is not an actual *spectroscopic* classification and must not find its way into any listing of such. The evidence pointing towards the giant type is as follows. (*i*) The authors[14] who gave the K5 V classification noted that it implied a "spectroscopic parallax = 17 pc" — curious units for a parallax but one can understand what was meant! At that distance the transverse velocity represented by the proper motion of just over 0″·01 in each coordinate is only about 1 km s^{-1} — suspiciously small although obviously not conclusive. (*ii*) The orbit is circular (or at least we may say that it is not definitely eccentric), despite its period of ~100 days — natural enough for a giant but without an obvious explanation apart from coincidence in the case of a dwarf. (*iii*) The mass function of 0·29 $M_{\odot}$ would require the secondary star, if the primary is of type K5 V with a mass generously estimated at 0·7 $M_{\odot}$, to have a mass of at least 0·9 $M_{\odot}$; the only way in which it could be hidden in the system would be for it to be a binary itself, consisting of a pair of M dwarfs in an orbit having a period of not more than a few days. (*iv*) The radial-velocity traces yield a $v \sin i$ value of 9·1 ± 0·4 km s^{-1}, which finds a natural explanation as the projected equatorial velocity of the star rotating in synchronism with its orbital revolution, and implies a projected stellar radius, $R_{\star} \sin i$, of 19 $R_{\odot}$.

The large mass function poses quite a problem even when it is accepted that HD 16884 is a giant. If the primary mass is taken, for the sake of example, as 2 $M_{\odot}$, then the secondary has to be at least 1·5 $M_{\odot}$, corresponding to a main-sequence star in the early-F range or earlier. It is noticed that the two largest values among the areas (equivalent widths) of the dips in the 14 radial-velocity traces were those obtained when the velocity was nearest to the γ-velocity; that could easily imply that the dip of the supposed K4 III star was then being reinforced by an almost-coincident one arising from the secondary, but the enhancement was not great and could possibly be assigned merely to chance. The star is quite faint in the violet where the radial-velocity instrument operates, and it is hard to obtain traces of very good *S/N* ratio. After interest in the secondary was aroused, the observing season ended (the star became inaccessible) before it was possible to pursue the matter at a node of the orbit, which would be the most favourable time; a wide scan on 2009 February 26, when the primary was removed about 23 km s^{-1} from the γ-velocity and the secondary could be expected to be rather further from it in the opposite direction, showed *no* feature there, but there was a possible small depression, blended with the primary dip, in the vicinity of the γ-velocity itself. The matter clearly warrants further investigation.

HDE 283716 (V1110 Tau)

P	=	1·48460 ± 0·00013 days	$(T_0)_{46}$	=	MJD 54877·1430 ± 0·0017
γ	=	+9·12 ± 0·26 km s^{-1}	$a_1 \sin i$	=	1·032 ± 0·009 Gm
K_1	=	50·5 ± 0·4 km s^{-1}	$a_2 \sin i$	=	1·144 ± 0·016 Gm
K_2	=	56·0 ± 0·8 km s^{-1}	$f(m_1)$	=	0·0199 ± 0·0005 $M_\odot$
q	=	1·108 ± 0·018 (= m_1/m_2)	$f(m_2)$	=	0·0271 ± 0·0011 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	0·0981 ± 0·0032 $M_\odot$
ω	is undefined in a circular orbit		$m_2 \sin^3 i$	=	0·0885 ± 0·0021 $M_\odot$
			R.m.s. residual (unit weight)	=	0·80 km s^{-1}

The writer's embarrassment at finding this tenth-magnitude star to be on the faint side for convenient observation with the 36-inch reflector was mitigated somewhat when he came to look at the literature on it. Henry, Fekel & Hall[17] remarked upon its faintness and mentioned that, photometrically, it "has tested the observing limits of our telescope", and "it was, likewise, one of the faintest stars we observed spectroscopically and could be observed only on nights with seeing about 1″ or better" — and that was with the 38-inch coudé-feed telescope at Kitt Peak, where observing conditions are doubtless better than in Cambridge! A radial-velocity trace of the object appears here as Fig. 4, and well illustrates the unequal broad dips. The measurements are very few, but they are well distributed in phase; and since each observation provides two velocities and the number of unknowns for which a double-lined circular orbit has to be solved is only five, there is a tolerably comfortable number of degrees of freedom

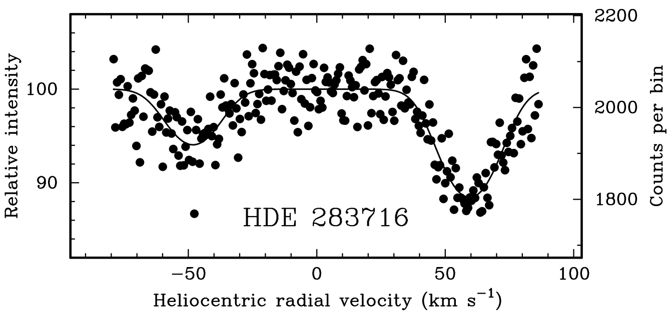

FIG. 4

Radial-velocity trace of HDE 283716, obtained on 2009 January 20.

remaining. In the solution of the orbit (illustrated in Fig. 5) the measurements of the secondary were half-weighted. The sum of the weighted squares of the deviations is 11·58 (km s^{-1})^2; it falls only to 9·84 if the imposition of zero eccentricity is relaxed, showing that the two degrees of freedom represented by e and ω cost scarcely more per degree than the 11 that remain in the solution computed with e free, so the eccentricity is definitely non-significant. It could hardly be expected to be otherwise, in any case, in an orbit of such short period. That period is, to well within its standard error, *one* of the shortest to have been documented so far in this series of orbits; the *other*, that of HR 6985[18], is, by an astonishing coincidence, exactly the same as far as the third decimal of a day, but takes the palm by being smaller by one in the fourth digit!

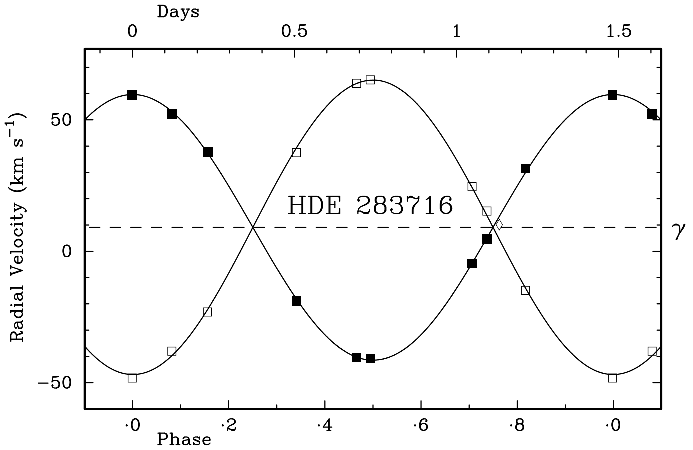

FIG. 5
Orbit of HDE 283716.

TABLE IV

Cambridge radial-velocity observations of HDE 283716

Date (UT)	*MJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O−C) Prim. km s⁻¹*	*(O−C) Sec. km s⁻¹*
2008 Dec. 9·984	54809·984	+10·1		0·763	—	—
2009 Jan. 20·903	54851·903	+59·5	−48·2	28·999	−0·2	−1·3
29·933	860·933	+52·3	−38·0	35·081	−0·9	+1·8
30·907	861·907	+4·6	+15·3	·737	−0·5	+1·7
Feb. 3·772	865·772	−18·9	+37·5	38·341	−0·8	−1·8
3·958	865·958	−40·4	+63·9	·466	−0·1	0·0
10·922	872·922	+37·8	−23·1	43·157	+0·7	−0·9
11·903	873·903	+31·5	−14·9	·818	+1·6	−0·9
Mar. 23·822	913·822	−4·6	+24·6	70·706	0·0	+0·3
Apr. 8·838	929·838	−40·7	+65·2	81·494	+0·7	+0·1

In view of the interest that has been shown in HDE 283716 in the literature, it is surprising that its orbit has not been published previously. Straižys & Meistas[19] improved on the *HDE* [20] type of G0 by making a 'photometric classification' of K1 V. Walter[21], who dubbed the star 'Tau 1', a name that has stuck and been used by several subsequent authors, gave its magnitudes as $V = 10^{m}\!\cdot\!36$, $(B-V) = 0^{m}\!\cdot\!98$, $(U-B) = 0^{m}\!\cdot\!58$, and said that its "spectrum is that of a lightly reddened [$E(B-V) = 0\cdot1$] K0 IV." Martin *et al.*[22] found it somewhat bluer, with colour indices of $0^{m}\!\cdot\!94$ and $0^{m}\!\cdot\!49$. The brightness is considerably variable, no doubt owing to BY Dra activity (starspots); that was first documented by Grankin[23], who proposed a photometric period of 3·06 days. That is actually an alias of the orbital period, which must be the true one — the reciprocals of the two periods add up very exactly to 1 day^{-1}. Grankin, with collaborators, subsequently published[24] another account, which showed that the star was considerably brighter than before, its magnitude varying between $10^{m}\!\cdot\!06$ and $10^{m}\!\cdot\!20$, and it had at the same time become considerably bluer, with $(B-V) = 0^{m}\!\cdot\!86$. On the basis of the aliased rotation period of 3·06 days plus the erroneous classification[21] of K0 IV, they produced an equatorial rotational velocity of 32·6 km s^{-1} that is not far from the probable truth! Quite recently Grankin's syndicate has presented a further description[25] showing the results of photometry in ten of the twelve consecutive years 1993–2004, in which the mean magnitude declined almost monotonically from year to year, with fluctuations typically of a tenth of a magnitude in any given season; in the final year it reached close to the magnitude ($10^{m}\!\cdot\!36$) that Walter[21] gave for it in 1986.

It was Martin *et al.*[22], writing in 1994, who first asserted in print the double-lined nature of HDE 283716; they referred to a private communication from Mathieu in 1992 saying that it has a short orbital period. Henry, Fekel & Hall[17] found the true photometric period to be 1·487 days, and not only thought synchronous rotation was "likely" but noted a private communication from Mathieu & Torres in which they said that the orbital period was the same. Henry *et al.*[17] gave the spectral types of the components as K0 V and K3 V, their line-strength ratio as 0·67, and the $v \sin i$ values for both of them as 16 ± 2 km s^{-1}. Later, Fekel[26] gave the rotational velocities as 18·8 and 19·5 km s^{-1} for the A and B components, respectively.

There are seven unblended radial-velocity traces that allow the parameters of the two dips to be independently assessed. They show their areas to have a mean ratio of 1 to 0·45. Since both stars are K dwarfs, their spectra may be supposed to match the Arcturus spectrum that formed the basis for the design of the mask in the *Coravel* about equally well, so the directly corresponding ratio, of slightly less than a magnitude when expressed in that way, is the estimate of the difference in luminosity between the components in the wavelength band, approximately B, in which the *Coravel* operates. The mean $v \sin i$ values are 19·3 ± 0·7 and 15·7 ± 1·4 km s^{-1} for the primary and secondary, respectively.

Both the magnitude difference and the mass ratio are consonant with the spectral types of K0 and K3 V proposed by Henry, Fekel & Hall[17]; the colour indices, too, though somewhat variable, are agreeable to those types. Since the actual mass m_1 of the primary must be expected to be about 0·8 $M_\odot$ and the orbit shows $m_1 \sin^3 i$ to be just 0·1 $M_\odot$, we find $\sin i$ to be 0·5 and the orbital inclination is thus 30°, with little uncertainty in view of the cube-root dependence of $\sin i$ on the other data. Thus, if we relieve the observed projected rotational velocities of the components of the projection factor of one-half, they become equatorial velocities of about 39 and 31 km s^{-1}; multiplying them by the orbital period expressed in seconds and dividing by 2π, we obtain the radii of the stars as about 800 000 and 630 000 km, respectively — about 1·14 and 0·91 $R_\odot$. The expected values for stars of the supposed types are[27] about 0·85 and 0·79 $R_\odot$. The only plausible way of reducing sufficiently the radii implied observationally is to suppose that the $v \sin i$ values have been over-estimated: they 'should' be about 14·7 and 13·5 km s^{-1}. The uncertainty of the value for the secondary could just about accommodate the desired number, but the discrepancy in the case of the primary is quite definite. We notice that our $v \sin i$ measurement for the primary is fully supported by Fekel[26], whose result for the secondary is substantially *further* from the 'proper' value than ours. We can conclude only — though not very confidently — that the stars really *are* somewhat bigger than ones of those types are supposed to be, or (even less probably) that their masses are very considerably less than we have assumed.

HD 31738 (V1198 Ori)

P	= 0·4502588 ± 0·0000006 days*	$(T_0)_{141}$	= MJD 54841·1589 ± 0·0021
γ	= +10·32 ± 0·18 km s^{-1}	$a_1 \sin i$	= 0·0314 ± 0·0015 Gm
K_1	= 5·06 ± 0·24 km s^{-1}	$a_2 \sin i$	= 0·171 ± 0·005 Gm
K_2	= 27·6 ± 0·8 km s^{-1}	$f(m_1)$	= 0·0000061 ± 0·0000008 $M_\odot$
q	= 5·45 ± 0·30 (= m_1/m_2)	$f(m_2)$	= 0·00098 ± 0·00008 $M_\odot$
e	≡ 0	$m_1 \sin^3 i$	= 0·00138 ± 0·00011 $M_\odot$
ω	is undefined in a circular orbit	$m_2 \sin^3 i$	= 0·000253 ± 0·000023 $M_\odot$

R.m.s. residual (unit weight) = 0·75 km s^{-1}

*The true period, in the rest-frame of the system, is 0·4502433 ± 0·0000006 days. It differs from the observed period by 25 standard deviations.

HD 31738 has been an enigma for a long time: it has more than 50 citations in *Simbad*, and it is known to be double-lined, but nobody has managed to determine the orbital period. The reason for such failure is, clearly, that the period is unexpectedly, indeed astoundingly, short. It is shorter by a factor of more than three than that of the immediately preceding star treated above, which itself has one of the two equal-shortest periods of any object yet treated in this series. The only reason that its period can be so short without the projected rotational velocity being large enough to smear the spectrum out to such an extent as to make radial-velocity measurement practically impossible must be that it has an unusually small axial inclination.

The star drew attention to itself by its high activity, manifested by strong emission in the *H* and *K* lines; it was discovered by Bidelman & MacConnell[28], who classed it as G5 IV, in a preliminary survey of objective-prism plates taken with the University of Michigan's *Curtis Schmidt* telescope after it was moved to Cerro Tololo in 1966. (HD 31738's declination is just half a degree north of the celestial equator.) It is such an active object that it features in a list[29] of the 100 brightest X-ray stars within 50 pc of the Sun. The *PASP* has twice published[30,31] actual pictures of the star (enlargements of a Schmidt plate, on different scales) in 'optical atlases' of X-ray and far-UV sources, respectively — not very informative, really!

There has been quite a number of more or less determined photometric campaigns (five are listed by Strassmeier *et al.*[32]) aiming to elucidate a variation of the BY Dra or RS CVn type, but the only one to have proposed a period was that of Strassmeier *et al.*[33], who found a 4·55-day variation (probably an alias of the orbital period, since the sum of the frequencies is 2·0011 day^{-1}) in 1984–86. Since then nobody seems to have found periodic changes, but Strassmeier *et al.*[32] saw what appeared to be a slow and monotonic decline of the order of $0^{m}\cdot1$ in 1993–96 and suggested a possible period of about 15 years.

Still more troublesome has been the situation concerning radial velocities. At the SAAO, after one photographic spectrogram had been obtained[34], Balona[35] obtained no fewer than 28 measurements with his photoelectric radial-velocity spectrometer (operating on the same principle as *Coravel*) on the former Radcliffe 74-inch telescope after its removal to the SAAO out-station at Sutherland. He found the spectrum to show broad lines, with a $v \sin i$ of 17 km s^{-1}; the velocities did not agree very well with one another (r.m.s. dispersion 3·3 km s^{-1}), but no period could be found, and he concluded that HD 31738 was a single star. Fekel, Moffett & Henry[36] similarly found small velocity variations and a line broadening of 17 km s^{-1} in spectra taken at McDonald and KPNO. They listed five velocities, but mentioned that "several observations show asymmetric absorption lines probably due to an unresolved secondary component". Randich, Gratton & Pallavicini[37], who obtained a spectrum in the red, showing no detectable lithium line, reported the $v \sin i$ to be 30 km s^{-1}. Fekel[26] later lamented that, "Although numerous additional spectra have been obtained, the system is quite difficult to analyze because the line broadening of both components is significant, and the velocity variations are small, resulting in lines that are always blended. The orbital period has not yet been determined." He gave projected rotational velocities for the two components as 21·7 and 8·9: km s^{-1}. Nordström *et al.*[38], in their large catalogue of F and G stars, reported that they had 19 *Coravel* radial-velocity measurements, but the only information that they gave about them was that the mean velocity was +7·2 km s^{-1} and the r.m.s. spread 5·0 km s^{-1}. Cutispoto[39] made an effort to classify the components by a photometric decomposition procedure, suggesting types of G6 IV + G1 V; later, with collaborators[40], he gave a more circumspect revision to 'G5/6 IV + ?', thereby effectively returning to the type given in the first place by Bidelman & MacConnell[28].

When the writer came to observe the system with the Cambridge *Coravel*, he naturally encountered the same problems as everyone else, finding that the radial-velocity traces showed asymmetrical dips which changed in a seemingly haphazard fashion from night to night. Eventually he tried to get a handle on the rate of variation by resorting to taking two observations on the same night. At the unfavourable declination of the star (for an observer above +52° latitude), the Cambridge telescope does not offer a very large range of hour angle, literally running into the floor at hour angles of $2^{h}\ 40^{m}$ east and west, but in order to allow as much time as possible for any variation to take place between the observations he managed to obtain them about 4½ hours apart. Imagine his astonishment at finding that, in the interim, the system had gone almost from one node of the orbit to the other! Thus alerted, he was able immediately to determine the orbital period, of only $10^{h}\ 48^{m}$. Fig. 6 shows an observation deliberately obtained, after the orbit was known, exactly at a node, and shows the best resolution that is ever to be seen in such a radial-velocity trace. Most of the time the observations show the two dips badly blended together, and without the clues that the traces taken at the nodes give as to the profiles of the dips they would be effectively irreducible. In the solution of the orbit, the velocities corresponding to the weak secondary dip have had to be weighted only ⅒ to bring the variances for the two components into approximate equality. The orbit is portrayed in Fig. 7. It is, surely, no more than a formality to check that an orbit of such a short period is truly circular, and indeed it passes the usual statistical test with flying colours.

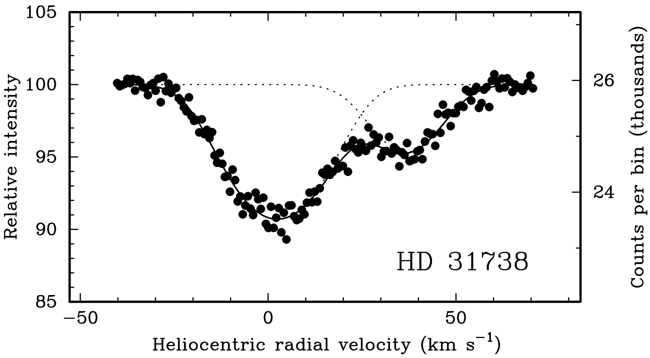

FIG. 6

Radial-velocity trace of HD 31738, obtained on 2009 February 10·896, right at a node of the orbit.

There are, of course, no previously published double-lined radial velocities for HD 31738; there are a few relatively isolated measurements (including those of Fekel *et al.*[36] mentioned above) reduced as single-lined, but the only substantial series is that of Balona[35], dating from thirty years ago. It readily shows (Fig. 8) the variation once it is told the approximate period, although naturally the amplitude is muted because the velocities were measured as single-lined. A completely independent orbital solution, made on the 28 Balona velocities

TABLE V

Cambridge radial-velocity observations of HD 31738

Date (UT)	*MJD*	*Velocity Prim.* *km s⁻¹*	*Velocity Sec.* *km s⁻¹*	*Phase*	*(O – C) Prim.* *km s⁻¹*	*(O – C) Sec.* *km s⁻¹*
2008 Nov. 8·115	54778·115	+16·1	−17·7	0·983	+0·7	−0·6
19·082	789·082	+7·0	+22·4	25·340	−0·6	−2·7
23·082	793·082	+10·9	+5·7	34·224	−0·2	−0·1
Dec. 3·012	803·012	+9·2		56·278	—	—
7·091	807·091	+7·2	+29·8	65·337	−0·5	+5·1
10·035	810·035	+13·7	−8·0	71·876	−0·2	+1·3
27·008	827·008	+5·6	+33·0	109·572	−0·2	−2·2
2009 Jan. 2·977	54833·977	+15·4	−16·9	125·049	+0·3	−0·9
6·917	837·917	+10·0		133·800	—	—
13·957	844·957	+5·1	+37·6	149·435	−0·6	+1·9
18·889	849·889	+8·1	+26·3	160·389	+1·7	−5·2
19·931	850·931	+8·2	+19·1	162·703	−0·7	+0·8
20·944	851·944	+15·4	−17·9	164·953	+0·2	−1·8
23·928	854·928	+6·3	+34·7	171·580	+0·4	+0·2
29·763	860·763	+6·4	+34·6	184·540	+1·0	−2·5
29·952	860·952	+15·7	−18·0	·959	+0·5	−1·6
30·924	861·924	+13·0	−8·0	187·118	−1·1	+2·0
Feb. 10·878	872·878	+5·7	+37·9	211·446	+0·2	+1·5
10·896	872·896	+3·9	+38·2	·486	−1·4	+0·4
11·866	873·866	+8·3	+29·1	213·641	+1·2	+1·3
21·822	883·822	+9·7	+12·7	235·752	−0·7	+2·8

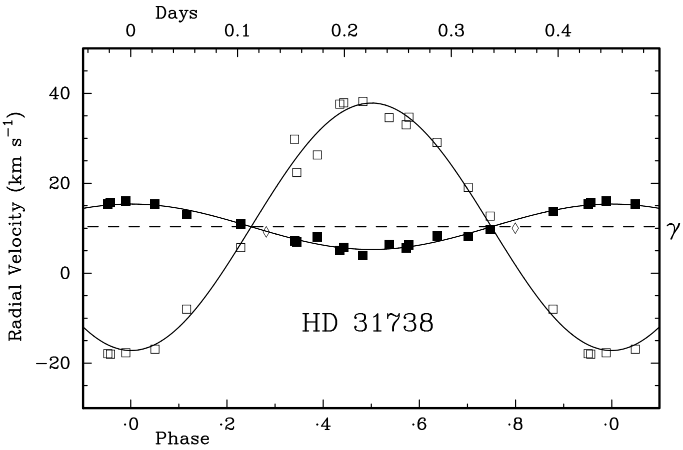

FIG. 7
Orbit of HD 31738.

alone, yields the period as 0·45028 ± 0·00004 days. The reality of the solution may be gauged from the fact that it — involving the fitting of four parameters — reduces the sum of the squared deviations to 181 (km s^{-1})^2 from the 312 obtained from the straight mean, whose fitting costs one degree of freedom. Thus the extra three degrees of freedom gained by not-fitting the orbit cost 131 (km s^{-1})^2 while the other 24 cost 181, so $F_{3,24}$ = (131/3)/(181/24), about 5·8 — well beyond the 1%-significance point, which is 4·72.

The phasing of the variation could be expected to agree with that of the *secondary* in the writer's orbit, since the secondary, though being considerably weaker than the primary, has more than five times the amplitude of variation. Despite the comparative imprecision of the determination of the T_0 epoch of the old velocities, the hundredfold increase that they offer in the time base enables

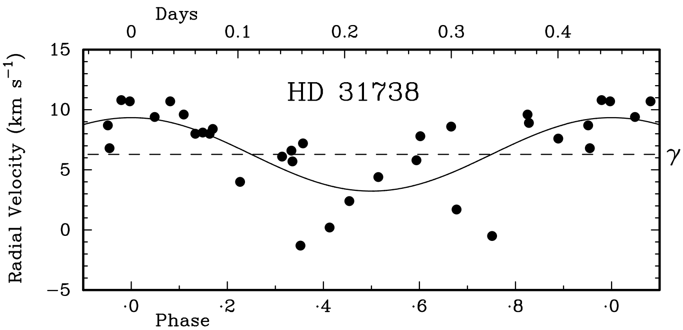

FIG. 8

Radial velocities of HD 31738, obtained photoelectrically by Balona[35] with the Radcliffe 74-inch telescope in 1979/80, reduced as single-lined, and used here in a totally independent solution of the orbit.

their phase discrepancy to be turned to account to refine the orbital period derived from the Cambridge observations alone, provided the correctness of the cycle count could be assured. Although the standard error of the Cambridge period was only 29 millionths of a day, that is about one fifteen-thousandth of the period and so creates a phasing uncertainty of much more than a whole period at the epoch of the Balona observations about 25 000 cycles ago. The writer brought the orbital period to the attention of Dr. Fekel, who was able to confirm and refine it from his own data spread over a number of years, and graciously permitted his figure of 0·4502604 ± 0·0000009 days to be utilized here to identify the cycle count between the recent measurements and Balona's. The combination of the Cambridge and Balona data refines the orbital period to 0·4502588 ± 0·0000006 days — good to one-twentieth of a second! The final orbital elements, tabulated above, were computed from the Cambridge observations alone but with that period imposed upon the solution.

It is apparent from the mass ratio that in HD 31738 we are not dealing with a pristine binary but one in which there has been a lot of mass exchange, so we cannot model the system by appeal to the tabulated properties of normal stars. We can, however, calculate the projected stellar radii from the observed rotational velocities, which are 22 and 9 km s^{-1}, the latter value being rather uncertain; they come out at 0·20 and 0·08 $R_\odot$. It must be significant that the sum of the projected rotational velocities is equal, to well within the observational uncertainties, to the sum of K_1 and K_2 — the projected orbital velocities — showing that the stars must be touching one another. The ratio of the dip areas of the two components is 1 to 0·4, so if the stars are of comparable colours there is a brightness difference of one magnitude between them.

Possibly the most plausible way of estimating the orbital and axial inclination of the system is from the absolute magnitude, which the parallax shows to be about $4^{m}\cdot 5$ — not much brighter than the Sun. In fact, when allowance is made for the contribution of the secondary, which at one magnitude fainter raises the combined brightness to $0^{m}\cdot 3$ above that of the primary alone, the luminosity of the latter is seen to be practically equal to that of the Sun. Classifiers have agreed that it has a mid-G spectrum, so it must be a little cooler and a little larger than the Sun, say 1·2 $R_\odot$ or six times the projected radius found in the preceding paragraph, showing that $\sin i \sim 1/6$ or $i \sim 10°$. That is not a very welcome conclusion, because it implies that the distance between the centres of the two stars is six times the sum of the values of $a_1 \sin i$ and $a_2 \sin i$ shown in the informal table above, or 1·2 Gm (0·008 AU). Inserting that quantity, and the period of 0·00123 years, into the Keplerian equation $m = a^3/P^2$, where all the quantities are expressed in Solar System units, we obtain m, the sum of the masses, to be only about 0·34 $M_\odot$. It might agree better with prejudice if the inclination were little more than half the figure we have just found, to make the sum of the masses about 2 $M_\odot$. The primary would then be about 2 $R_\odot$ and thereby merit, more or less, the luminosity class IV that has been repeatedly attributed to it; but that would make the system far too bright to be consonant with the measured parallax.

It may be noted parenthetically that a high-resolution observation of the *spectrum* could be better resolved than the profile seen in Fig. 6, because the resolution in the radial-velocity trace is degraded by a factor of the order of $\sqrt{2}$ by the cross-correlation with the mask, which has apertures of widths comparable with that of the entrance slit of the instrument. Further elucidation of this interesting system should therefore not be too difficult now that observations can be scheduled at optimal times, as for Fig. 6.

HD 62668 (*BM Lyn*)

P	=	69·319 ± 0·005 days	$(T_0)_0$	=	MJD 54807·69 ± 0·05
γ	=	−15·83 ± 0·10 km s^{-1}	$a_1 \sin i$	=	29·60 ± 0·15 Gm
K	=	31·06 ± 0·15 km s^{-1}	$f(m)$	=	0·216 ± 0·003 $M_\odot$
e	≡	0			
ω	is undefined in a circular orbit		R.m.s. residual (wt. 1)	=	0·47 km s^{-1}

The Cambridge observations of this single-lined object yield a period of 69·28 ± 0·08 days. There are two radial-velocity measurements in the literature, by Strassmeier *et al.*[3]; they are slightly displaced in phase when plotted on the Cambridge orbit, and when incorporated they refine the period to the value listed in the informal table above. A check for circularity shows the eccentricity to be not-significant at the 10% level. The orbit is shown in Fig. 9.

The nature of HD 62668, a star that was already listed in the *New Catalogue of Suspected Variables*[41], was greatly elucidated by Henry, Fekel & Hall[17] in 1995. They showed that it not only exhibited RS CVn-type variations, sometimes with a very large amplitude (extreme range 0^{m}·25), but also eclipses, with a period of 69·7 days. They also noted (though without giving any data or additional information) that spectroscopy showed it to have an orbital period of 69·3 days,

TABLE VI

Radial-velocity observations of HD 62668

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity km s^{-1}*	*Phase*	*(O–C) km s^{-1}*
1999 Feb. 12·26*	51221·26	−17·1	$\overline{52}$·262	+1·0
15·27*	224·27	−27·5	·305	−1·1
2008 Dec. 27·10	54827·10	−21·9	0·280	−0·3
2009 Jan. 3·09	54834·09	−38·2	0·381	+0·4
6·11	837·11	−43·8	·424	−0·4
14·04	845·04	−46·6	·539	−0·6
21·04	852·04	−36·2	·640	−0·5
24·09	855·09	−28·3	·684	+0·1
Feb. 4·05	866·05	+1·4	·842	+0·3
8·03	870·03	+8·1	·899	−1·1
10·98	872·98	+12·8	·942	−0·4
11·95	873·95	+13·7	·956	−0·3
13·98	875·98	+14·8	·985	−0·3
21·91	883·91	+10·0	1·100	+0·7
Mar. 6·04	896·04	−20·8	·275	−0·2
20·96	910·96	−47·2	·490	−0·4
27·89	917·89	−41·5	·590	+0·6
Apr. 5·87	926·87	−21·3	·719	+0·5
7·83	928·83	−15·3	·748	+1·0
8·87	929·87	−13·3	·763	+0·1
29·86	950·86	+12·9	2·065	+0·3
May 3·90	954·90	+6·5	·124	+0·2
6·86	957·86	−0·2	·166	0·0
8·90	959·90	−5·5	·196	0·0
10·89	961·89	−10·7	·224	+0·2
16·88	967·88	−27·1	·311	+0·3

*Observation by Strassmeier *et al.*[3].

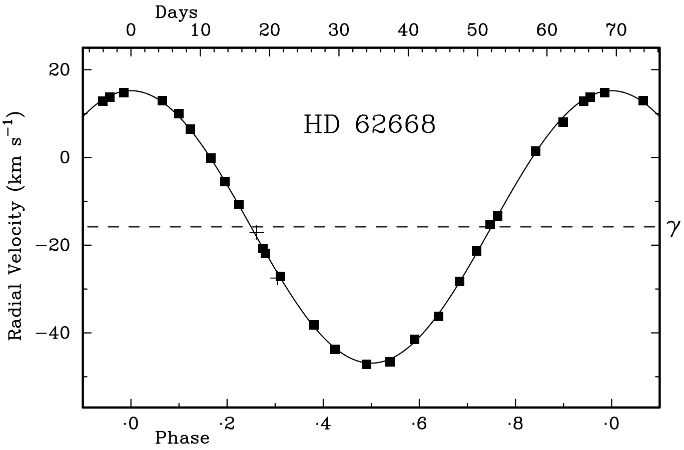

FIG. 9
Orbit of HD 62668.

a circular orbit, and a projected rotational velocity of 14 ± 2 km s^{-1}. Knowing from the existence of eclipses that the orbital inclination — and implicitly the axial inclination — must be near 90°, they inferred a stellar radius of 19·3 $R_{\odot}$. Fekel[26] has given the rotational velocity as 16 km s^{-1}. Strassmeier *et al.*[3] found the photometric period to be 34·59 days, just half the orbital period, but it is not clear whether that was a misinterpretation of a double-waved RS CVn variation. Koen & Eyer[42] detected in the *Hipparcos* epoch photometry a variation with a frequency of 0·01459 day^{-1}, *i.e.*, a period of 68·5 days. King *et al.*[43], in an investigation of the Ursa Major 'moving group', gave HD 62668 scant chance of membership, but identified it as one of seven stars that might constitute an older group; subsequently Daane *et al.*[44] investigated that group but concentrated their observations on only four of the stars, not including HD 62668.

The mean $v \sin i$ value given by the Cambridge traces is 15·6 ± 0·3 km s^{-1}; with the axial inclination assumed to be 90° it gives a stellar radius of 21·4 $R_{\odot}$, which (like the $(B-V)$ colour index) is very much in line with the classification of K0 III but might suggest an absolute magnitude somewhat brighter than the rather uncertain value near +1 indicated by the parallax.

The mass function would demand a secondary of mass 1·35 $M_{\odot}$ if the primary is supposed to be 2 $M_{\odot}$; it suggests about an F4 main-sequence star. The eclipse depths are reported[17] as $0^{m}{\cdot}08$ in V and $0^{m}{\cdot}15$ in B. It is not clear whether the eclipses are total, but owing to the great disparity in the sizes of the two stars the statistical probability of an eclipse being total, if it occurs at all, is high, so for the purposes of discussion we will assume a total eclipse. Then the eclipse depth of $0^{m}{\cdot}08$ in V shows that the secondary has about 8% of the brightness of the primary, *i.e.*, $\Delta m_V \sim 2^{m}{\cdot}8$, so if the secondary is F4, with $M_V \sim 3^{m}{\cdot}2$, then the primary would be about $+0^{m}{\cdot}4$. An analogous calculation (or reference to the useful tabulation in the *Skalnaté Pleso Atlas*[45]) shows that $\Delta m_B \sim 2^{m}{\cdot}1$, implying that the secondary is $0^{m}{\cdot}7$ bluer than the primary in $(B-V)$ — just as would be expected between K0 III and F4 V. The contribution of the much hotter secondary star to the combined light of the system outside eclipse means that the real colour index of the primary is about $1^{m}\cdot18$ rather than the observed $1^{m}\cdot10$. A careful search has been made for the signature of the secondary in radial-velocity traces obtained near the nodes of the orbit, but no convincing evidence of it has been seen. Not only is the secondary much fainter than the primary, but its early type means that it cannot be expected to match the (Arcturus-based) mask in the *Coravel* at all well, so its dip would be very shallow in any case; it may also be rotating much more rapidly, smearing out an already weak dip beyond recognition.

HD 73512

P	=	128·25 ± 0·03 days	$(T)_0$	=	MJD 54785·87 ± 0·20
γ	=	+34·52 ± 0·05 km s^{-1}	$a_1 \sin i$	=	39·56 ± 0·15 Gm
K_1	=	23·27 ± 0·08 km s^{-1}	$a_2 \sin i$	=	44·89 ± 0·27 Gm
K_2	=	26·41 ± 0·16 km s^{-1}	$f(m_1)$	=	0·1504 ± 0·0017 $M_\odot$
q	=	1·135 ± 0·008 (= m_1/m_2)	$f(m_2)$	=	0·2197 ± 0·0040 $M_\odot$
e	=	0·2661 ± 0·0029	$m_1 \sin^3 i$	=	0·778 ± 0·011 $M_\odot$
ω	=	276·4 ± 0·7 degrees	$m_2 \sin^3 i$	=	0·685 ± 0·007 $M_\odot$

R.m.s. residual (unit weight) = 0·24 km s^{-1}

A characteristic radial-velocity trace of HD 73512 appears as Fig. 10, and the orbit is plotted in Fig. 11. In the solution of the orbit the velocities of the secondary component were weighted ¼. Table VII includes two isolated observations, made by Strassmeier *et al.*[3] and by Nidever *et al.*[46]. The Cambridge observations alone give the orbital period as 128·41 ± 0·25 days, and then the incorporation of the extra pair of velocities from Strassmeier and the primary-only one from Nidever, all made ten years previously (they have both been given a systematic adjustment of +0·8 km s^{-1} and a weighting of ¼ with respect to the Cambridge data), improves the period to the listed value while making negligible changes to the other elements.

The mean ratio of dip areas in the radial-velocity traces is 1 to 0·37, implying a difference of just over one magnitude between the components. The spectral

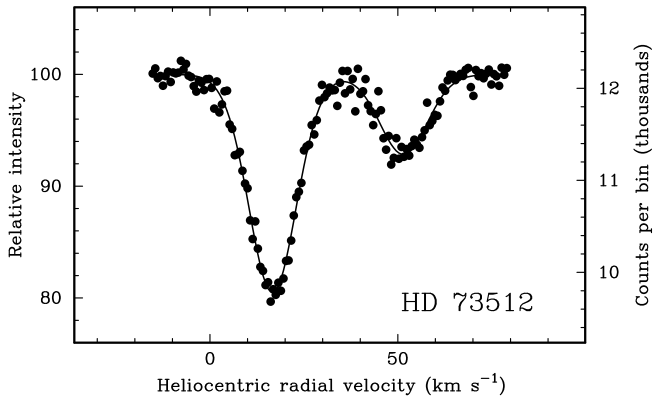

FIG. 10

Radial-velocity trace of HD 73512, obtained on 2009 February 11.

TABLE VII

Radial-velocity observations of HD 73512

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O−C)* Prim. km s^{-1}	*(O−C)* Sec. km s^{-1}
1998 Dec. 23·53*	51170·53	+11·5	—	$\overline{29}$·810	−0·5	—
1999 Feb. 14·28†	51223·28	56·2	+10·4	$\overline{28}$·222	+0·2	+0·3
2008 Dec. 27·19	54827·19	48·2	19·4	0·322	−0·1	+0·6
2009 Jan. 3·10	54834·10	43·6	24·5	0·376	0·0	+0·3
6·15	837·15	41·9	27·4	·400	+0·4	+0·8
14·09	845·09	35·9	32·8	·462	−0·1	−0·1
21·07	852·07	31·4	38·5	·516	+0·2	+0·2
24·12	855·12	28·5	39·2	·540	−0·6	−1·4
Feb. 4·08	866·08	21·8	48·9	·625	−0·3	+0·3
8·05	870·05	19·8	51·3	·656	+0·1	0·0
11·00	873·00	18·1	52·9	·679	+0·1	−0·3
13·99	875·99	16·4	55·0	·703	0·0	0·0
16·97	878·97	15·2	57·1	·726	+0·2	+0·5
Mar. 5·00	895·00	12·5	59·5	·851	−0·1	+0·1
6·92	896·92	13·5	59·4	·866	+0·1	+0·9
23·93	913·93	37·7	31·2	·999	+0·3	0·0
25·96	915·96	41·2	25·9	1·014	−0·3	−0·7
26·90	916·90	43·3	24·5	·022	0·0	0·0
27·90	917·90	45·2	22·9	·029	0·0	+0·4
29·89	919·89	48·4	18·7	·045	−0·1	+0·1
31·85	921·85	51·6	16·3	·060	+0·2	+0·9
Apr. 1·89	922·89	52·6	14·3	·068	−0·1	+0·4
5·89	926·89	56·4	10·0	·100	−0·1	+0·4
7·84	928·84	58·0	8·1	·115	+0·5	−0·3
19·84	940·84	56·4	8·4	·208	−0·4	−0·9
20·89	941·89	56·2	9·7	·217	−0·1	−0·1
29·88	950·88	51·3	15·5	·287	0·0	0·0
May 6·87	957·87	+46·6	+20·5	·341	−0·1	−0·2

*Observation by Nidever *et al.*[46].
†Observation by Strassmeier *et al.*[3].

type of the system as a whole is listed in the Catalogue as K0 V; the writer has not found an original source of that classification, but it accords well enough with the $(B-V)$ colour index of $0^{m}\cdot90$ and the absolute magnitude of $5^{m}\cdot9$ implied by the parallax. The magnitude difference, entirely supported by the mass ratio, suggests that the secondary star must be about K4 V. The minimum masses given by the orbit are about the masses that main-sequence stars of types K0 and K4 are expected to possess, so the orbital inclination must be high, but since $\sin i$ is so nearly unity for quite a range of i around 90° one cannot estimate an accurate value or suggest that eclipses are at all probable. Neither of the component stars is found from the *Coravel* traces to have a measurable rotational velocity; the pseudo-synchronous rotational period would be about 87 days, at which the equatorial velocity of rotation would be only about 0·5 km s^{-1} and consonant with observation, but the orbital period is not nearly short enough to lead to any expectation that the stars' rotations ought to be synchronized in that way.

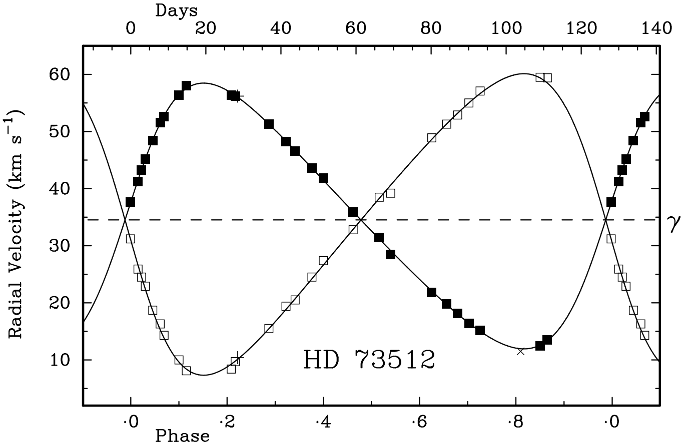

FIG. 11
Orbit of HD 73512. The cross plots an observation by Nidever *et al.*[46].

EQ Leonis

P	=	34·297 ± 0·004 days	$(T_0)_0$	=	MJD 54809·05 ± 0·06
γ	=	+12·77 ± 0·13 km s^{-1}	$a_1 \sin i$	=	6·61 ± 0·09 Gm
K	=	14·01 ± 0·19 km s^{-1}	$f(m)$	=	0·0098 ± 0·0004 $M_\odot$
e	≡	0			
ω	is undefined in a circular orbit		R.m.s. residual (wt. 1)	=	0·58 km s^{-1}

The orbital solution (Fig. 12) includes two measurements by Strassmeier *et al.*[3]. The Cambridge radial velocities, alone, give the period as 34·31 ± 0·06 days, just sufficiently accurate to define the cycle count back about ten years (100 periods) to the Strassmeier epoch. The usual statistical test shows that the

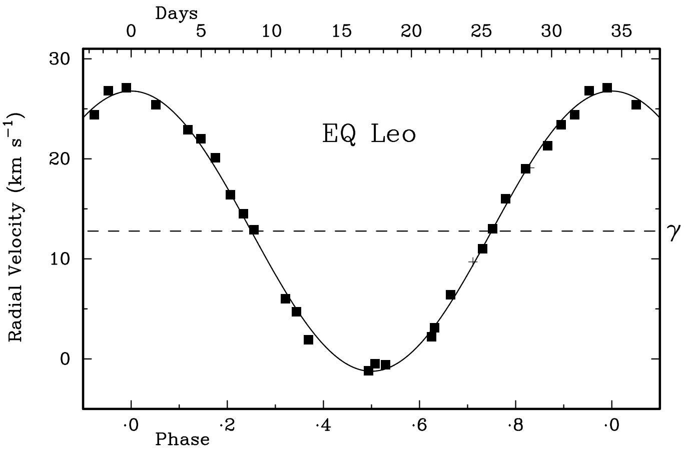

FIG. 12
Orbit of EQ Leo.

TABLE VIII

Radial-velocity observations of EQ Leo

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
1999 Feb. 23·28*	51232·28	+9·7	$\overline{1}$05·711	+0·3
27·34*	236·34	+19·1	·829	−0·4
2008 Dec. 27·20	54827·20	−0·6	0·529	+0·4
2009 Jan. 3·13	54834·13	+11·0	0·731	−0·1
6·19	837·19	+19·0	·820	+0·2
14·11	845·11	+25·4	1·051	−0·7
21·10	852·10	+12·9	·255	+0·6
24·14	855·14	+4·7	·344	−0·3
Feb. 4·14	866·14	+6·4	·665	+0·8
7·14	869·14	+13·0	·752	+0·1
8·08	870·08	+16·0	·779	+0·7
11·07	873·07	+21·3	·867	−0·8
12·04	874·04	+23·4	·895	−0·4
14·03	876·03	+26·8	·953	+0·6
Mar. 5·03	895·03	−0·5	2·507	+0·7
9·08	899·08	+2·2	·625	−0·7
25·98	915·98	+22·9	3·118	−0·2
26·93	916·93	+22·0	·145	+0·7
27·95	917·95	+20·1	·175	+1·0
29·02	919·02	+16·4	·206	−0·2
29·96	919·96	+14·5	·234	+0·3
Apr. 1·96	922·96	+6·0	·321	−0·7
7·87	928·87	−1·2	·494	0·0
24·91	945·91	+27·1	·990	+0·3
May 7·91	958·91	+1·9	4·369	−1·3
16·90	967·90	+3·1	·632	−0·2
26·89	977·89	+24·4	·923	−0·8

*Observation by Strassmeier *et al.*[3].

orbit is by no means distinguishable from a circle. The mass function demands a secondary of no more than 0·4 $M_\odot$ if the primary is deemed to have a mass of 2 $M_\odot$.

The mean $v \sin i$ value found from the *Coravel* traces is 19·2 ± 0·2 km s⁻¹. Although *Hipparcos* measured a parallax for EQ Leo, it is only just twice its own standard error, so the distance and luminosity are very uncertain, but it is clear that the star is a giant. The projected stellar radius, on the assumption (very likely to be correct) that the star rotates in the orbital period, is 13·0 $R_\odot$.

HD 93915

P	=	223·02 ± 0·05 days	$(T)_1$	=	MJD 54851·15 ± 0·20
γ	=	−16·26 ± 0·04 km s⁻¹	$a_1 \sin i$	=	63·53 ± 0·25 Gm
K_1	=	22·38 ± 0·08 km s⁻¹	$a_2 \sin i$	=	66·00 ± 0·24 Gm
K_2	=	23·25 ± 0·08 km s⁻¹	$f(m_1)$	=	0·2059 ± 0·0024 $M_\odot$
q	=	1·039 ± 0·005 (= m_1/m_2)	$f(m_2)$	=	0·2309 ± 0·0025 $M_\odot$
e	=	0·3788 ± 0·0024	$m_1 \sin^3 i$	=	0·889 ± 0·008 $M_\odot$
ω	=	37·7 ± 0·4 degrees	$m_2 \sin^3 i$	=	0·856 ± 0·008 $M_\odot$

R.m.s. residual (unit weight) = 0·26 km s⁻¹

HD 93915 is conspicuously double-lined, with slightly unequal components. A Cambridge radial-velocity trace of it appears as Fig. 13. The writer's own measurements result in an orbital period of 222·71 ± 0·33 days, plenty accurate enough to allow Strassmeier *et al.*'s velocities[3] to be included to refine the period. The star is the one of longest period among the 20 treated here. Indeed, there has barely been time since this project began to see it round a cycle, and the final observation lacks a measurement of the primary component because it was compromised by blending with the solar dip given by evening twilight as the star approached the limit of access in hour angle at nearly 7^h west. There have been only 16 cycles since the observations of Strassmeier *et al.* Those authors made two observations four days apart; the first was treated as single-lined, but two velocities were determined from the second one. The change in phase in four days is not great, but reference to the diagram of the orbit (Fig. 14) shows that the observations were made at a critical time when the components were indeed drawing apart, and the change in four days must have been just enough to make the difference between the spectrum appearing single or double. In the double-lined one, the components were assigned as primary and secondary in the way that would be expected on the basis of the Cambridge orbit. Their projected rotational velocities were listed[3] as 5·2 and 6·7 km s^{-1}, but they seem too small to be reliably determined from the Cambridge traces, probably no greater than 2 km s^{-1}.

The masses found from the orbit differ by 3·9 ± 0·5 per cent, an amount expected to correspond to slightly more than one spectral sub-type. The absolute magnitude of the system is accurately determined by the parallax and is very close to what could be expected for a pair of G5 main-sequence stars; the types might be taken as G5 V and G6 V. The areas of the dips in radial-velocity traces bear a mean ratio of 1 to 0·86 to one another; a direct conversion to Δm (actually to more like ΔB, since the *Coravel* operates in about the photometric B band) is $0^m \cdot 16$, but the better correlation of the cooler star's spectrum with the mask in the instrument would raise the true value to almost $0^m \cdot 2$. Since the ΔB between G5 and K0 is[27] $1^m \cdot 0$, it agrees excellently with the conclusion from the mass ratio that there is a difference of one sub-type between the components.

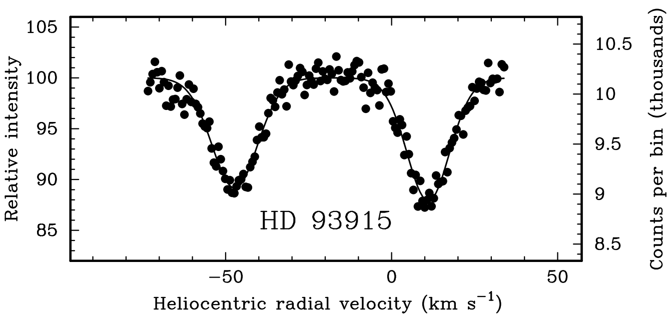

FIG. 13

Radial-velocity trace of HD 93915, obtained on 2009 January 6.

TABLE IX

Radial-velocity observations of HD 93915

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	Sec. km s^{-1}	*Phase*	*(O–C)* Prim. km s^{-1}	Sec. km s^{-1}
1999 Feb. 21·34*	51230·34	−18·2		$\overline{16}$·764	—	—
25·35*	234·35	−7·3	−26·4	·782	−0·4	−0·5
2008 Dec. 27·22	54827·22	+7·7	−41·1	0·893	+0·1	0·0
2009 Jan. 3·14	54834·14	+11·0	−45·1	0·924	−0·2	−0·3
6·21	837·21	+12·6	−45·7	·937	+0·3	+0·2
14·14	845·14	+12·1	−45·7	·973	0·0	0·0
21·13	852·13	+7·5	−40·4	1·004	+0·3	+0·2
24·15	855·15	+3·9	−37·3	·018	0·0	−0·1
Feb. 4·15	866·15	−9·7	−22·5	·067	+0·3	+0·2
12·05	874·05	−16·5		·103	—	—
16·95	878·95	−22·3	−10·7	·125	−0·2	−0·5
Mar. 5·05	895·05	−29·3	−2·5	·197	+0·1	+0·1
21·01	911·01	−31·7	−0·1	·268	0·0	+0·1
26·96	916·96	−32·0	−0·1	·295	−0·1	−0·1
Apr. 5·96	926·96	−31·8	−0·6	·340	−0·1	−0·4
19·89	940·89	−29·9	−1·5	·402	+0·6	−0·1
29·01	950·01	−29·5	−2·9	·443	−0·1	−0·3
May 6·94	957·94	−28·3	−3·8	·479	−0·2	+0·2
16·93	967·93	−25·8	−5·4	·524	+0·4	+0·5
24·90	975·90	−24·2	−7·7	·559	+0·3	0·0
June 1·91	983·91	−22·6	−9·9	·595	−0·1	−0·1
11·91	993·91	−19·7	−13·1	·640	−0·1	−0·3
July 1·91	55013·91	−12·0	−20·2	·730	+0·4	+0·1
12·90	024·90	−7·3	−25·5	·779	0·0	+0·1
20·89	032·89	−3·5	−30·5	·815	−0·5	−0·5
30·88	042·88	—	−36·6	·860	—	−0·2

*Observation by Strassmeier *et al.*[3].

HDE 237944

P	=	5·507669 ± 0·000015 days*	$(T)_{-16}$	=	MJD 54735·99 ± 0·15†
γ	=	−14·22 ± 0·10 km s^{-1}	$a_1 \sin i$	=	5·768 ± 0·014 Gm
K_1	=	76·16 ± 0·19 km s^{-1}	$a_2 \sin i$	=	5·903 ± 0·021 Gm
K_2	=	77·94 ± 0·28 km s^{-1}	$f(m_1)$	=	0·2526 ± 0·0019 $M_\odot$
q	=	1·023 ± 0·004 (= m_1/m_2)	$f(m_2)$	=	0·2708 ± 0·0029 $M_\odot$
e	≡	0·0143 ± 0·0016	$m_1 \sin^3 i$	=	1·058 ± 0·009 $M_\odot$
ω	=	103 ± 10 degrees	$m_2 \sin^3 i$	=	1·034 ± 0·007 $M_\odot$

R.m.s. residual (unit weight) = 0·44 km s^{-1}

*The true period, in the rest-frame of the system, is 5·507930 ± 0·000015 days. It differs from the observed period by 17 standard deviations.

†T is poorly determined owing to the smallness of the eccentricity. T_0 = MJD 54734·4195 ± 0·0016.

HDE 237944 is a visual double star (ADS[47] 7957) with a separation of about 1″·2. It was discovered by Aitken[48] in 1906, and has since advanced in position angle by about 1° per decade. Its parallax shows it to be about 100 pc away, so the projected linear separation of the components is something like 120 AU.

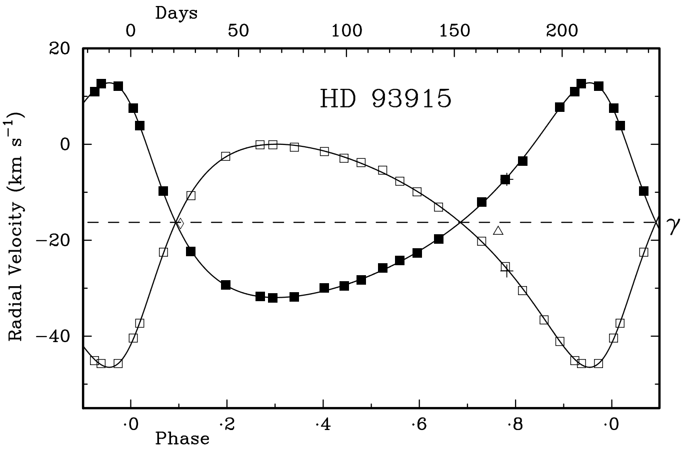

FIG. 14

Orbit of HD 93915. The 'missing' observation of the primary near phase 190 days is explained by its being compromised by blending with the 'daylight dip' as the effort was made to complete the otherwise rather uniform phase coverage with a final observation in the evening twilight.

A circular orbit with that radius ought to be accomplished in the order of 1000 years, whereas the rate of change of position angle suggests a much longer time: it may be that the true separation is substantially more than 120 AU, but the apparent motion over a short arc is greatly dependent on the eccentricity and inclination of the orbit and no definite statement is possible. The components' respective V magnitudes and $(B-V)$ colour indices have been found by Fabricius & Makarov[49] from a re-discussion of the *Tycho* photometry to be $9^{m}\cdot59$, $0^{m}\cdot82$ for the primary and $11^{m}\cdot48$, $1^{m}\cdot01$ for the secondary. Spectroscopic observations inevitably include light from both components; the brighter one is double-lined, so there are three spectra present, and the radial-velocity traces therefore usually exhibit three dips — a weak one that remains in a fixed position, flanked by a stronger pair, nearly equal, whose positions on either side of it change from

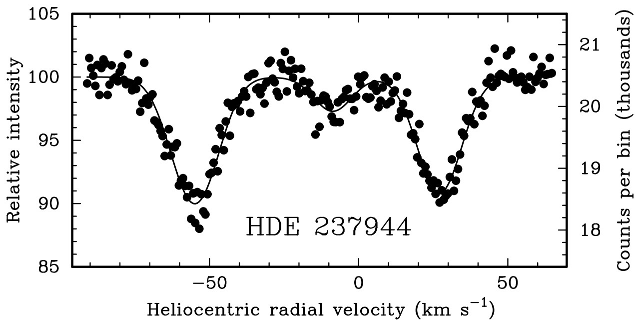

FIG. 15

Radial-velocity trace of HDE 237944, obtained on 2009 March 6. The small central dip arises from the visual secondary star.

TABLE X

Radial-velocity observations of HDE 237944

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* Aa	B	Ab	*Phase*	*(O–C)* Aa	Ab
		km s⁻¹	*km s⁻¹*	*km s⁻¹*		*km s⁻¹*	*km s⁻¹*
1999 Feb. 23·352*	51232·352	+31·9	−9·5	−63·3	$\overline{653}$·861	−0·9	−1·0
28·236*	237·236	+60·1	−9·6	−90·3	$\overline{652}$·748	−0·3	+0·3
2008 Dec. 27·244	54827·244	+31·1	−10·0	−60·4	0·568	+0·1	+0·1
2009 Jan. 6·227	54837·227	−51·9	−10·1	+23·4	2·380	−0·5	−0·5
14·171	845·171	+46·8	—	−76·6	3·823	+0·9	−0·9
21·150	852·150	−69·9	−13·3	+42·6	5·090	−0·5	+0·4
24·180	855·180	+52·8	−11·9	−83·1	·640	0·0	−0·3
Feb. 4·165	866·165	+51·7	—	−81·0	7·634	+0·1	+0·6
7·158	869·158	−88·8	−13·6	+62·9	8·178	+0·3	+0·5
7·877	869·877	−76·2	−13·1	+49·8	·308	+0·3	+0·3
11·036	873·036	+23·7	−10·0	−53·0	·882	−0·7	+0·7
12·067	874·067	−62·0	−13·4	+34·0	9·069	−0·1	−0·6
17·021	879·021	—	−13·4†	—	·969	—	—
Mar. 6·002	896·002	−54·8	−8·5	+27·2	13·052	+0·2	−0·3
21·050	911·050	+55·0	−13·2	−85·0	15·784	−0·3	+0·4
Apr. 21·013	942·013	−40·6	−12·0	+13·0	21·406	0·0	+0·2

*Observation by Strassmeier *et al.*[3].
†Single-lined blend of all components.

night to night. An example is shown in Fig. 15. Table X includes an extra column to show the radial velocities obtained from the weak central component.

The solution of the orbit benefits from two observations made by Strassmeier *et al.*[3] some ten years ago. (Those authors say that 'HD 237944A' is triple-lined, but that is not so — the visual A component is a simple double, and the 'triplicity' arises from the superposition of the spectrum of the visual companion.) By themselves, the Cambridge data yield a period of 5·5082 ± 0·0003 days — plenty accurate enough to provide an unambiguous count of the 600-odd cycles elapsed since the time of the earlier measurements, which were then able to contribute to the solution whose elements are given above and which is portrayed in Fig. 16. The eccentricity is about nine times its standard deviation and, though small, is therefore certainly significant. It is clear, too, what its significance actually *is*: it doubtless arises as a result, as explained by Mazeh & Shaham[50], of perturbations by the relatively remote third component of the system. Previous papers in this series that have documented analogous cases are nos. 110, 128, 160, and 176. For a system with such a small eccentricity, the uncertainty of T and ω is so large that it is useful to give in addition the quantity T_0, which is included in the footnotes to the informal table above.

HDE 237944 was quite recently recognized[51] as an eclipsing system, with a period of 5·50762 days — differing only in the fifth decimal from the orbital period found here — or possibly half that value. No doubt the possible ambiguity arose because, with very similar components in an almost-circular orbit, the primary and secondary eclipses are barely distinguishable, and there was slight doubt as to whether there was really a significant difference between alternate eclipses.

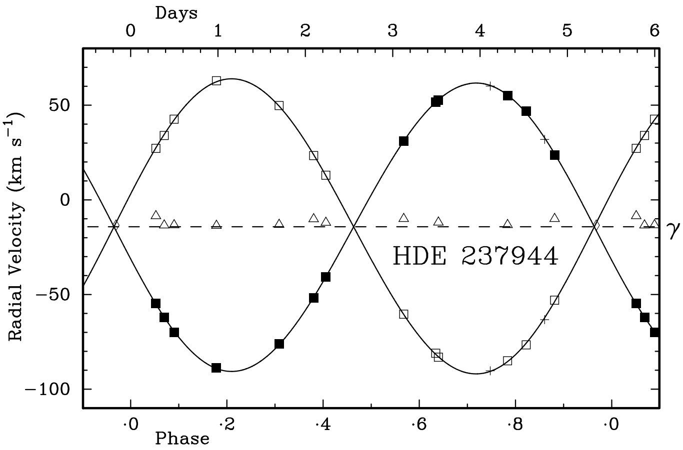

FIG. 16

Orbit of HDE 237944. The triangles refer to the (constant) velocity of the visual secondary.

The radial velocities of the 11½^{m} visual secondary, given in the column headed 'B' in Table X, have a mean of −11·7 ± 0·5 km s^{-1}, differing by 2·5 km s^{-1} from the γ-velocity of the double primary. That is about the relative velocity to be expected in a 2500-year circular orbit in a system having a total mass of about 3 $M_{\odot}$, and so is entirely acceptable for the HDE 237944 system.

The components of the visual primary give slightly unequal dips in the radial-velocity traces, the mean ratio of dip areas being 1 to 0·93. That implies a magnitude difference of just under a tenth of a magnitude, as indeed does the mass ratio, so the individual magnitudes must be very close to 10^{m}·3 and 10^{m}·4. The dip given by the visual secondary, similarly normalized, has a strength of 0·22 in the mean, indicating a magnitude difference of about 1^{m}·6. The *Coravel* operates in about the photometric *B* band, in which the faint component is likely to be — and *is* according to Fabricius & Makarov[49] — about 0^{m}·2 redder than the visual primary, so in terms of *V* magnitude the estimate from the radial-velocity traces is that it is 1^{m}·4 fainter than the brightest component, and therefore 11^{m}·7. The writer is not going to lose any sleep over the apparent discrepancy of 0^{m}·2 from Fabricius & Makarov's 11^{m}·5. If a reason for the discrepancy had to be identified, one that could well be adduced is that the guiding is almost unavoidably done on the photocentre of the visual binary, no doubt tending to bias the proportion of the light being transmitted by the entrance slit in favour of the primary.

It is a bit disturbing that the colour index of the system as a whole, as given in the Catalogue, is 0^{m}·71, about the colour of a G5 main-sequence star[27], whereas even the visual primary is put at 0^{m}·82, more like a G8 colour, by Fabricius & Makarov. The *Hipparcos* parallax corresponds to an absolute magnitude of 4^{m}·5, which would put the two bright components at about 5^{m}·3 and 5^{m}·4; but the parallax has a 1-σ uncertainty of about 20%, which translates to 0^{m}·4 in luminosity terms and would be consonant with spectral types from about G4 nearly to K0. It is of considerable interest that those two stars, though certainly later and possibly very considerably later in type than the Sun, nevertheless have masses that are incontrovertibly demonstrated by the orbital parameters to be appreciably in excess of 1 $M_{\odot}$.

Projected rotational velocities — which in view of the near-90° inclination demonstrated by the existence of eclipses must be practically the same as the true equatorial velocities — are 11·5 ± 0·6 and 10·0 ± 0·5 km s^{-1}. Probably most of the difference between them is to be ascribed to observational uncertainty, as is just about allowed by the formal standard errors. The velocities lead to radii of 1·25 and 1·09 $R_{\odot}$, which are larger than are to be expected for late-G stars; but since we are obliged to accept that the stars are more massive than the Sun it is only a small further step to admitting that they could be a little larger too, although we have to rely on their relative coolness to explain how, despite larger surfaces, they can be half a magnitude fainter in terms of V luminosity. The dips given by the faint visual secondary do not show significant broadening, but they are too weak for any reliable $v \sin i$ number to be assigned to them.

HD 112099

P	=	23·5035 ± 0·0018 days	$(T)_2$	=	MJD 54881·312 ± 0·025
γ	=	−15·81 ± 0·04 km s^{-1}	$a_1 \sin i$	=	5·217 ± 0·020 Gm
K	=	18·04 ± 0·06 km s^{-1}	$f(m)$	=	0·01026 ± 0·00012 $M_{\odot}$
e	=	0·4470 ± 0·0026			
ω	=	21·2 ± 0·5 degrees	R.m.s. residual (wt. 1)	=	0·19 km s^{-1}

An orbit from Cambridge observations alone has a period of 23·496 ± 0·008 days. There are two measurements made in 1999 by Strassmeier *et al.*[3], and when incorporated in the solution they lead to the orbit shown above and in Fig. 17. Nordström *et al.*[38] reported that there were 11 *Coravel* radial velocities of the star, giving a mean of −15·0 ± 4·2 km s^{-1} but seemingly no orbit; the measurements are not publicly available. Gray *et al.*[52] have given a classification of the star as K1 (implicitly K1 V) — much more consonant with the colour ($(B-V) = 0^{m}{\cdot}86$) and the luminosity ($M_V \sim 6^{m}{\cdot}1$) implied by the parallax than

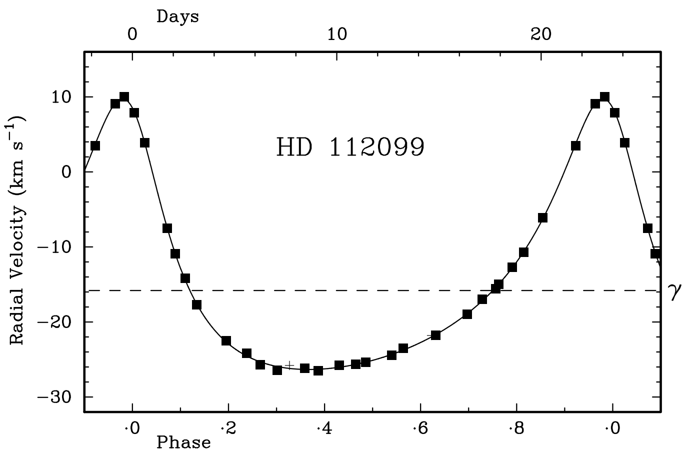

FIG. 17
Orbit of HD 112099.

TABLE XI

Radial-velocity observations of HD 112099

Except as noted, the observations were made at Cambridge

Date (UT)			MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
1999	Feb.	13·45*	51222·45	−25·8	$\overline{154}$·327	+0·4
		20·40*	229·40	−21·8	·623	+0·2
2009	Jan.	14·24	54845·24	−25·6	0·465	+0·1
		21·24	852·24	−15·0	·763	−0·1
	Feb.	4·24	866·24	−26·2	1·359	+0·1
		7·22	869·22	−25·4	·486	0·0
		12·19	874·19	−19·0	·697	−0·1
	Mar.	6·15	896·15	−21·8	2·631	−0·1
		9·10	899·10	−15·6	·757	−0·2
		21·08	911·08	−25·7	3·267	−0·4
		28·06	918·06	−23·5	·563	+0·3
	Apr.	9·03	930·03	−7·5	4·073	0·0
		20·00	941·00	−24·4	·540	−0·1
		29·00	950·00	+3·5	·922	−0·2
		29·98	950·98	+9·1	·964	0·0
	May	3·98	954·98	−17·7	5·134	−0·1
		10·95	961·95	−25·8	·431	+0·2
		17·93	968·93	−17·0	·728	+0·2
		19·96	970·96	−10·7	·814	0·0
		20·91	971·91	−6·1	·855	+0·1
		23·93	974·93	+10·0	·983	+0·3
		24·92	975·92	+3·9	6·025	−0·1
		26·91	977·91	−14·2	·110	+0·2
		28·91	979·91	−22·5	·195	+0·1
		29·92	980·92	−24·2	·238	+0·3
	June	11·90	993·90	−12·7	·790	+0·1
		16·92	998·92	+7·9	7·004	−0·1
		18·92	55000·92	−10·9	·089	−0·1
		23·92	005·92	−26·4	·302	−0·5
		25·92	007·92	−26·5	·387	−0·2

*Observation by Strassmeier *et al.*[3].

the Catalogue type of G5V. As far as the Cambridge traces are concerned, the rotational velocity is indeterminately small; $v \sin i$ values of 4·500 and 1·000 km s^{-1} are reported in the Catalogue.

HD 112859 (*BQ CVn*)

P	=	18·49857 ± 0·00024 days*	$(T_0)_0$	=	MJD 54830·645 ± 0·007
γ	=	+23·25 ± 0·08 km s^{-1}	$a_1 \sin i$	=	11·06 ± 0·03 Gm
K_1	=	43·47 ± 0·12 km s^{-1}	$a_2 \sin i$	=	13·31 ± 0·07 Gm
K_2	=	52·35 ± 0·29 km s^{-1}	$f(m_1)$	=	0·1578 ± 0·0013 $M_\odot$
q	=	1·204 ± 0·007 (= m_1/m_2)	$f(m_2)$	=	0·276 ± 0·005 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	0·923 ± 0·012 $M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	0·767 ± 0·006 $M_\odot$

R.m.s. residual (unit weight) = 0·40 km s^{-1}

*The true period, in the rest-frame of the system, is 18·49714 ± 0·00024 days. It differs from the observed period by 5·9 standard deviations.

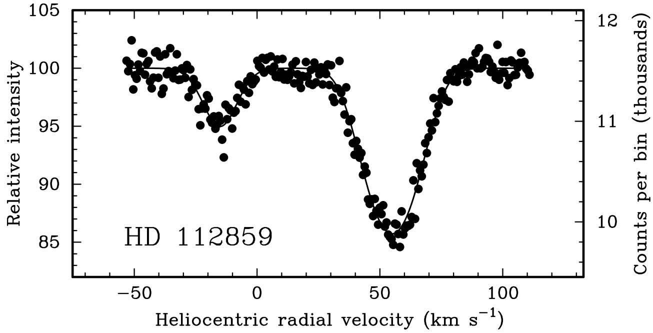

FIG. 18
Radial-velocity trace of HD 112859, obtained on 2009 May 6.

Radial-velocity traces of HD 112859, such as that reproduced in Fig. 18, exhibit two dips of very unequal sizes — not surprisingly, in view of its classification[17] as K0 III + late F. In the computation of the orbit, it has been found appropriate to attribute a weighting of ⅕ to the velocities of the secondary component. The literature[3] offers three velocity measurements, which date from 1999 and are useful for refining the period; in two cases velocities are given for both components, whereas the third is noted as a blend. An orbital solution that uses the Cambridge observations, only, has a period of $18{\cdot}4976 \pm 0{\cdot}0022$ days, easily accurate enough to phase the 1999 data in with the correct cycle count. When that is done, it is discovered that the supposedly blended observation is actually a measure of the primary alone, falling at a phase when the components are well separated, so it has been utilized accordingly; the final solution of the orbit is given above and shown in Fig. 19. The solution easily passes the usual statistical test for the non-significance of the eccentricity.

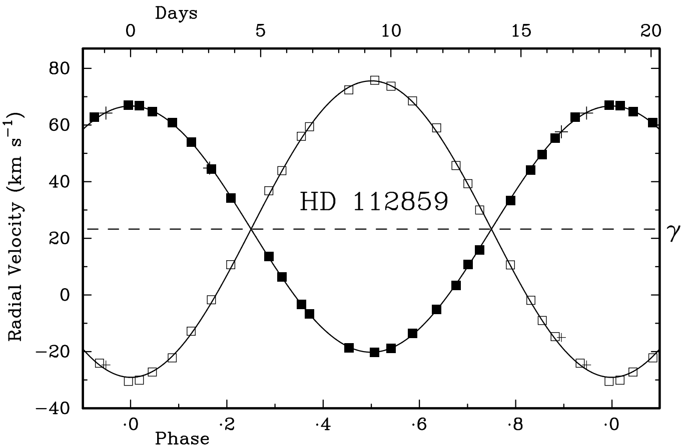

FIG. 19
Orbit of HD 112859.

TABLE XII

Radial-velocity observations of HD 112859

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O−C)* Prim. km s⁻¹	*(O−C)* Sec. km s⁻¹
1999 Feb. 12·49*	51221·49	+57·6	−15·0	$\overline{196}$·896	−0·1	+3·2
13·46*	222·46	+64·2	−24·7	·948	−0·2	+1·6
17·45*	226·45	+44·8	—	$\overline{195}$·164	−0·9	—
2009 Jan. 14·25	54845·25	+33·4	+10·6	0·790	−0·5	+0·2
21·24	852·24	+44·5	−1·7	1·167	−0·3	+1·0
Feb. 4·24	866·24	+62·7	—	·924	+0·8	—
7·23	869·23	+60·8	−22·2	2·086	+0·3	−0·5
12·20	874·20	−3·3	+56·0	·355	0·0	+0·8
Mar. 6·15	896·15	−18·9	+73·7	3·541	−0·1	−0·2
9·11	899·11	+10·8	+39·3	·701	+0·7	+0·2
27·15	917·15	+3·4	+45·7	4·676	−0·4	−0·9
28·06	918·06	+15·9	+30·0	·726	−0·7	−1·3
30·03	920·03	+44·1	−1·9	·832	−0·6	+0·6
Apr. 2·04	923·04	+67·0	−30·5	·995	+0·3	−1·4
9·01	930·01	−6·7	+59·4	5·372	+0·1	0·0
20·96	941·96	+66·9	−30·1	6·018	+0·4	−1·3
29·02	950·02	−18·6	+72·4	·453	−0·2	−1·0
30·02	951·02	−20·2	+75·8	·507	0·0	+0·3
May 6·96	957·96	+55·4	−14·7	·882	0·0	+0·8
19·97	970·97	−13·6	+68·5	7·586	+0·5	+0·3
20·90	971·90	−5·1	+59·0	·636	+0·2	+1·4
29·95	980·95	+53·9	−12·8	8·125	0·0	+0·9
June 1·94	983·94	+13·6	+36·8	·287	+0·3	+1·5
20·94	55002·94	+6·3	+43·9	9·314	+0·1	+0·2
30·96	012·96	+49·5	−9·0	·856	−0·5	0·0
July 20·92	032·92	—	−24·1	10·935	—	+0·6
22·95	034·95	+64·8	−27·2	11·044	−0·2	−0·1
25·97	037·97	+34·2	+10·7	·208	−0·5	+1·2

*Observation by Strassmeier *et al.*[3].

The system was found by *Hipparcos* to be a very unequal 'visual' double star, with an angular separation of about 0″·8 and a Δm of $3^{m}\cdot29 \pm 0^{m}\cdot11$ — certainly difficult to recognize visually through a telescope. That discovery seems not to have been followed up. It does not promise to impinge materially on the present discussion, however, since the companion contributes only about 5% of the total light of the system.

The *Hipparcos* parallax has a 1-σ uncertainty of 22%, which translates into a luminosity uncertainty of nearly half a magnitude around the central value of $M_V \sim +1^{m}\cdot7$. The luminosity itself is in any case not a fixed quantity — the *Hipparcos* 'epoch photometry', plotted for us in vol. **12**, p. C91, shows a range of fully $0^{m}\cdot2$. The mean dip areas in radial-velocity traces, expressed as equivalent widths in km s⁻¹ in a manner exactly analogous to those of absorption-line intensities in Å in spectra, are 4·16 and 0·78 km s⁻¹ for the primary and secondary, respectively — a ratio of 1 to 0·19. Because an F-type spectrum matches the (K2) mask in the *Coravel* so much worse than a K-type one, its intrinsic dip strength can be expected to be at most half that of the K giant, so its relative luminosity is probably at least double its relative dip strength in the radial-velocity traces, meaning that there is a Δm of (at most) about one magnitude in the *B* photometric band which is the region in which the *Coravel* operates. Even though the integrated luminosity is low for a system that includes a K giant, a late-F dwarf is not bright enough in comparison with that giant to produce radial-velocity traces like those observed. In view of the substantial disparity in masses, it seems unlikely that the dwarf is significantly evolved, so it is better to assume that it is not a late- but a mid-F star. A photometric model that satisfies the known constraints and reproduces the measured colour indices[53] appears here as Table XIII. The giant is listed in the Table with type K0 III–IV, so that the luminosity class agrees with the ascribed absolute magnitude. It is noted that Schild[53] attributed class III–IV to the system although he called it G8, understandably as a result of the admixture of an appreciable proportion of F spectrum with that of the K giant.

TABLE XIII

Photometric model (absolute magnitudes, colour indices) for HD 112859

	Star	M_V	$(B-V)$	$(U-B)$	M_B	M_U
		m	*m*	*m*	*m*	*m*
Model	K0 III–IV	2·0	1·07	0·90	3·07	3·97
	F5 V	3·4	0·42	0·03	3·82	3·85
	K0 III–IV+F5 V	1·74	0·90	0·52	2·64	3·16
HD 112859 (observed[53])			0·93	0·54		

Henry, Fekel & Hall[17], having been informed by a 'private communication', were aware as long ago as 1995 of the approximate orbital period and near-circularity of the orbit of HD 112859, and they discovered that there were considerable photometric variations with a similar period and with a complex wave-form of a very variable character and amplitude. Evidently there are major changes, on unusually short time-scales (weeks), in the starspots responsible for the variability. They are well shown in the *Hipparcos* plot cited above; the chaotic character of the variations must have defeated the mathematical tools brought to bear both by the *Hipparcos* authors and by Koen & Eyer[42], since neither of those syndicates recognized the underlying periodicity of the variation.

The mean $v \sin i$ value derived for the primary star from the *Coravel* traces is 18·1 ± 0·2 km s^{-1}; previously published values are 17[17] and 20·2[26]. On the basis that the secondary star is of type F5 V as proposed above, and that it accordingly[27] has a mass of 1·3 $M_{\odot}$, the $m_2 \sin^3 i$ value of 0·772 $M_{\odot}$ indicates that $\sin^3 i \sim 0·59$, $\sin i \sim 0·84$, and finally $i \sim 57°$. Freeing the $v \sin i$ value from $\sin i$, we obtain the equatorial velocity of the giant as 21·5 km s^{-1}, which with the 18·5-day rotation period yields a stellar radius of 7·9 $R_{\odot}$, which may be seen as being in reasonable accord with the under-luminosity already indicated for the giant star. If the correct mass has been attributed to the component that is here taken to be F5, then it follows from the mass ratio that the K star has a mass of 1·56 $M_{\odot}$.

Rotational velocities of <6 and 7·5 km s^{-1} have been published[17,26] for the secondary star. The mean of the values from the Cambridge traces is about 3 km s^{-1}, but that is certainly over-stated: half the values are zero. Since negative values are not permitted, dips that are, within the uncertainty caused by noise, the same width as the basic unbroadened profile inevitably give a positive mean $v \sin i$, since those in which the noise makes them look wider are attributed a positive $v \sin i$ whereas those that look narrower are called zero. It is fairest to say that no rotational broadening is certainly visible in the Cambridge traces.

CD Canum Venaticorum

P	=	$38{\cdot}7223 \pm 0{\cdot}0029$ days	$(T_0)_1$	=	MJD $54884{\cdot}26 \pm 0{\cdot}05$
γ	=	$-45{\cdot}23 \pm 0{\cdot}09$ km s^{-1}	$a_1 \sin i$	=	$8{\cdot}50 \pm 0{\cdot}07$ Gm
K	=	$15{\cdot}96 \pm 0{\cdot}13$ km s^{-1}	$f(m)$	=	$0{\cdot}0164 \pm 0{\cdot}0004\, M_\odot$
e	$\equiv$	0			
ω		is undefined in a circular orbit	R.m.s. residual (wt. 1)	=	0·44 km s^{-1}

There are two earlier radial-velocity measurements[3] which allow the period determined from the Cambridge observations alone (38·747 ± 0·032 days) to be refined. They date from about ten years (nearly 100 periods) ago; successive values of the cycle count therefore give periods differing by about P/100 or 0·4 days, more than 12 standard deviations, so the cycle count is secure. The eccentricity of the orbit is far from being significant.

TABLE XIV

Radial-velocity observations of CD CVn

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity km s^{-1}*	*Phase*	*(O–C) km s^{-1}*
1999 Feb. 23·43*	51232·43	−51·0	$\overline{94}$·692	−0·1
27·39*	236·39	−40·9	·794	0·0
2009 Feb. 7·24	54869·24	−57·0	0·612	+0·4
12·22	874·22	−46·8	·741	−0·6
Mar. 6·16	896·16	−51·8	1·307	−1·0
9·12	899·12	−57·2	·384	−0·1
21·10	911·10	−50·1	·693	+0·7
28·08	918·08	−34·5	·873	−0·4
30·04	920·04	−30·5	·924	+0·6
Apr. 1·08	922·08	−29·4	·977	0·0
2·06	923·06	−29·8	2·002	−0·5
9·02	930·02	−38·6	·182	0·0
20·01	941·01	−60·7	·465	+0·1
20·98	941·98	−61·0	·490	+0·2
May 3·99	954·99	−38·2	·826	−0·3
12·94	963·94	−30·5	3·058	−0·2
19·95	970·95	−43·6	·239	+0·5
20·94	971·94	−46·4	·264	+0·3
23·95	974·95	−53·6	·342	+0·3
24·91	975·91	−56·7	·367	−0·8
26·93	977·93	−59·5	·419	−0·3
30·99	981·99	−60·9	·524	+0·1
31·94	982·94	−60·1	·548	+0·4
June 1·98	983·98	−59·3	·575	+0·1
23·99	55005·99	−34·3	4·144	+1·0
July 3·94	015·94	−58·2	·400	0·0
12·95	024·95	−56·4	·633	−0·5
Aug. 7·87	050·87	−50·5	5·303	−0·1
22·86	065·86	−51·4	·690	−0·3
23·86	066·86	−48·2	·716	+0·5

*Observation by Strassmeier *et al.*[3].

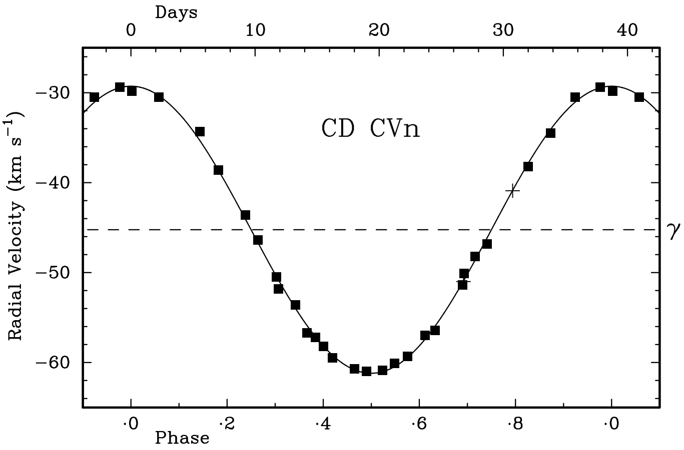

FIG. 20
Orbit of CD CVn.

The mass function is small and, if the primary star is assumed to be a giant with a mass of 2 $M_\odot$, it does not demand for the secondary a mass of more than about 0·45 $M_\odot$, corresponding to about M0 for a star on the main sequence. The mean of the Cambridge measures of the projected rotational velocity is 18·4 ± 0·2 km s^{-1}; on the usual, and in cases such as this doubtless correct, assumptions that the rotation is synchronized and that the axial and orbital rotational vectors are aligned, the projected radius, $R \sin i$, is 14·1 $R_\odot$. One cannot help noticing that that is nearly twice the actual (not projected) radius deduced for the supposedly analogous K giant in HD 112859, the system treated in the section immediately preceding this one. CD CVn is quite red for a star of its reported type of K0 III, which was derived from a low-dispersion objective-prism spectrogram[54]; it is probably appreciably cooler than the giant in HD 112859, so its surface brightness would be lower and a larger surface area would be needed to give the same luminosity. The luminosity itself is quite uncertain, since the *Hipparcos* parallax has a 1-σ uncertainty of 37%; if the true value is even one sigma down on the central value, the absolute magnitude is altered from +2^{m}·0 to +1^{m}·0 — a change which, in conjunction with the suggested lower surface brightness, would accommodate the inequality between the radius of CD CVn and that of the giant component in HD 112859.

CD CVn is one of the many variables that were discovered serendipitously by *Hipparcos*, but no period was established. Strassmeier *et al.*[3], on the basis of 63 measurements obtained with an automated photometric telescope, found a period of 39 days, but noted that it could be 38 to 42 days; the amplitude was as much as 0^{m}·125. The Catalogue now lists the period (surely with exaggerated precision) as 38·000000000 days. In any case it is evident that the variability closely matches the orbital period and is therefore almost certainly a manifestation of starspots.

HD 127068 (*HK Boo*)

P	=	$15{\cdot}1500 \pm 0{\cdot}0004$ days*	$(T_0)_2$	=	MJD $54889{\cdot}206 \pm 0{\cdot}011$
γ	=	$-49{\cdot}36 \pm 0{\cdot}07$ km s^{-1}	$a_1 \sin i$	=	$4{\cdot}610 \pm 0{\cdot}021$ Gm
K_1	=	$22{\cdot}12 \pm 0{\cdot}10$ km s^{-1}	$a_2 \sin i$	=	$6{\cdot}57 \pm 0{\cdot}07$ Gm
K_2	=	$31{\cdot}55 \pm 0{\cdot}34$ km s^{-1}	$f(m_1)$	=	$0{\cdot}01704 \pm 0{\cdot}00024\ M_\odot$
q	=	$1{\cdot}426 \pm 0{\cdot}017$ ($= m_1/m_2$)	$f(m_2)$	=	$0{\cdot}0494 \pm 0{\cdot}0016\ M_\odot$
e	$\equiv$	0	$m_1 \sin^3 i$	=	$0{\cdot}1430 \pm 0{\cdot}0037\ M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	$0{\cdot}1003 \pm 0{\cdot}0016\ M_\odot$

R.m.s. residual (unit weight) = $0{\cdot}35$ km s^{-1}

*The true period, in the rest-frame of the system, is $15{\cdot}1525 \pm 0{\cdot}0004$ days. It differs from the observed period by 6·6 standard deviations.

The spectrum of this object exhibits very unequal double lines, illustrated here in Fig. 21 which reproduces a radial-velocity trace obtained quite near a node of the orbit. Once again, to add to the Cambridge measurements there are two radial-velocity measurements made about ten years ago by Strassmeier *et al.*[3]. By themselves, the Cambridge measures yield a period of $15{\cdot}1495 \pm 0{\cdot}0039$ days. Measurements of the secondary have been globally weighted 0·1 in comparison with the primary. About 250 orbital cycles have elapsed since the early measures, so the eigenvalues of the period at which those measures would fit the orbit are about 15/250, or 0·06, days apart. Since they fall not far from the correct phase there is no doubt as to which cycle is the correct one. The second observation of the secondary in the listing by Strassmeier *et al.*[3] is given an internally estimated standard error of 9·4 km s^{-1}, implying that it is almost indeterminate, so it was zero-weighted — and in truth it does give a far larger residual than any other measurement, although not nearly as large as 9·4 km s^{-1}. The final orbital elements are listed above, and the orbit is shown in Fig. 22. As in so many other cases, it is statistically indistinguishable from a circle. Nordström *et al.*[38] reported that they had two radial velocities, but did not publish them.

Photometric variability was discovered by *Hipparcos*, which shows a plot of them (**12**, p. A312) in which their timings have been folded on a period of 6·953 days (approximated in the Catalogue as 6·950000000 days). It is not a

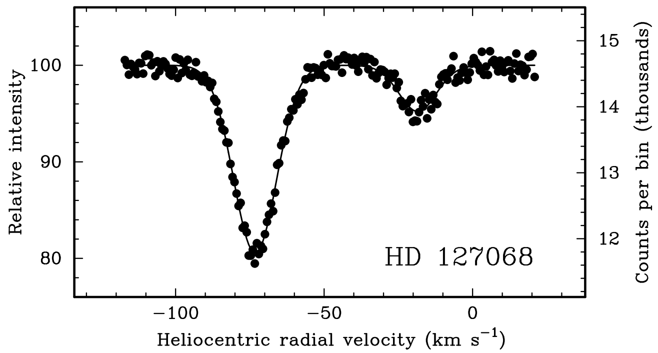

FIG. 21

Radial-velocity trace of HD 127068, obtained on 2009 July 5.

TABLE XV

Radial-velocity observations of HD 127068

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* Prim. km s^{-1}	*Velocity* Sec. km s^{-1}	*Phase*	*(O−C)* Prim. km s^{-1}	*(O−C)* Sec. km s^{-1}
1999 Feb. 12·53*	51221·53	−31·4	−74·5	$\overline{241}$·909	−0·6	+1·4
15·52*	224·52	−32·7	−70·3†	$\overline{240}$·106	−0·7	+3·9
2009 Feb. 12·23	54874·23	−27·3	−79·6	1·011	0·0	+1·2
Mar. 6·20	896·20	−70·7	−18·9	2·462	+0·1	−0·2
9·14	899·14	−61·0	−32·6	·656	+0·7	−0·9
21·14	911·14	−70·5	−18·6	3·448	−0·2	+0·9
27·16	917·16	−37·3	−66·2	·845	−0·4	+0·9
28·09	918·09	−31·2	−75·8	·907	−0·3	−0·2
30·13	920·13	−28·3	−79·9	4·041	−0·3	0·0
Apr. 2·09	923·09	−46·7	−51·8	·237	+0·8	+0·2
22·07	943·07	−70·3	−19·6	5·555	−0·1	+0·1
30·06	951·06	−30·1	−77·9	6·083	+0·1	−1·2
May 4·05	955·05	−61·9	−30·2	·346	0·0	+1·2
20·02	971·02	−67·8	−23·2	7·400	−0·5	+0·6
23·04	974·04	−67·4	−24·3	·600	−0·1	−0·5
27·00	978·00	−34·7	−71·5	·861	+0·5	−1·9
31·02	982·02	−33·8	−72·6	8·126	0·0	−1·1
31·98	982·98	−40·9	−60·9	·190	+0·3	+0·1
June 18·00	55000·00	−58·3	−39·3	9·313	−0·4	−2·1
23·97	005·97	−54·8	−41·5	·707	+0·4	−0·5
24·97	006·97	−46·6	−51·5	·773	−0·5	+2·4
July 3·93	015·93	−64·0	−27·1	10·365	−0·1	+1·5
5·94	017·94	−71·7	−16·9	·497	−0·2	+0·9
12·94	024·94	−27·9	−77·9	·959	+0·1	+2·0
15·93	027·93	−37·2	−65·7	11·157	−0·1	+1·1
24·91	036·91	−49·4		·749	—	—
25·91	037·91	−40·6	−61·5	·815	−0·1	+0·5

*Observation by Strassmeier *et al.*[3].
†Rejected observation.

very convincing plot; but the *Hipparcos* measurements (which were of course a massive bonus and not the principal objective of the satellite) were made in batches lasting the order of a day, and their distribution in time was by no means ideal for the determination of periodicities that were other than strict. The Catalogue lists a period of 17·62 days, which is almost equally incommensurable with the orbital period.

The mean dip areas are 3·56 and 0·71 km s^{-1}, giving a ratio of just 5 to 1 or in magnitude terms $1^{m}\cdot 75$. We have no indication as to any difference in spectral types between the components, such as could make the brightness ratio differ substantially from the observed ratio of areas. The spectral type of G5IV, taken together with the Δm that represents the dip ratio, as well as with the absolute magnitude of $3^{m}\cdot 4 \pm 0^{m}\cdot 3$ that corresponds to the parallax, does suggest a G-type subgiant plus a main-sequence star of similar colour. Absolute magnitudes of about $3^{m}\cdot 6$ and $5^{m}\cdot 4$ would add up to the proper total. That would make the secondary about a G8V star. The colour index of the system seems to be a bit on the red side for a normal G5IV (μ Her, the obvious analogue among the bright stars, has $(B-V) = 0^{m}\cdot 75$, whereas HD 127068 is $0^{m}\cdot 89$), so we

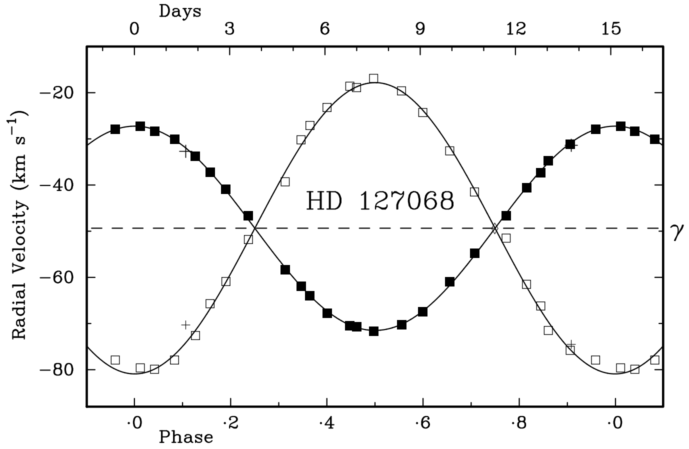

FIG. 22
Orbit of HD 127068.

might be more comfortable with a type of G8 IV for the primary — but when discussing photometry we are in a weak position to argue about spectra with spectroscopists!

If we allow, as a working hypothesis, that the secondary star is of type G8 V, it should have a mass near 0·9 $M_{\odot}$, about nine times the minimum demanded by the orbit. Thus $\sin^3 i \sim 1/9$, $\sin i \sim 0{\cdot}48$, and i is about 28 or 29 degrees. From the mass ratio, the primary would have a mass of about 1·3 $M_{\odot}$. The mean $v \sin i$ found for that star is 6·8 ± 0·3 km s^{-1}; in the light of $\sin i$, the equatorial velocity is about 14 km s^{-1}, and if the star rotates in the orbital period it would need to have a radius of 4·2 $R_{\odot}$. That would make it some three magnitudes brighter than its companion, instead of the $1^{\mathrm{m}}{\cdot}75$ suggested by the radial-velocity traces on the basis that the colours of the two components are similar. It is regretted, therefore, that the model is not self-consistent, but without further information it is scarcely possible to determine where it goes astray. The weakest point is probably the assumption of synchronous rotation: a main-sequence star in a 15-day orbit would not usually be expected to rotate synchronously, and maybe the subgiant is not doing so. For equal surface brightnesses the primary should have (from the dip ratio of 5 to 1) a radius only $\sqrt{5}$ times larger than the secondary, or about 2 $R_{\odot}$, and it could well be rotating at about twice the synchronous rate — though *why* would be still more difficult to answer!

HD 142680 (*V383 Ser*)

P	=	24·5345 ± 0·0006 days*	$(T)_7$	=	MJD 54708·895 ± 0·022
γ	=	−82·93 ± 0·05 km s^{-1}	$a_1 \sin i$	=	11·900 ± 0·030 Gm
K	=	37·14 ± 0·09 km s^{-1}	$f(m)$	=	0·1117 ± 0·0008 $M_{\odot}$
e	=	0·3139 ± 0·0021			
ω	=	324·7 ± 0·4 degrees	R.m.s. residual (wt. 1)	=	0·30 km s^{-1}

*The true period, in the rest-frame of the system, is 24·5413 ± 0·0006 days.
It differs from the observed period by 11 standard deviations.

TABLE XVI

Radial-velocity observations of HD 142680

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
1999 Feb. 23·52*	51232·52	−82·6	$\overline{135}$·307	−0·4
Mar. 4·39*	241·39	−110·8	·668	−0·2
4·39*†	241·39	−52·4	·668	—
2008 Mar. 31·16	54556·16	−105·7	0·775	+0·4
Apr. 24·10	580·10	−108·3	1·750	−0·2
May 3·10	589·10	−44·9	2·117	−0·3
19·04	605·04	−106·9	·767	−0·1
June 26·00	643·00	−83·3	4·314	0·0
July 1·00	648·00	−105·1	·518	−0·1
4·04	651·04	−110·4	·642	−0·1
8·98	655·98	−95·4	·843	+0·5
12·99	659·99	−41·3	5·007	+0·1
20·95	667·95	−85·3	·331	+0·5
21·91	668·91	−90·7	·370	+0·3
23·93	670·93	−99·5	·453	+0·3
24·91	671·91	−102·9	·493	+0·3
28·91	675·91	−110·2	·656	+0·3
30·89	677·89	−109·1	·736	−0·2
Aug. 2·90	680·90	−92·3	·859	0·0
10·88	688·88	−59·2	6·184	0·0
12·89	690·89	−75·6	·266	−0·1
22·84	700·84	−110·7	·672	−0·2
25·86	703·86	−104·1	·795	−0·2
30·84	708·84	−43·3	·998	+0·4
Sept. 13·86	722·86	−108·6	7·569	−0·6
19·81	728·81	−101·5	·812	+0·1
25·81	734·81	−36·3	8·056	+0·1
26·79	735·79	−40·6	·096	0·0
27·80	736·80	−48·8	·137	0·0
Oct. 2·78	741·78	−87·0	·340	+0·1
8·76	747·76	−108·5	·584	+0·2
11·76	750·76	−110·3	·706	−0·2
16·76	755·76	−77·3	·910	−0·7
17·76	756·76	−61·2	·951	−0·6
22·74	761·74	−52·4	9·154	+0·1
27·74	766·74	−89·2	·358	+0·2
2009 July 12·96	55024·96	−85·5	19·882	+0·4
20·95	032·95	−64·4	20·208	−0·1
22·92	034·92	−79·2	·288	0·0
25·97	037·97	−96·7	·413	−0·8
Aug. 7·88	050·88	−65·3	·939	+0·2
18·85	061·85	−92·8	21·386	+0·1

*Observation by Strassmeier *et al.*[3].
†Ditto, of secondary component.

The orbit, plotted in Fig. 23, benefits from more data than most in this paper, because the star was placed on the programme in the spring of 2008 — earlier than the others. The Cambridge observations alone define the period as 24·5334 ± 0·0012 days; the addition of two Strassmeier *et al.*[3] measures from 1999 refine the solution to the one given above. The high γ-velocity, and the fact that the star has a velocity below –100 km s⁻¹ for about 40% of the time,

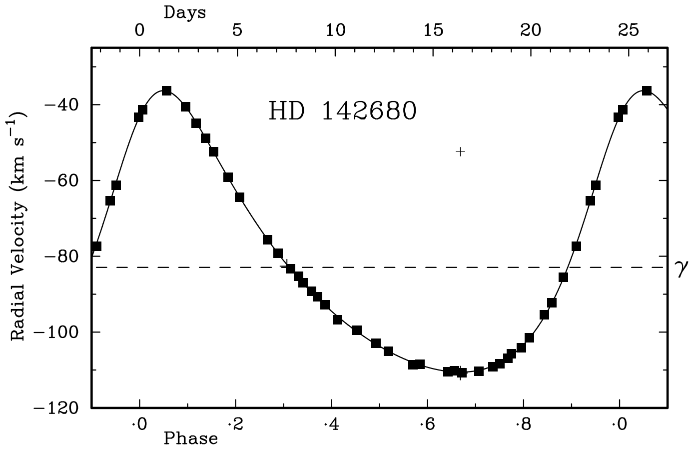

FIG. 23

Orbit of HD 142680. The isolated plus symbol is a possible measurement[3] of the secondary star.

is noteworthy. The Catalogue describes HD 142680 as a "newly discovered SB" and gives a reference at that point to Strassmeier *et al.*[3]. They probably *did* discover its binary nature, but their published paper is rather confusing on that issue. It has lists of single- and double-lined binaries that were found in their observing programme; in the case of the single-lined ones it lists separately those that were being claimed as new discoveries, but for the double-lined ones it simply says that *so-many* of them were new detections, but it does not identify which stars they were. We notice that among the double-lined stars there is ζ Cyg, for which a single-lined orbit was given[55] more than 100 papers back in the present series and whose secondary is known[56] to be a white dwarf, which one would suppose to be beyond detection by ordinary spectroscopy in the optical region. HD 142680 features in the list of newly discovered single-lined binaries. The published text of the paper refers to it in these terms: "It has a double-peaked cross-correlation function but the spectrum shows no clear evidence of the secondary lines. We list it as a SB1 but it could be an unresolved SB2 system. If so, the second peak in the red spectrum gives $v_r = -52{\cdot}5 \pm 3{\cdot}5$ km s^{-1}". (The table of results that is not in the printed paper gives $-53{\cdot}2 \pm 6{\cdot}2$ km s^{-1}.) As far as the Cambridge observations are concerned, no secondary is detectable, but the mass function suggests that it might well be. The primary is an early-K dwarf which could be expected to have a mass near 0·8 $M_\odot$, and the mass function of 0·11 $M_\odot$ demands a secondary of not less than 0·6 $M_\odot$, corresponding to about K7V, not much more than two magnitudes fainter in *V*.

Strassmeier *et al.*[3] reported a photometric period of 33·52 days, and were evidently inclined to attribute it to eclipses because there is a note in their computer-accessible Table 3 saying "P=67d eclipsing?". Of course they were unaware at that time of the 24½-day period, which does not seem to be comprehensibly related to the photometric one at all; the pseudo-synchronous period[10] would be about 15·1 days. The rotational velocity is too small to be determinable from the radial-velocity traces, so no clue concerning the rotation period is available from that source.

HD 145230 (PX Ser)

P	=	12·2961 ± 0·0004 days	$(T_0)_{-3}$	=	MJD 54904·836 ± 0·008
γ	=	−4·25 ± 0·14 km s^{-1}	$a_1 \sin i$	=	8·64 ± 0·04 Gm
K_1	=	51·09 ± 0·21 km s^{-1}	$a_2 \sin i$	=	12·43 ± 0·13 Gm
K_2	=	73·5 ± 0·8 km s^{-1}	$f(m_1)$	=	0·1703 ± 0·0021 $M_\odot$
q	=	1·439 ± 0·017 (= m_1/m_2)	$f(m_2)$	=	0·508 ± 0·016 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	1·46 ± 0·04 $M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	1·013 ± 0·015 $M_\odot$
			R.m.s. residual (unit weight)	=	0·66 km s^{-1}

This a fairly faint star, double-lined but with a weak secondary dip that was not recognized in the Cambridge traces, one of which is reproduced in Fig. 24, until some way into the observing campaign. The Cambridge measurements, by themselves, produce an orbit with a period of 12·295 ± 0·004 days; it is refined by the two 1999 observations[3] (the later of which is double-lined) to the value seen in the table above. The orbit is circular, its eccentricity being far short of statistical significance; it is plotted in Fig. 25.

Strassmeier *et al.*[3] found the star to show photometric variability with an amplitude of 0$^{\rm m}$·13 and a period of 12·32 days; the similarity to the orbital period seems to be a clear demonstration that the photometric period represents the period of axial rotation and that the origin of the variations lies in starspots or at least in azimuthal inequalities. Curiously, HD 145230 does not feature in those authors' list of double-lined binaries (or single-lined ones, for that matter), but it does appear in a list (their Table 3) of 'new Doppler-imaging candidates' in which it is listed as being of type K2 IV, SB2, and having a $v \sin i$ of 19 km s^{-1}. The mean projected rotational velocity of the primary star is given by the Cambridge traces as 22·4 ± 0·4 km s^{-1}. The subgiant luminosity class is in accord with the *Hipparcos* parallax which, though having a 27% uncertainty that translates to a luminosity uncertainty of about 0$^{\rm m}$·6, indicates an M_V of about +3$^{\rm m}$·1. The Catalogue lists the type as 'K2 V + K5?', which is in clear conflict with the parallax and also with the high rotational velocity. That velocity, coupled with the rotational period equal to the orbital period, implies a projected stellar radius of nearly 5½ $R_\odot$ — so much larger, even if $\sin i \sim 1$, than a main-sequence K2 star as to require HD 145230 to be about four magnitudes above the main sequence, *viz.*, at an M_V of about +2$^{\rm m}$·4, which is seen to be just about consonant with the

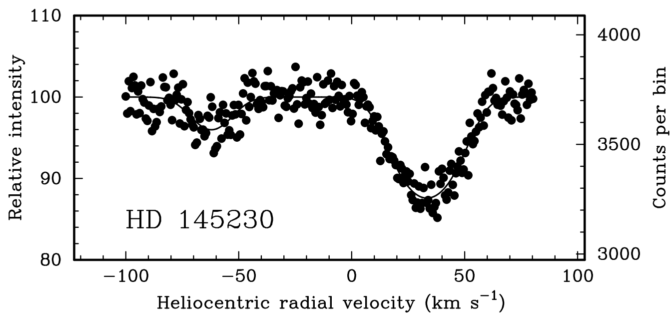

FIG. 24

Radial-velocity trace of HD 145230, obtained on 2009 May 29.

TABLE XVII

Radial-velocity observations of HD 145230

Except as noted, the observations were made at Cambridge

Date (UT)			*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	*(O – C)* Sec. km s⁻¹
1999	Mar.	3·50*	51240·50	+44·8	—	$\overline{302}$·992	−2·0	—
		4·43*	241·43	+42·2	−69·9	$\overline{301}$·068	−0·1	+1·3
2009	Apr.	29·10	54950·10	−26·1	—	0·681	−0·4	—
		30·08	951·08	−0·9	—	·761	−0·1	—
	May	4·09	955·09	+38·5	—	1·087	−0·9	—
		7·05	958·05	−28·7	—	·328	−0·5	—
		20·06	971·06	−42·6	—	2·386	+0·1	—
		23·08	974·08	−40·4	—	·631	−1·5	—
		24·05	975·05	−16·8	—	·710	+0·1	—
		27·03	978·03	+44·4	−76·1	·953	−0·2	−1·5
		29·02	980·02	+34·4	−61·0	3·114	+0·2	−1·4
		30·02	981·02	+13·7	−30·9	·196	+0·9	−2·1
		31·05	982·05	−15·1	+8·2	·280	−1·4	−1·1
	June	1·02	983·02	−36·7	+43·1	·358	−0·3	+1·0
		2·02	984·02	−51·1	+63·6	·440	+0·6	−0·5
		11·96	993·96	−3·5		4·248	—	—
		17·00	999·00	−31·4	+34·9	·658	+0·8	−1·0
		20·98	55002·98	+47·5	−76·2	·982	+1·0	+1·1
		24·01	006·01	+1·8	−12·9	5·228	−0·9	+1·4
	July	1·01	013·01	+10·7	−21·7	·797	−0·1	+4·2
		1·97	013·97	+31·6	−54·1	·876	−0·4	+2·3
		3·98	015·98	+45·9	−74·3	6·039	+0·6	+1·3
		9·96	021·96	−53·8	+70·3	·525	+0·9	+1·9
		15·95	027·95	+47·2	−76·5	7·012	+0·5	+1·1
		22·92	034·92	−48·6	+62·8	·579	+0·5	+2·5

*Observation by Strassmeier *et al.*[3].

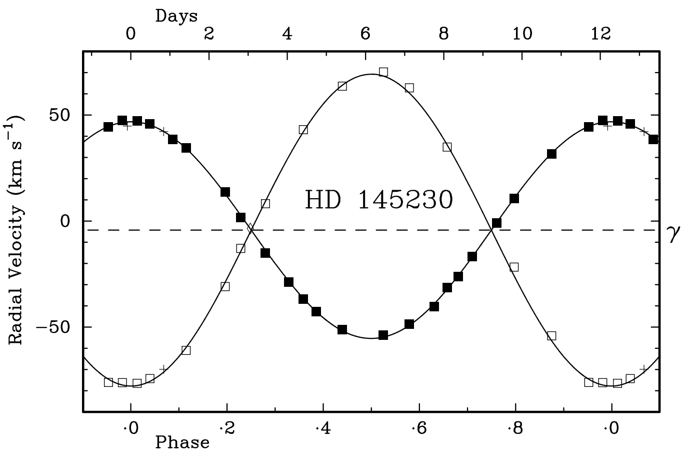

FIG. 25
Orbit of HD 145230.

parallax. Since the primary is evidently an evolved star, we cannot use either the mass ratio or the ratio of dip areas on radial-velocity traces to make any reliable deductions about the natures of the components. On the supposition that the secondary star is not another evolved object, however, we might suggest that its mass, shown by the orbit to be just over 1 $M_{\odot}$ as a minimum, is by no means consonant with the K5 type suggested (albeit with a question mark) in the Catalogue but would demand that it should be, at the latest, little later than solar type. The S/N achievable with the Cambridge traces is not sufficient for a good determination of the projected rotational velocity of the secondary. The sum of the projected stellar radii being some 6 $R_{\odot}$ or 4·2 Gm, and the sum of the projected orbital radii 21 Gm, there would have to be eclipses if $\tan i > 21/4{\cdot}2$, *i.e.*, $i \gtrsim 79°$. No evidence has been seen in the *Hipparcos* 'epoch photometry' or other photometry of such eclipses; there is[3] a variation with the same period as the orbit, but it is not of an eclipse nature.

HD 150202 (GI Dra)

P	=	68·476 ± 0·004 days	$(T_0)_3$	=	MJD 54914·952 ± 0·027
γ	=	−6·33 ± 0·04 km s^{-1}	$a_1 \sin i$	=	21·67 ± 0·06 Gm
K	=	23·01 ± 0·06 km s^{-1}	$f(m)$	=	0·0866 ± 0·0007 $M_{\odot}$
e	≡	0			
ω	is undefined in a circular orbit		R.m.s. residual (wt. 1)	=	0·23 km s^{-1}

HD 150202 presents another (and more acute) case of the problem seen with HD 16884, inasmuch as the early Strassmeier *et al.*[3] observations require a change of period greater than the Cambridge measurements are at all willing to accommodate. The 34 new velocities, by themselves, yield a circular orbit with a period of 68·552 ± 0·019 days. The usual Bassett[10] statistical test, outlined in the section on HD 16884 above, actually indicates that the eccentricity is just significant at the 5% level — it yields $F_{2,28} \sim 3{\cdot}51$, the 5% level being 3·34. The eccentricity, when permitted as a free parameter, is only 0·0043 ± 0·0018, with $\omega = 94° \pm 26°$, and even when zero eccentricity is forced upon the solution the r.m.s. residual of the 34 points is only 0·17 km s^{-1} — agreeably small in comparison with the residuals of all the other stars treated in this paper — so it seems quite likely that the orbit is really circular and that the formally somewhat significant eccentricity is due merely to the observations not being of statistically uniform quality and the residuals not forming a normal distribution.

Be that as it may, the two early measures, obtained about 55 cycles previously, fall about 4 days 'late' according to the Cambridge orbit, and when included in the solution they demand a reduction of the period to 68·476 days — a change of no less than four standard deviations. The gradient of velocity change between the two old observations does not encourage the idea of any variation of the γ-velocity, so the writer elects to accept the revised period, together with the implication that, for whatever reason (starspots are the obvious one, especially since the star shows RS CVn-type variability), the formal standard error of the Cambridge-only period is misleadingly small. The r.m.s. residual in the joint solution is increased to 0·23 km s^{-1}, which is still quite agreeable, and the solution (Fig. 26) does not *look* bad; the eccentricity is still just significant at 5%, with the actual value much as before, but that is hardly surprising since the vast majority of the data are the same. The circular solution is the one that is adopted here.

TABLE XVIII

Radial-velocity observations of HD 150202

Except as noted, the observations were made at Cambridge

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
1999 Feb. 13·53*	51222·53	+13·1	$\overline{51}$·077	−0·9
20·46*	229·46	+4·4	·179	+0·7
2008 Nov. 7·78	54777·78	+16·7	0·997	0·0
22·74	792·74	−1·7	1·215	−0·4
25·72	795·72	−7·9	·259	−0·3
Dec. 3·74	803·74	−22·9	·376	−0·2
6·72	806·72	−26·5	·419	0·0
7·74	807·74	−27·8	·434	−0·4
9·73	809·73	−29·0	·463	−0·3
11·71	811·71	−29·2	·492	+0·1
2009 Mar. 30·17	54920·17	+14·2	3·076	+0·1
May 4·10	955·10	−25·5	·586	+0·5
7·09	958·09	−22·0	·630	+0·1
20·07	971·07	+3·2	·820	−0·2
23·08	974·08	+8·6	·863	−0·1
24·05	975·05	+10·4	·878	+0·2
27·06	978·06	+13·9	·922	0·0
29·05	980·05	+15·7	·951	+0·1
June 11·97	993·97	+7·1	4·154	+0·4
15·02	997·02	+1·3	·198	+0·3
18·06	55000·06	−5·3	·243	0·0
24·04	006·04	−17·4	·330	0·0
26·01	008·01	−20·7	·359	+0·2
July 6·95	018·95	−29·1	·519	+0·1
9·98	021·98	−27·5	·563	+0·1
12·96	024·96	−24·3	·607	+0·1
15·96	027·96	−19·7	·650	+0·1
19·99	031·99	−12·1	·709	+0·1
20·96	032·96	−10·2	·723	0·0
22·96	034·96	−5·7	·753	+0·3
24·05	036·05	−3·9	·768	−0·2
25·05	037·05	−1·9	·783	−0·3
25·92	037·92	0·0	·796	−0·2
Aug. 12·00	055·00	+15·8	5·045	0·0
15·92	058·92	+12·1	·102	0·0
17·85	060·85	+9·4	·131	0·0

*Observation by Strassmeier *et al.*[3].

It was *Hipparcos* that discovered the photometric variability of HD 150202 and arranged for the star to receive a variable-star designation. The range is quite small, about 0^{m}·05; *Hipparcos* (vol. **12**, p. A356) shows it folded on a period of 76·70 days, which we have to admit is of the same order as the (then-unknown, obviously) orbital period, but does not appear very convincing, and the Catalogue leaves a blank in the relevant place rather than reporting that period. Both the colour and the trigonometrically derived distance modulus are in agreement with the classification of the star as K0 III. If the star is assumed to have a mass of 2 $M_{\odot}$, then the mass function demands a secondary mass of at least 0·9 $M_{\odot}$, corresponding to that of a late-G main-sequence star.

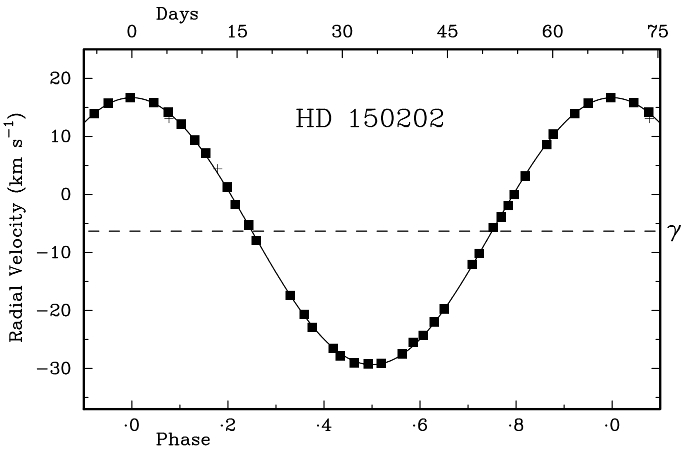

FIG. 26
Orbit of HD 150202.

Radial-velocity traces give evidence of modest rotational broadening, of $v \sin i = 4{\cdot}8 \pm 0{\cdot}2$ km s^{-1}. If the star's axial rotation is synchronized to the orbit, as seems likely, the rotation yields a value for $R_\star \sin i$ of 6·5 $R_\odot$. Since the star can be expected to be a good deal larger than that minimum radius, its companion can be expected to be substantially above the minimum mass needed to satisfy the mass function; it has, however, not been detected in radial-velocity traces.

2E 1848·1 +3305

P	=	1·10354 ± 0·00016 days	$(T_0)_{43}$	=	MJD 55040·6870 ± 0·0032
γ	=	−11·83 ± 0·32 km s^{-1}	$a_1 \sin i$	=	0·417 ± 0·009 Gm
K_1	=	27·5 ± 0·6 km s^{-1}	$a_2 \sin i$	=	0·414 ± 0·012 Gm
K_2	=	27·3 ± 0·8 km s^{-1}	$f(m_1)$	=	0·00238 ± 0·00015 $M_\odot$
q	=	0·99 ± 0·04 (= m_1/m_2)	$f(m_2)$	=	0·00232 ± 0·00021 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	0·0094 ± 0·0007 $M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	0·0094 ± 0·0005 $M_\odot$

R.m.s. residual = 2·5 km s^{-1}

This object came to attention as an X-ray source observed in the *Einstein* Galactic-plane survey described by Hertz & Grindlay[57]. The same authors[58] subsequently made optical observations of the relevant fields, obtaining low-dispersion spectra of candidate identifications with the 60-inch *Tillinghast* reflector at the Whipple Observatory. The brightest and most likely identification, among three objects observed within the X-ray error circle, was what they characterized as a G star. At much the same time, Takalo & Nousek[59] obtained spectra of some of the X-ray objects with an échelle/CCD spectrograph[60] giving a resolving power of about 12000 on the 62-inch reflector at the Black Moshannon Observatory that the University of Pennsylvania operated at that time. For the object of present interest, they proposed a spectral type of K0 III–IV, and in eight observations spread over a total interval of some two months (if a misprint is admitted in one of the dates) they found it always to show Hα in strong emission. They measured their spectra for radial velocities, which they said had an 'error' of ±5 km s^{-1}; they gave an orbital period of 2·3 days and a mathematical expression for the orbital velocity curve, which though defined as a sine wave appears in their Fig. 4 to have a major discontinuity in slope at zero phase. The projected rotational velocity was put at 24 km s^{-1}, and on the basis of that value and the period — and presumably an estimate, based on the spectral type, of the absolute value of the stellar radius — they proposed a value of 70° for the orbital and axial inclination. As a close-binary system the object is in good company, being only about 12′ due south of the famous variable star and contact binary β Lyrae.

The only other information that we might have about the object and that is relevant to the present paper are the V and $(B-V)$ magnitudes, for which *Vizier* gives values derived from *Tycho* photometry as 10^{m}·72 and 1^{m}·114, respectively; but from the V_T and B_T given by *Tycho* 2 and transformed according to the recipe given in the *Introduction* to the *Hipparcos* catalogue, equations 1·3·20, the writer obtains V = 10^{m}·70, $(B-V)$ = 0^{m}·75 — a serious discrepancy in the colour index, although both values are based on the same observations. Mr. R. Pickard has kindly undertaken photometry of the star, and has provided a preliminary value of 0^{m}·67 ± 0^{m}·04 for the $(B-V)$ colour index. That is clearly *not* compatible with the spectral classification of K0 III–IV proposed by Takalo & Nousek[59], but there is no means of resolving the conflict at the time of writing.

The star is very faint for observation with the 36-inch telescope and *Coravel* radial-velocity spectrometer, and on that account no effort was made to observe it until work was well advanced upon the objects that were more readily measured. When the 2E object was eventually observed, the difficulty over its faintness, already exacerbated by significant dark count from the (un-cooled) *Coravel* photomultiplier on the warm summer nights that then prevailed, was further increased by the nature of the cross-correlation dip. The dip was found to be split into two shallow components; they were never more than just separated owing to the modest velocity amplitudes of the components. A trace obtained near a node is seen in Fig. 27. Although very noisy, such a trace does serve to give radial velocities of a sort. A period a little over 10 days fitted the initial observations, but then a suspicion arose that it was an alias, representing the difference in frequency from 1 day^{-1}, and indeed the true period of about 1·1 days was promptly confirmed by an observation made at a substantial hour

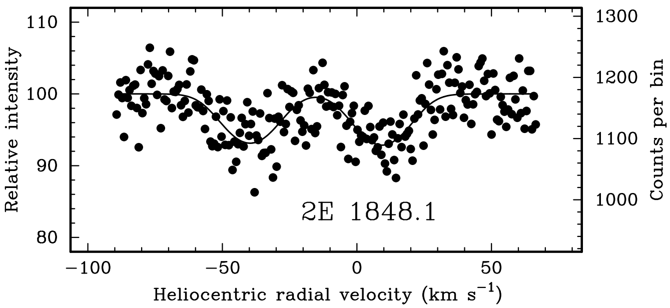

FIG. 27

Radial-velocity trace of 2E 1848·1 +3305, obtained on 2009 July 10.

TABLE XIX

Cambridge radial-velocity observations of 2E 1848·1 +3305

Date (UT)		*MJD*	*Velocity*		*Phase*	*(O – C)*	
			Prim. *km s*$^{-1}$	*Sec.* *km s*$^{-1}$		*Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$
2009 June	12·05	54994·05	−15·7	−5·7	0·739	−1·9	+4·2
	24·06	55006·06	−28·3	+6·9	11·622	+3·3	−0·9
	26·05	008·05	−36·0	+11·8	13·425	+0·3	−0·7
July	3·05	015·05	−15·7		19·768	—	—
	4·03	016·03	−28·4	+4·7	20·656	−1·3	+1·4
	5·01	017·01	−42·6	+15·0	21·544	−4·4	+0·6
	6·00	018·00	−36·0	+13·3	22·442	+1·5	−0·3
	7·01	019·01	−28·9	+3·2	23·357	0·0	−1·9
	10·02	022·02	+10·8	−38·3	26·084	−1·1	−2·9
	13·00	025·00	−5·3	−15·8	28·785	+0·6	+1·9
	15·99	027·99	−39·9	+16·7	31·494	−0·6	+1·3
	20·03	032·03	+0·6	−24·4	35·155	−3·0	+2·7
	20·98	032·98	+16·9	−43·4	36·016	+1·4	−4·4
	22·05	034·05	+18·0	−40·8	·986	+2·5	−1·8
	23·01	035·01	+4·9	−29·7	37·856	−0·2	−1·1
	24·04	036·04	−2·0	−20·1	38·789	+3·2	−1·7
	25·01	037·01	−23·2	−3·2	39·668	+2·2	−4·8
	26·00	038·00	−36·1	+14·0	40·565	+0·9	+0·8
	27·99	039·99	−31·4	+6·5	42·368	−1·0	−0·1
	28·08	040·08	−40·1	+14·6	·450	−2·1	+0·5
	30·05	042·05	−12·6		44·231	—	—
	30·92	042·92	+13·1	−38·2	45·027	−2·2	+0·5
Aug.	7·92	050·92	−16·7	−3·3	52·276	−0·5	+4·2
	11·93	054·93	+7·0	−32·9	55·907	−4·1	+1·6
	15·02	058·02	−11·6	−9·2	58·709	+7·3	−4·4
	15·97	058·97	−37·1	+12·4	59·571	−0·5	−0·4
	17·94	060·94	−29·9	+2·0	61·354	−1·4	−2·7
	18·05	061·05	−38·5	+13·3	·449	−0·6	−0·7
	18·91	061·91	−11·7		62·231	—	—
	19·89	062·89	+12·5	−35·3	63·119	+4·2	−3·5
	20·88	063·88	+14·1	−39·8	64·015	−1·4	−0·8
	20·97	063·97	+9·4	−32·0	·102	−0·8	+1·7
	21·89	064·89	+10·7	−33·0	·929	−2·3	+3·5
	21·99	064·99	+18·3	−37·3	65·022	+2·9	+1·5
	22·92	065·92	+4·1	−25·7	·867	−2·5	+4·5
	23·90	066·90	−12·2		66·751	—	—
	24·89	067·89	−30·5	+9·6	67·651	−2·6	+5·5
	28·04	071·04	−38·7	+16·1	70·504	+0·6	+0·7

angle; previously, the faintness of the system had encouraged the observer to keep near the meridian. Once observations of the object had begun and its interest had become apparent, it was observed quite assiduously in an effort to offset the poor quality of the radial velocities. The data are set out in Table XIX and lead to the elements given at the head of this section; they are plotted in Fig. 28.

The two dips have areas and widths that are exactly equal as nearly as can be determined, and the masses of the stars, too, are seen from the orbital elements to be indistinguishable from one another; the designation of the primary can only be arbitrary. The characteristic value for the projected rotational velocities may be taken as 15 km s^{-1}; a mathematical mean would not be an improvement, partly owing to differences in the qualities of the individual traces, and also because, away from the nodes of the orbit, the dips can be slightly widened by

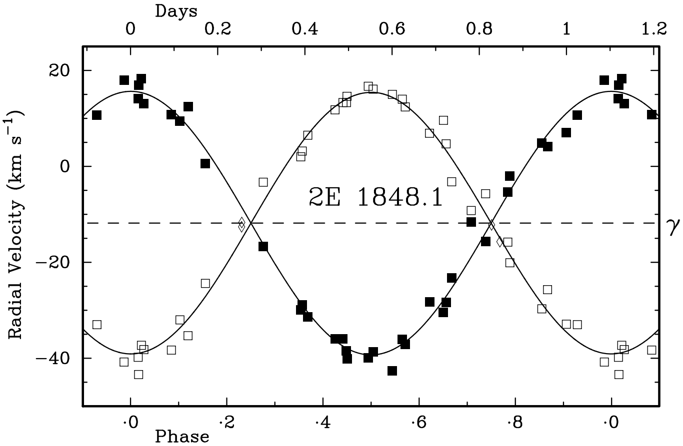

FIG. 28
Orbit of 2E 1848·1 +3305.

actual change of the radial velocities in this very-short-period system during the necessarily considerable integration time.

It is quite difficult to believe that the writer's observations described here can refer to the same object that was observed at a good site and with a considerably larger (if somewhat crude*) telescope, with integration times of 4800 seconds, by Takalo & Nousek[59]. It has been mentioned above that those authors gave a spectral type that is incompatible with the colour index. Also, they did not discover the double lines, although the sum of the amplitudes is 55 km s^{-1}, which is about $c/5500$ and therefore ought easily to have been resolved by their $R = 12000$ spectra. Moreover, since the two dips — and according to hypothesis, therefore, the two spectra — are indistinguishable from one another, their centroid is stationary, and it follows that observations that are reduced as single-lined should show no significant changes in velocity at all. The spectral region around the Hα emission line is shown by Takalo & Nousek in a montage of seven of their spectra; there are substantial differences among them in the wavelength of the line but no obvious variations in line-width, so the indications are that the system that they observed is single-lined — there is no evidence of any spectrum of the second star, either in emission or in absorption. The spectra in the montage are not identified by date, and even if their order is random it is difficult to relate the distribution of radial velocities that the reader may think he can estimate from the plots with the distribution of the tabulated numerical results. The Takalo & Nousek radial velocities, when folded on the period that is asserted here, bear no apparent relationship to it. If it comes to that, they have a surprisingly poor relationship to the curve plotted by those authors themselves, in view of the fact that the curve represents the best fit that they could obtain for any choice of period. The Cambridge observations pretty certainly refer to the correct star, not only according to its position in the sky and a check of the

*The 62-inch reflector of the Black Moshannon Observatory is described in ref. 61; it had a metal mirror of indifferent optical quality, and the observatory was abandoned when it broke.

field from a finding chart downloaded from *Vizier*, but also because a random star would be most unlikely to prove to be a double-lined binary of very short period — just the sort of object that could well be expected to be an X-ray source.

If it were not for HD 31738, treated above, the 2E system would have the shortest period ever found by the present writer. Its components must be dwarf stars to have such a short period, and the colour index then suggests that they must be of solar type. The fact that $v \sin i$ is much less than K for both stars shows that the system is well detached. If the stars are of 1 $M_\odot$, then their mass functions would demonstrate that $\sin^3 i \sim 0{\cdot}0094$, $\sin i \sim 0{\cdot}22$, $i \sim 13°$. But with that value of $\sin i$, the observed $v \sin i$ values would represent actual rotational velocities of nearly 70 km s^{-1}, requiring the stellar radii to be about 1·5 $R_\odot$. Adjudication is not possible without additional observational input.

HD 191179

P	=	10·79775 ± 0·00009 days*	$(T_0)_0$	=	MJD 54771·723 ± 0·006
γ	=	−19·92 ± 0·17 km s^{-1}	$a_1 \sin i$	=	9·21 ± 0·04 Gm
K_1	=	62·01 ± 0·24 km s^{-1}	$a_2 \sin i$	=	9·64 ± 0·17 Gm
K_2	=	64·9 ± 1·2 km s^{-1}	$f(m_1)$	=	0·267 ± 0·003 $M_\odot$
q	=	1·047 ± 0·019 (= m_1/m_2)	$f(m_2)$	=	0·307 ± 0·016 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	1·17 ± 0·05 $M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	1·120 ± 0·023 $M_\odot$

R.m.s. residual (unit weight) = 0·77 km s^{-1}

*The true period, in the rest-frame of the system, is 10·79847 ± 0·00009 days. It differs from the observed period by 8·4 standard deviations.

This is a troublesome star to observe, as it is double-lined, with one of the dips being very wide and notably shallow (Fig. 29); in the solution of the orbit it has been necessary to give the corresponding component a global weighting of only 0·05. The star was first observed by the writer 43 years ago, in the course of measurements[62] in the +15° Selected Areas[63] (it is in Area 88) in the very first season that cross-correlation was being used[64] as a means of determining radial velocities (or for *any* astronomical purpose, for that matter). Not surprisingly, only the dip stemming from the star that we shall here

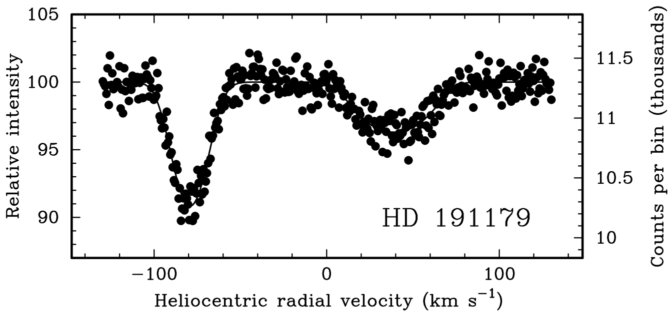

FIG. 29

Radial-velocity trace of HD 191179, obtained on 2009 July 2.

TABLE XX

Radial-velocity observations of HD 191179

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

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Phase*	*(O–C)* Prim. km s⁻¹	*(O–C)* Sec. km s⁻¹
1966 Sept. 6·92*	39374·92	+35·4	—	$\overline{1426}$·073	−0·2	—
1969 Sept. 22·86*	40486·86	+37·6	—	$\overline{1323}$·052	−1·2	—
1992 June 8·48†	48781·48	−72·2	−18·6	$\overline{555}$·232	−59·1	+8·5
1993 Sept. 25·83‡	49255·83	+13·2	−62·0	$\overline{511}$·163	+0·8	−8·3
1994 Sept. 5·00‡	49600·00	+40·4	—	$\overline{479}$·037	0·0	—
2008 Nov. 7·83	54777·83	−77·4	+38·5	0·566	−0·7	−1·0
11·86	781·86	+35·3	—	·939	−2·3	—
14·75	784·75	−2·3	−31·8	1·206	+0·9	+5·7
16·73	786·73	−67·8	+30·2	·390	−0·1	+0·2
18·77	788·77	−74·6	+27·8	·579	−0·1	−9·4
22·75	792·75	+38·6	−84·8	·947	−0·1	−3·5
Dec. 6·76	806·76	−16·4	−21·3	3·245	+1·5	+0·7
7·76	807·76	−52·5	+16·1	·337	−0·2	+2·1
9·76	809·76	−81·9	+44·6	·523	−0·6	+0·3
11·75	811·75	−35·1	−10·2	·707	+1·4	−7·6
2009 May 31·09	54982·09	−81·5	+42·8	19·482	+0·1	−1·8
June 2·09	984·09	−50·3	+10·0	·668	+0·3	−2·2
17·08	999·08	+39·2	−82·3	21·056	+0·9	−1·5
18·07	55000·07	+18·3	−58·5	·148	+1·0	+0·3
26·08	008·08	+27·0	−76·6	·889	−0·7	−6·8
July 2·06	014·06	−78·1	+40·2	22·443	−0·1	−0·7
4·09	016·09	−62·3	+22·1	·631	−0·3	−2·0
6·07	018·07	+4·9	−48·2	·815	+0·3	−2·6
10·11	022·11	+3·7	−44·0	23·189	+0·4	+0·3
20·08	032·08	+26·1	−67·8	24·112	−1·2	+1·6
22·12	034·12	−39·6	+5·1	·301	−0·1	+4·5
28·04	040·04	+17·0	−60·8	·849	+0·7	−3·0
Aug. 18·07	061·07	−1·5	−43·1	26·797	+0·4	−4·3

*Observed with original spectrometer, weight 0·05.
†Observation by Osten & Saar[66], weight 0.
‡Observation by Cutispoto *et al.*[67], weight 0·1.

designate the primary — the deeper and less-wide one — was recognized, and even *that* was distinctly noted on the observing records as being a "*very* feeble dip". Another measurement with the same instrument three years later gave a similar velocity — which in a way was unfortunate, because the two velocities just happened to made at almost identical phases near a node, and were nearly 60 km s⁻¹ away from the actual γ-velocity, thus innocently misleading Eggen when he utilized the mean velocity as if it were the γ-velocity in investigations[65] of stellar kinematics. The two measurements are attributed a weighting of 0·1 in the solution of the orbit, which is illustrated in Fig. 30.

The journal of observations in Table XX includes not only the above-mentioned early pair and the recent Cambridge measures, but others from two different sources. There is one (with velocities attributed to both components)

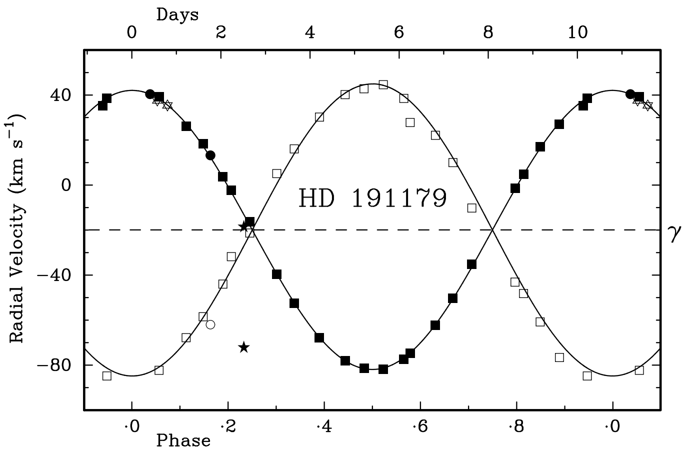

FIG. 30

Orbit of HD 191179. The two open stars represent measurements made by the writer with the original radial-velocity spectrometer in the 1960s; two measurements of the primary and one of the secondary by Cutispoto *et al.*[67] are shown as circles. The two filled stars plot observations reported by Osten & Saar[66]; they could not be utilized in the orbit.

by Osten & Saar[66]. They must be mistaken, because one of the velocities is practically the γ-velocity of the system, which would be understandable enough because it falls at a phase where the object would appear single-lined, whereas the other is far away and cannot correspond even approximately to either component; they form an impossible combination whatever the phase, and consequently cannot be included in the solution. Then there are three velocities in a table described by Cutispoto *et al.*[67]. Two are assigned to HD 191179a, and the other, made at a time identical to one of them and evidently measured from the same spectrum, to HD 191179b. It is not immediately obvious which star is which, and the measurements did not seem consonant with the Cambridge orbit whichever assignment was adopted. The correct assignments, agreeable to the orbit, were established with the kind assistance of Drs. Cutispoto and Pastori. A further complication is that although the *dates* of those observations are known, the *times* are not. The star would have been on the meridian at ESO, where the observations were made, at about 0^h UT at the relevant time of year, and, on the assumption that they were made somewhere near the meridian, *that* is the time attributed to them here. The uncertainty as to their timing has led to their being given low weight (0·05) in the solution of the orbit; they nevertheless offer some reassurance by certifying the cycle count back to the writer's own observations made in the 1960s. (That count is in fact secure even without them: the orbital period derived from the recent Cambridge measurements alone is 10·7970 ± 0·0006 days, and over the ~1400 cycles back to the 1960s the phasing uncertainty has still not grown to as much as one day, or a tenth of the period.) There are in addition, reported by Duflot *et al.*[68], five single-value radial velocities obtained by the French objective-prism method in 1959–71 but said to have been made to complement *Hipparcos*; they range from −57 to −4 km s^{-1} with no discernible relationship to orbital phase, and are not included in the present discussion.

HD 191179 is not an *Hipparcos* star, so we have not got the advantage of knowing the absolute magnitude to assist in the assignment of spectral types. Osten & Saar[66], who had a spectrum (illustrated in their paper) obtained at the *McMath* solar tower at Kitt Peak, with very high resolution but not very good *S*/*N*, gave the types as K0 IV and G2 V, and the projected rotational velocities as 38 and 15 km s^{-1}, respectively. Cutispoto *et al.*[67], from a photometric decomposition of the colours, suggested K0 IV and G2 IV, with M_Vs of $+2^{m}\cdot61$ and $+3^{m}\cdot52$.

The exceptional difficulties presented by the character of the spectrum of HD 191179 have impelled the writer not to take formal mean values for the equivalent widths of the dips seen in the radial-velocity traces or of the rotational velocities derived from them. The quality of the traces is far from uniform, and it has seemed best to take a somewhat subjective approach, assessing best values by paying most attention to the best traces while nevertheless considering the whole *ensemble*. The values thus adopted are a ratio of 1 to 0·85 ± 0·05 for the equivalent widths, and rotational velocities of 14 ± 1 and 33 ± 2 km s^{-1} for the primary and secondary, respectively. The uncertainties given are, like the quantities themselves, estimates, and are intended to indicate limits that might correspond to confidence of the order of 1–1½ σ. Hesitating to take an equally cavalier approach to the radial velocities themselves, however, the author has attributed the same weight to all (of the same component) in the solution of the orbit; he may not have obtained quite the best elements that suitable weighting of the data might be capable of giving, but at least he cannot be held guilty of any fudge!

Late-type stars do not rotate at the rates observed for the components of HD 191179 without good reason: the rotations must surely be synchronized to the orbit. In that case, the rotational velocities, in conjunction with the 10·8-day period, show that the projected radii ($R_\star \sin i$) of the stars are about 3 and 7 $R_\odot$ respectively. It must be supposed that the larger radius must belong to the cooler star, otherwise there would be a very large disparity in their luminosities in the violet, where the *Coravel* operates.

The masses of the components are almost equal; both of them must be evolving, since no late-type main-sequence star has a radius anywhere near as large as even the smaller of the stars that constitute HD 191179. The sizes of the stars' Roche lobes must be, like their masses, much the same as one another, each extending roughly to the mid-point between them, some 9/sin *i* Gm from either; even the larger component, with a radius of 7/sin *i* $R_\odot$ or 4·8/sin *i* Gm, is well short of that dimension, so it may be concluded that neither star fills its Roche lobe and so no mass transfer is occurring at present, despite the anomalously short orbital period of this pair of evolved stars. Indeed, it appears on the face of it unlikely that there has ever been any mass transfer, otherwise the near-equality of the masses would have to be put down to a remarkable coincidence. As matters stand, it seems natural that two stars with closely similar masses should be evolving simultaneously, with the slightly more massive one a bit further advanced in its evolution than its companion.

Whichever of the classifications mentioned in the last paragraph but three is favoured, one would expect that in a system consisting of a K star and a G star, with the K component the brighter by a magnitude or more, the dip from that component would have to be several times stronger than that of the other. But no, the dips, though not equal, differ only by about 15%. In the radial-velocity traces it is the smaller, and presumed hotter, star, which gives the less-wide dip, whose dip has slightly the larger equivalent width of the two.

It is at this point that this discussion begins to become unglued, because the luminosities, in the B photometric band that corresponds roughly to the wavelengths observed by the *Coravel* instrument, must be rather more disparate than the equivalent widths of the radial-velocity dips, since the spectrum of the hotter component will not match the (K2) mask in the *Coravel* as well as the K-type spectrum does. The hot component, therefore, may be expected to have a considerably higher flux in B light than the cool one; but at the same time, their relative rotational velocities and accordingly their radii are in the ratio of about 2·3 to 1, so the cool star should have a surface area about five times larger than its companion. The apparent need for about a sevenfold difference in B surface brightness seems like a tall order and indicates that some error or oversight is falsifying this discussion. The spectral classifications[66,67], which make the K star decisively the brighter in V magnitude (and *a fortiori* in B), are in general agreement with the fivefold difference proposed for the surface areas of the stars; the incongruity is the near-equality of the areas of the two dips seen in radial-velocity traces such as that of Fig. 29. There is evidently scope and incentive for a detailed study of this interesting system.

HD 192785

P	$=$	19·2735 ± 0·0016 days	$(T_0)_7$	$=$	MJD 54910·255 ± 0·010
γ	$=$	−23·23 ± 0·08 km s^{-1}	$a_1 \sin i$	$=$	1·56 ± 0·05 Gm
K_1	$=$	5·89 ± 0·18 km s^{-1}	$a_2 \sin i$	$=$	11·59 ± 0·06 Gm
K_2	$=$	43·73 ± 0·23 km s^{-1}	$f(m_1)$	$=$	0·00041 ± 0·00004 $M_\odot$
q	$=$	7·43 ± 0·24 ($= m_1/m_2$)	$f(m_2)$	$=$	0·1674 ± 0·0026 $M_\odot$
e	$\equiv$	0	$m_1 \sin^3 i$	$=$	0·215 ± 0·004 $M_\odot$
ω	is undefined in a circular orbit		$m_2 \sin^3 i$	$=$	0·0290 ± 0·0015 $M_\odot$

R.m.s. residual (unit weight) $=$ 0·42 km s^{-1}

Radial-velocity traces of HD 192785 exhibit two extremely similar dips, of generous depth and modest broadening (Fig. 31). There seem not to be any useful velocities in the literature to add to those recently observed from Cambridge, which produce the orbit shown in Fig. 32, the primary velocities being half-weighted. As in the case of HD 191179, a pair of velocities has been given by Osten & Saar[66], who discovered the SB2 nature of the system;

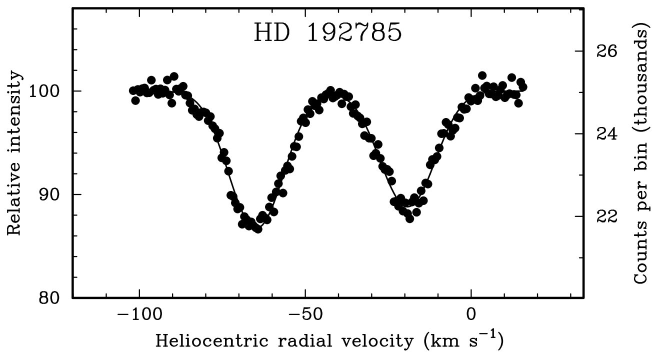

FIG. 31

Radial-velocity trace of HD 192785, obtained on 2009 August 20.

TABLE XXI

Cambridge radial-velocity observations of HD 192785

Date (UT)	*MJD*	*Velocity*		*Phase*	*(O–C)*	
		Prim. *km s⁻¹*	*Sec.* *km s⁻¹*		*Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
2008 Nov. 7·85	54777·85	−19·2	−52·9	0·130	0·0	+0·2
11·87	781·87	−26·3	+0·2	·339	0·0	+0·3
12·93	782·93	−28·5	+10·2	·394	−0·6	−0·9
14·74	784·74	−28·6	+20·0	·488	+0·5	−0·4
18·94	788·94	−24·2	−11·3	·706	+0·6	−0·1
22·77	792·77	−18·6	−59·6	·904	−0·2	−0·3
25·83	795·83	−17·4	−63·7	1·063	+0·4	−0·1
Dec. 2·87	802·87	−28·6	+15·6	·428	−0·1	−0·5
6·87	806·87	−26·4	+5·6	·636	+0·7	+0·1
7·82	807·82	−24·5	−5·6	·685	+1·1	+0·3
9·81	809·81	−22·4	−33·9	·788	−0·6	−0·2
11·83	811·83	−18·6	−58·2	·893	0·0	−0·7
2009 June 24·10	55006·10	−17·4	−66·0	11·973	0·0	+0·3
July 5·08	017·08	−28·7	+19·2	12·543	+0·2	+0·3
6·06	018·06	−27·7	+13·9	·593	+0·4	+0·7
20·10	032·10	−26·1	−5·1	13·322	−0·3	−1·0
30·06	042·06	−19·0	−46·4	·839	+1·1	0·0
Aug. 18·97	061·97	−18·8	−53·9	14·872	+0·3	−0·4
20·11	063·11	−17·6	−63·3	·931	+0·3	−0·4
21·96	064·96	−17·4	−66·3	15·027	0·0	0·0
24·95	067·95	−20·2	−40·7	·182	+0·6	+0·7

unfortunately, at –94: and –45 km s^{-1} they make another altogether impossible pair — one of them would fall far below the bottom of the box enclosing the graph of Fig. 32 — so they have not been included either in that figure or in Table XXI.

Barker[69] has said that HD 192785 is a Be star, whereas Motch *et al.*[70] have called it K0V. More probable than either of those is the Osten & Saar[66] classification as K3 IV + K2 IV, although those types may seem a little too late to agree readily with the $(B-V)$ colour index of $1^{m}\cdot05$ derived from *Tycho*. The exciting feature of HD 192785 is that, although the two stars give the impression from the radial-velocity traces of being almost as alike as two pins, they have extraordinarily different masses, with a ratio of about 7½ to one. In all respects it is uncannily similar to HD 61396, whose remarkable nature[71] was also discovered in Cambridge, though the period of HD 192785, at 19 days, is even shorter than that of HD 61396 (34 days).

The mean equivalent widths from the 17 traces that were reduced with all the dip parameters 'free' are 2·52 ± 0·05 km s^{-1} for the primary and 2·74 ± 0·03 km s^{-1} for the secondary. Thus the primary, as the more massive star is deemed to be, has on average a very slightly smaller dip signature than the secondary. The mean $v \sin i$ values are 10·7 ± 0·3 and 10·6 ± 0·3 km s^{-1} for the primary and secondary, respectively. The rotations of the stars must certainly be synchronous with the orbital revolution; rotation at 11 km s^{-1} in a period of 19·3 days implies that the projected radii of both stars are about 4·2 $R_{\odot}$. It is unfortunate that we have no information, in particular the parallax, from which to estimate the inclination. The sum of the masses, multiplied by the unknown factor $\sin^3 i$, is only 0·24 $M_{\odot}$; on the purely hypothetical basis that maybe the stars started out with masses near 2 $M_{\odot}$ each and that no large proportion of the total has been

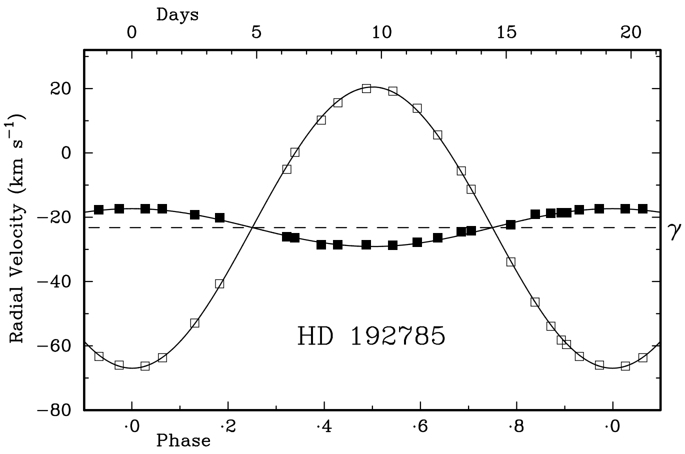

FIG. 32
Orbit of HD 192785.

lost to the system during the evidently considerable evolution of what is now the secondary component, we might hazard a guess that the unknown factor is something like 1⁄16. That would make $\sin i$ about 0·4 (with much less uncertainty, being a cube root) and i about 24°; the stars would have radii of ten or eleven solar radii, implying luminosities of MK class III–IV.

Regardless of the inclination, the projected radii of the stars, 4·2 $R_\odot$ or 2·9 Gm, can be compared directly with their projected separation, $(a_1+a_2)\sin i$, of 13 Gm. Eggleton[72] has given an expression for the size R_L of the Roche lobe, in relation to the separation a, in terms of the mass ratio q:

$$\frac{R_L}{a} = \frac{0{\cdot}49q^{2/3}}{0{\cdot}6q^{2/3} + ln(1+q^{1/3})}$$

Taking the inverse of the q value in the table of orbital elements above, in order to view the situation from the perspective of the secondary star, we find $R_L \sim 0{\cdot}22a$ or 2·9 Gm — just the size of the secondary star itself, strongly suggesting that that star fills its lobe and is still transferring mass onto the primary. Clearly HD 192785 joins HD 191179 — and indeed a number of others among those discussed in this paper — as an interesting system that deserves further study.

BI Delphini

P	=	7·2535 ± 0·0005 days	$(T_0)_{34}$	=	MJD 55023·545 ± 0·006
γ	=	+13·10 ± 0·27 km s^{-1}	$a_1 \sin i$	=	7·51 ± 0·04 Gm
K_1	=	75·3 ± 0·4 km s^{-1}	$a_2 \sin i$	=	7·35 ± 0·14 Gm
K_2	=	73·7 ± 1·4 km s^{-1}	$f(m_1)$	=	0·322 ± 0·005 $M_\odot$
q	=	0·979 ± 0·019 (= m_1/m_2)	$f(m_2)$	=	0·301 ± 0·017 $M_\odot$
e	≡	0	$m_1 \sin^3 i$	=	1·23 ± 0·05 $M_\odot$
ω		is undefined in a circular orbit	$m_2 \sin^3 i$	=	1·259 ± 0·027 $M_\odot$

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

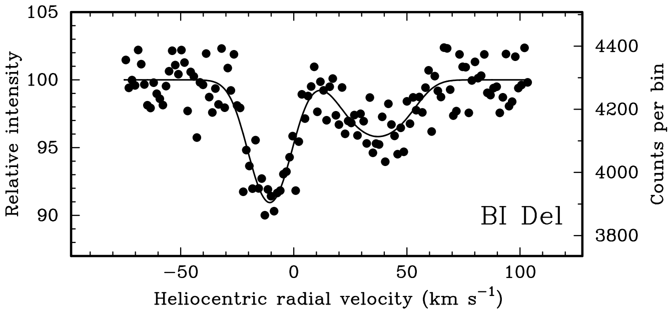

FIG. 33

Radial-velocity trace of BI Del, obtained on 2009 August 18. Purely for cosmetic reasons, the 'bin counts' have been summed pairwise. They are still totally independent counts — not running means — but the count per bin is doubled at the cost of halving the number of bins.

BI Del is quite faint for observation with the *Coravel*. Two observations were made of it at the beginning of this programme in late 2008, while it was still accessible in the evening sky; but then, like 2E 1848·1 +3305 and for the same reason, it was not observed again until near the end of the programme when less observing time was occupied by measurements of the more easily observed stars. Although its orbit has now been determined, the nature of the system remains enigmatic.

The photometric variability of BI Del was discovered[73] at Simeis in 1933 by Beljawksi, who identified it as an Algol system whose photographic magnitude varied between $11^{m}\cdot4$ and $13^{m}\cdot3$. An investigation of it was made by Kordilewski[74], who gave its period as 7·2527 days and noted the duration of the minimum as 12 hours; it seems, however, that no actual light-curve has ever been published. The bare facts were duly listed in the first edition of the *General Catalogue of Variable Stars*[75]. Very little more seems subsequently to have been discovered about it, with the same data, plus a spectral type of G0, being transcribed into successive catalogues of eclipsing stars[76–80]. The present Catalogue[1] gives, however, a maximum magnitude and $(B-V)$ colour index derived from *Tycho*, of $10^{m}\cdot604$ and $0^{m}\cdot889$, respectively; they are nothing like as accurate as might be implied by the precision with which they are quoted — *Vizier*, for example, gives the results from the identical source as $10^{m}\cdot68$ and $1^{m}\cdot087$. It is to be presumed that the *Tycho* magnitudes would average indiscriminately values obtained during eclipse with those obtained at maximum light, but in actual fact (and in most respects unfortunately) none of them has a phase within half a day of a time of primary minimum. Certain other quantities have accrued in some of the catalogues, but they are viewed here as being so speculative that it is better largely to refrain from quoting them; just as an example, however, the mass ratio is given by the same authors[80] under two different assumptions as 0·72 and 0·02.

The total amount of astrophysical information known about BI Del, apart from the photometry, is probably included in just a few lines in a 1996 paper[81] by Popper. He obtained spectra for a substantial number of late-type eclipsing systems with the *Hamilton* échelle instrument[82] on the Lick 120-inch telescope, with a resolution that he indicated to be about 5 km s^{-1}, *i.e.*, $R \sim 60\,000$. The

TABLE XXII

Cambridge radial-velocity observations of BI Delphini

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	Sec. km s⁻¹	*Phase*	*(O−C)* Prim. km s⁻¹	Sec. km s⁻¹
2008 Nov. 7·86	54777·86	+66·2	—	0·129	+1·1	—
22·84	792·84	+38·7	—	2·194	−0·4	—
2009 July 4·06	55016·06	+89·0	−57·0	32·968	+2·1	+1·7
5·07	017·07	+69·8	—	33·107	−2·1	—
6·09	018·09	+12·9	—	·248	−1·1	—
7·07	019·07	−41·6	+63·0	·383	+1·3	−4·3
10·09	022·09	+36·7	—	·799	+0·7	—
20·09	032·09	+46·2	—	35·178	+0·2	—
22·11	034·11	−59·4	—	·457	+0·2	—
24·09	036·09	+4·6	—	·729	+1·3	—
25·09	037·09	+62·5	—	·867	−1·2	—
26·04	038·04	+88·2	−60·2	·998	−0·2	0·0
30·08	042·08	−57·1	+85·5	36·555	+0·8	+3·7
Aug. 12·05	055·05	−28·8	+56·8	38·343	−0·1	+3·2
16·07	059·07	+73·1	−49·7	·898	−0·3	−4·1
18·01	061·01	+51·4	−18·3	39·165	0·0	+5·9
19·00	062·00	−9·1	+38·8	·302	+1·9	+2·5
20·06	063·06	−60·0	+83·4	·448	−1·7	+1·1
21·00	064·00	−54·4	+77·1	·577	−0·7	−0·7
22·97	065·97	+55·1	−32·6	·849	−1·8	−3·0
24·99	067·99	+66·3	−37·8	40·127	+0·8	+0·2
28·07	071·07	−59·3	+78·6	·552	−0·9	−3·7

entry for BI Del in his Table 1 shows that he had five spectra of it, and saw them as double-lined. From the largest observed velocity separation, taken in conjunction with the period known from the eclipses, the sum of the masses had to be at least 2·6 $M_\odot$. He gave a 'mean spectral type' of K3:, by the extraordinary method of simply measuring the total equivalent width of the Na I *D* lines of the two components and comparing it with a calibration curve. In a discussion, occupying only six lines, of the system, he repeated some of that information, concluded from the masses being > 1 $M_\odot$ that BI Del is an evolved detached system, and also said "The hotter component has sharper lines". Unfortunately he did not say anything further about the differences between the components, or disclose the velocities that he obtained, or derive an orbit from them (as ought to have been easy to do from five double-lined data and the known orbital period). In fact, since he had actually set out to identify systems which might yield accurate masses for late-type main-sequence stars, once he discovered the masses in BI Del to be super-solar he had no further interest in the system. Mr. A. Misch has kindly informed the writer that it is not certain whether Popper's (digital) data still exist, but even if they do they would now be very difficult to locate, and (being in raw form) to reduce. Other spectral types that have been given for BI Del are K0[79] and "(A8) + G0"[78].

Coravel radial-velocity traces of BI Del were initially found to yield one weak but reasonably measurable dip. They duly indicated a circular orbit with the period expected from the photometry. The eclipse depth of $1^{m}{\cdot}9$ that is usually quoted, and was determined in 'photographic' light whose effective wavelength is probably close to that of the *Coravel*, corresponds to a brightness factor of 5·8 and thereby implies that the brightness ratio between the components is

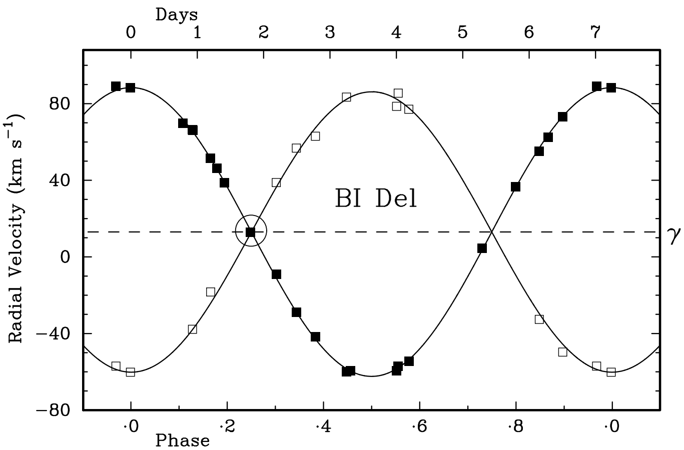

FIG. 34

Orbit of BI Del. The large open circle indicates the time of eclipse, according to the photometric ephemeris, and its diameter indicates the approximate duration listed for the eclipse. The star that the observer thought he was measuring ought to have been eclipsed at the time of the observation that falls so centrally within the circle.

at least 4·8 to 1. Since the components are evidently of different temperatures, the ratio would be appreciably different at the *D* lines where Popper observed. If it were much more than 4·8 he would probably have been hard pressed to see the fainter component, so the chances are that the ratio is smaller there, which implies that it is the hotter star that is the brighter. That is the reverse of what one might normally expect in a system in which one star has evolved away from the main sequence. Even more troublesome is the phasing of the eclipses, whose ephemeris shows that they occur at phase ·25 in the spectroscopic orbit — the conjunction at which the star whose radial velocities are observed is *behind* its companion and therefore is the one that is eclipsed then. But one of the *Coravel* observations made early in the campaign, that of 2009 July 6·09, was made (in all innocence) almost exactly at the time of an eclipse, both according to the orbit determined here (which finds its phase to be ·248) and according to the eclipse epoch that was explicitly shown on the Mt. Suhora Observatory web site[83] as 2009 July 6·101; yet the star did not appear conspicuously fainter on that occasion than it normally does, nor was the character of the trace noticeably different. It is impossible to accept that the radial-velocity traces could refer to the fainter component of the binary, because the cross-correlation dips in them ought then be reduced in depth by a factor of at least 5·8 by dilution with the light of the primary and would be too weak to measure.

In a few of the early radial-velocity traces there seemed to be some indications of a very exiguous second dip. At first the observer was inclined to doubt its reality, but then it did seem that it moved in anti-phase to the more observable one, and efforts to measure it were redoubled, with the result that it is now possible to offer a reasonably well determined double-lined orbit. A *Coravel* trace showing both dips is reproduced as Fig. 33. It has been necessary to weight the very weak one ⅛ in comparison with the other to bring the weighted variances into approximate equality. On occasions when the radial velocities of the two components were very different from one another, they were usually observed in separate integrations, though they are listed on the same line in Table XXII against their mean time. The elements of the orbit, illustrated in Fig. 34, show that the star that produces the excessively weak dip (and is here regarded as the secondary) is probably slightly the more massive of the pair.

The projected rotational velocities are 13·3 ± 0·6 km s^{-1} for the component that is being called the primary, and about 24 km s^{-1} for the one that is only marginally measurable, whose dips have a mean area that is about six-tenths that of the primary (but their depths are further diminished by that area being spread over a substantially larger width). Popper's assertion[81], quoted above, that the hotter component has the narrower lines, certainly seems to identify the primary as the hotter star. In that case, the secondary's spectrum, being of later type, might be expected to match the mask in the *Coravel*, which corresponds to the spectrum of Arcturus (K2), at least as well as (if not better than) the primary's. Thus the secondary star could be expected to be little more than half as bright as the primary, so (very roughly) $\Delta m \sim 0^{m}{\cdot}6$. If the primary eclipse, which occurs at phase ·25 in the spectroscopic orbit (nothing has ever been said about the secondary eclipse) is total, then its depth could be as much as $1^{m}{\cdot}1$ — but not the $1^{m}{\cdot}9$ that has been constantly copied into the catalogues ever since it was asserted by Beljawski[73].

There seems still to be considerable doubt about the photometric properties of BI Del; observers tend simply to list times, but not depths, of minima, and the writer has not been able to find any actual light-curve at all. Sandig[84] implied that the depth of the primary minimum is only $0^{m}{\cdot}7$. Such a revision could alleviate some of the difficulties noted above, but it has not been confirmed. Some agreement on the spectral types would be advantageous too. In summary of this section, therefore, we may say that although it has been possible to produce a double-lined orbital solution for BI Del, a real understanding of that system will need to await comprehensive photometry and proper spectroscopy, which will no doubt enable the presently scattered and seemingly incompatible pieces of the jigsaw to be assembled (with others yet to be located) into a coherent whole.

Concluding remarks

This paper has provided orbital information for a considerable number of interesting binary systems, most of which have periods that are so short in relation to the sizes of the stars concerned as to result in captured rotations that are rapid enough to be the prospective drivers of the chromospheric activity that warranted the objects' inclusion in the Catalogue[1] in the first place. There are, however, three exceptions to that generalization: the components of HD 73712, HD 93915, and HD 142680 have rotational velocities too small (probably <2 km s^{-1}) to measure with the *Coravel*. In addition, the Catalogue includes other stars that have been observed during the current campaign and have proved either to have relatively long orbital periods and slow rotations or even to show no radial-velocity variations at all. Results on those stars will be presented in due course, but it is already clear that some mechanism(s) other than enhanced rotational velocities must be sought for their activity.

Acknowledgements

I am most grateful to Dr. Z. Eker and Miss N. Filiz Ak for providing a list of those Catalogue entries that needed to have their orbits determined, and to Dr. F. C. Fekel for allowing me to quote and use his refined value for the period of HD 31738. I am grateful, too, to Drs. G. Cutispoto and L. Pastori for information regarding their observations of HD 191179, to Mr. A. Misch for information about *Hamilton* échelle data, and to Dr. D. P. Schneider for information about the Black Moshannon Observatory. Much bibliographical assistance has been given by Mr. M. Hurn, Librarian at Cambridge. Other people and organizations who have very promptly and willingly assisted on the bibliographical front include Dr. J. M. Kreiner of Krakow (Mt. Suhora) Observatory, the BAA Variable Star Section (Messrs. R. Pickard, J. Isles, D. Loughney, and A. Markham), and the U. S. Naval Observatory. Mr. Pickard has, moreover, placed the 2E star and BI Del on his own photometric observing programme, and has already produced a useful colour index for the former.

References

(1) Z. Eker *et al.*, *MNRAS*, **389**, 1722, 2008.
(2) R. F. Griffin, *The Observatory*, **128**, 448, 2008. (See top of p. 449.)
(3) [Announced by] K. G. Strassmeier *et al.*, *A&AS*, **142**, 275, 2000.
(4) G. S. Vaiana *et al.*, *ApJ*, **245**, 163, 1981.
(5) T. A. Fleming, I. M. Gioia & T. Maccacaro, *ApJ*, **340**, 1011, 1989.
(6) F. Favata *et al.*, *A&A*, **277**, 428, 1993.
(7) G. Tagliaferri *et al.*, *A&A*, **285**, 272, 1994.
(8) G. Cutispoto *et al.*, *A&AS*, **115**, 41, 1996.
(9) A. J. Baker *et al.*, in J.-P. Caillault (ed.), *Cool Stars, Stellar Systems, and the Sun* (*Eighth Cambridge Workshop*) (*ASP Conf. Series*, **64**), 1994, p. 550.
(10) P. Hut, *A&A*, **99**, 126, 1981.
(11) G. Cutispoto *et al.*, *A&A*, **364**, 205, 2000.
(12) G. Cutispoto, S. Messina & M. Rodonó, *A&A*, **400**, 659, 2003.
(13) M. Gioia *et al.*, *ApJS*, **72**, 567, 1990.
(14) J. T. Stocke *et al.*, *ApJS*, **76**, 813, 1991.
(15) F. Favata *et al.*, *A&A*, **295**, 147, 1995.
(16) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(17) G. W. Henry, F. C. Fekel & D. S. Hall, *AJ*, **110**, 2926, 1995.
(18) R. F. Griffin, *The Observatory*, **116**, 233, 1996 (Paper 129).
(19) V. Straižys & E. Meistas, *Acta Astr.*, **30**, 541, 1980.
(20) A. J. Cannon & M. W. Mayall, *HA*, **112**, 52, 1949.
(21) F. M. Walter, *ApJ*, **306**, 573, 1986.
(22) E. L. Martin *et al.*, *A&A*, **282**, 503, 1994.
(23) K. N. Grankin, *IBVS*, no. 4042, 1994.
(24) K. N. Grankin *et al.*, *Astr. Reports*, **39**, 799, 1995.
(25) K. N. Grankin *et al.*, *A&A*, **479**, 827, 2008.
(26) F. C. Fekel, *PASP*, **109**, 514, 1997.
(27) C. W. Allen, *Astrophysical Quantities* (Athlone, London), 1973, pp. 206, 209.
(28) W. P. Bidelman & D. J. MacConnell, *AJ*, **78**, 687, 1973.
(29) V. V. Makarov, *AJ*, **126**, 1996, 2003.
(30) M. M. Shara, D. J. Shara & B. McLean, *PASP*, **105**, 387, 1993.
(31) M. M. Shara *et al.*, *PASP*, **109**, 998, 1997.
(32) K. G. Strassmeier *et al.*, *A&AS*, **125**, 11, 1997.
(33) K. G. Strassmeier *et al.*, *ApJS*, **69**, 141, 1989.
(34) T Lloyd Evans, *SAAO Circ.*, no. 10, 11, 1986.
(35) L. Balona, *SAAO Circ.*, no. 11, 21, 1987.
(36) F. C. Fekel, T. J. Moffett & G. W. Henry, *ApJS*, **60**, 551, 1986.
(37) S. Randich, R. Gratton & R. Pallavicini, *A&AS*, **100**, 173, 1993.
(38) [Announced by] B. Nordström *et al.*, *A&A*, **418**, 989, 2004.
(39) G. Cutispoto, *A&AS*, **111**, 507, 1995.
(40) G. Cutispoto, S. Messina & M. Rodonó, *A&A*, **367**, 910, 2001.
(41) B. V. Kukarkin *et al.*, *New Catalogue of Suspected Variable Stars* (Nauka, Moscow), 1982, p. 58.
(42) [Announced by] C. Koen & L. Eyer, *MNRAS*, **331**, 45, 2002.
(43) J. R. King *et al.*, *AJ*, **125**, 1980, 2003.
(44) A. R. Daane, J. R. King & S. C. Schuler, *AJ*, **133**, 2654, 2007.
(45) A. Bečvář, *Atlas Skalnaté Pleso II* (Přírodovědecké Vydavatelství, Praha), 1951, p. 280.

(46) D. L. Nidever *et al.*, *ApJS*, **141**, 503, 2002.
(47) R. G. Aitken, *New General Catalogue of Double Stars Within 120° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1932, vol. **1**, p. 667.
(48) R. G. Aitken, *LOB*, **4**, 101*, 1907.
(49) C. Fabricius & V. V. Makarov, *A&A*, **356**, 141, 2000.
(50) T. Mazeh & J. Shaham, *A&A*, **77**, 145, 1979.
(51) S. A. Otero & P. A. Dubovsky, *IBVS*, no. 5557, 2004.
(52) R. O. Gray *et al.*, *AJ*, **126**, 2048, 2003.
(53) R. Schild, *AJ*, **78**, 37, 1973.
(54) A. R. Upgren, *AJ*, **67**, 37, 1962.
(55) R. F. Griffin & P. C. Keenan, *The Observatory*, **112**, 168, 1992 (Paper 105).
(56) E. Böhm-Vitense, *ApJ*, **239**, L79, 1980.
(57) P. Hertz & J. E. Grindlay, *ApJ*, **278**, 137, 1984.
(58) P. Hertz & J. E. Grindlay, *AJ*, **96**, 233, 1988.
(59) L. O. Takalo & J. A. Nousek, *ApJ*, **326**, 779, 1988.
(60) L. W. Ramsey & D. P. Huenemoerder, in D. L. Crawford (ed.), *Instrumentation in Astronomy VI* (*Proc. SPIE*, vol. **627**), part 1, 282, 1986.
(61) F. R. Zabriskie, *Sky & Tel.*, **49**, 219, 1975.
(62) R. F. Griffin, *MNRAS*, **155**, 1, 1971.
(63) J. C. Kapteyn, *Plan of Selected Areas* (Hoitsema, Groningen), 1906.
(64) R. F. Griffin, *ApJ*, **148**, 465, 1967.
(65) O. J. Eggen, *PASP*, **102**, 242, 1990; *AJ*, **106**, 80, 1993.
(66) R. A. Osten & S. H. Saar, *MNRAS*, **295**, 257, 1998.
(67) [Announced by] G. Cutispoto *et al.*, *A&A*, **384**, 491, 2002.
(68) M. Duflot *et al.*, *A&AS*, **110**, 177, 1995.
(69) P. K. Barker, *AJ*, **89**, 899, 1984.
(70) C. Motch *et al.*, *A&AS*, **132**, 341, 1998.
(71) R. F. Griffin, R. P. Church & C. A. Tout, *New Astr.*, **11**, 431, 2006.
(72) P. P. Eggleton, *ApJ*, **268**, 368, 1983.
(73) S. Beljawski, *Nizhny Novgorod Veränderliche Sterne*, **4**, 254, 1934.
(74) K. Kordylewski, *Acta Astr.*, **3c**, 91, 1937.
(75) B. V. Kukarkin & P. P. Parenago, *General Catalogue of Variable Stars* (Nauka, Moscow), 1948, p. 150.
(76) H. K. Branciewicz & T. Z. Dworak, *Acta Astr.*, **30**, 501, 1980.
(77) E. Budding, *Bull. Inf. CDS*, no. 27, 91, 1984.
(78) M. A. Svechnikov & E. F. Kuznetsova, *Catalogue of Approximate Photometric and Absolute Elements of Eclipsing Variable Stars* (A. M. Gorky Univ. of the Urals, Sverdlovsk), 1990, p. 176.
(79) K. G. Strassmeier *et al.*, *A&AS*, **100**, 173, 1993.
(80) E. Budding *et al.*, *A&A*, **417**, 263, 2004.
(81) D. M. Popper, *ApJS*, **106**, 133, 1996.
(82) S. S. Vogt, *PASP*, **99**, 1214, 1987.
(83) [Announced by] J. M. Kreiner, *Acta Astr.*, **54**, 207, 2004.
(84) H.-U. Sandig, *AN*, **278**, 181, 1950.

*The second of the pages numbered 101 in that volume.

REVIEWS

Science & Islam: A History, by E. Masood (Icon, London), 2009. Pp. 240, 22·5 × 14 cm. Price £14·99 (hardcover; ISBN 978 1 84831 040 7).

I have in front of me two commonly used, university-level introductory astronomy textbooks. Turning to the section on the history of astronomy, the first book speaks of the pivotal rôle of the Greeks in the development of astronomy and then skips over the intervening 1500 years, sidestepping any mention of Islamic* scientists. The second book acknowledges the existence of Islamic astronomy but sums up its impact with a single sentence noting that the Arabs helped preserve the Greek legacy until its rediscovery by Europe, the implication being that nothing of significance occurred during this period. Is this really a valid representation of the scientific legacy of the Islamic civilization? And if not, how exactly did the Islamic scholars contribute to the venture of science? How is it that "the memory of an entire civilization and its contribution to the sum of knowledge has been virtually wiped from human consciousness [and] not simply in the West but in the Islamic world too"? And what happened? Why is science today in most of the Islamic world languishing? These are among the questions that Ehsan Masood seeks to address in his book, *Science & Islam: A History*, that is squarely aimed at the general readership. The book is billed as a companion to a popular BBC television series broadcast recently in the UK.

Over the course of the past four decades, the combined influences of an educated citizenry of Islamic origins across Europe and North America asserting itself, and the rise of a generation of historians keen to examine — for the first time in most instances — the large number of extant scientific manuscripts and willing to challenge entrenched viewpoints, has led to a careful review of the historical record, and where warranted, the righting of the record. However, the tremendous advances on the scholarly front — exploring the achievements of Islamic scientists and how these impacted the work of 14th–16th-Century European scholars — has yet to penetrate into the wider public consciousness. This is because well-written 'popular' books on the subject are rare. With the publication of Masood's refreshingly different *Science & Islam: A History*, the landscape is clearly changing. Far too many books on the subject are structured like a 'laundry list', with the authors more intent on establishing the relevance of Islamic science by quantity without distinguishing between the mundane and the significant, and certainly without offering an overarching narrative that establishes context and relevance. Masood's emphasis on context, combined with his easy prose, measured self-confident tone, and an effort to inject compelling human drama into the narrative, makes the present book — for the most part — wonderfully captivating.

The book can be divided into four sections. In the first section, consisting of the prologue and the first chapter, Masood introduces the *raisons d'être* for the book, outlines some of the more commonly voiced myths about the relationship between the two civilizations, and proceeds to demonstrate why those myths are incorrect. One commonly held myth is that the Western world's ignorance of

*Here, I follow the common convention and use the label 'Islamic' not to indicate a religious preference but rather to designate generations of scholars of different ethnicities and backgrounds — Muslims, Jews, Christians, Zoroastrians, Arabs, Persians, Armenians, Indians, *etc*. — who took advantage of the opportunity to pursue 'funded' scientific scholarship under the encouragement and patronage of a succession of Muslim dynasties that ruled over the lands stretching from southern Europe and West Africa, to the borders of China for approximately a millennium starting from the mid-7th Century.

the contributions of Islamic scientists is the result of minimal contact between the Christian and the Muslim worlds, perhaps even a deliberate shunning of one by the other, stemming from a protracted state of hostilities and tension. Certainly, the notions of 'hostility and tension' play a central rôle in how the relationship between the two civilizations has been crystallized in the popular Western imagination. However, contemporary historians[1] acknowledge that military conflict between the two civilizations was, for the most part, minor and peripheral and as Masood describes, the Muslim world and Christian Europe enjoyed "extensive and continuous contact … throughout the early and late middle ages". (This state of affairs encompasses the Crusades as well, which in the words of historian Jeremy Johns[1] "had less to do with the relationship between Christianity and Islam than with the internal stresses and strains of Christian Europe.") Arabic-speaking merchants pursued healthy trading relationships across Western Europe, Muslim courts regularly exchanged emissaries and gifts with their European counterparts, and early European scientists, for the most part, gave due credit to their Islamic predecessors and contemporaries. Based on information that Masood presents in Part III of the book, it would appear that the deterioration in the relationship between Christian Europe and the Islamic world, characterized by the rise in polemical attacks against Islam and Muslims and the downplaying of the contributions of the Islamic scholars, seems to have begun in the mid-14th Century. I wonder whether this turn of events is related to the very serious existential threat that the Ottomans posed to Europe soon after their arrival on the scene. Masood does not explore this particular idea in his book and in fact one could argue that his presentation generally only skims the surface; however, given that this book is aimed at the general public, I can appreciate the desire to avoid unnecessary complications.

In the second section, formally Part I of the book, Masood introduces the developments in the Islamic world, starting with the birth of Islam and spanning the following seven centuries, focussing on those that set the stage for the subsequent flowering of scholarship. He does so by using a neat literary device: he focusses on a select few influential personalities and creatively weaves their stories — strengths, challenges, flaws, personality quirks, and all — into his discussion of the evolving milieu, thereby infusing his narrative with human drama.

As compelling as the narrative is, there are a few 'eyebrow-raising' aspects that I would like to comment on: first, these early chapters make no mention of the rôle that the very faith of Islam played in providing an impetus for the study of science. Most Muslims would assert that various verses of the *Qur'an* that speak to the need to understand the natural phenomena, or the emphasis that the Prophet Mohammed placed on intellectual growth through an injunction to "seek knowledge everywhere, even if you have to go to China", played a pivotal rôle in, at the very least, setting the right mindset. The latter injunction is, for example, mentioned only once, and then in passing. Instead, the author adopts an instrumentalist view, suggesting that the impetus for the dramatic growth in scientific scholarship came from economic and political needs of the empire(s). No doubt these are important factors, but in positioning the narrative the way he has, Masood is indulging in oversimplification that detracts from the story more than benefits it.

Then there is a sentence discussing the seizing of the Caliphate by the Ummayads and, specifically, offering a reason for their doing so. Most Western readers are unlikely to give the sentence a pause. Muslims of the Shia' persuasion, however, are likely to be quite piqued, and needlessly too, since how and why the Ummayads came to power adds nothing to Masood's story.

And lastly, having recently finished reading Heinz Halm's book titled *The Fatimids and their Traditions of Learning*[2], I was surprised that Masood chose to profile the Andalusian period in a chapter of its own, but did not accord similar status to the Fatimids (or more generally, the Fatimids and the Ismailis). After all, *The Oxford History of Islam* notes that as a political entity, the Fatimid Empire was, at its height, the most powerful state in the Islamic world, and Halm refers to the epoch as "one of the most brilliant periods of Islamic history". To be fair, Masood has enumerated many of the accomplishments that the Fatimids/Ismailis were associated with, but the narrative about the milieu is cursory. We really don't get much of an insight into the motivations of the personalities involved, as we did in the earlier chapters, and the description offered contains some factual errors. One such error involves Masood confusing Caliph al-Hakim's Dar al-Ilm (House of Knowledge) with Dar al-Hikma, an institution that came into being much later. Halm offers an excellent description of the Dar al-Ilm. He notes that in terms of the evolution of the institutions of learning and scholarship, one of the several developmental themes that Masood seeks to highlight, the establishment of the Dar al-Ilm was a significant step forward. It was the first institution that brought research and instruction of a broad range of disciplines under a single roof, and whose scholars were supported by an independent endowment and awarded 'gowns of honour' to commemorate their accomplishments. The similarity in structure to the subsequent universities that sprang up in Europe makes one wonder whether it was this and not the other more famous Fatimid institution of learning, the al-Azhar, that was the archetype. Halm also offers several fascinating anecdotes, including a few about the bibliophilic proclivities of the Fatimid caliphs, whose incorporation into the present book would have further buttressed Masood's compelling narrative. These oversights aside, Masood deserves credit for his even-handed discussion of the Fatimids/Ismailis and for acknowledging the influence that they exerted over many of the greatest scientists of the Islamic world. Sectarianism within Islam has often led to their rôle being minimized or even glossed over.

In the third section of the book, Masood adopts a more classical presentation of Islamic sciences. He identifies a set of "areas of learning" and discusses a number of important individual scholars who took centre-stage over the course of seven centuries. As a consequence, the discourse begins to change from a narrative of individuals to a more factual listing of individuals and their specific achievements, and ceases to be as compelling. Personally, I think that Masood ought to have stuck to his original innovative approach. Both the narrative and the reader would have been much better served if the individual scientists and their achievements had been situated in their proper context. For example, the "dramatic impact" of the colourful trio, the Musa brothers, would have been much greater if all the references to their various escapades that appear throughout the book could have been collected together and offered to the reader alongside the story of their patron, Caliph al-Mamun, and of the institution to which they were intimately tied. And the narrative would have sparkled even more had Masood shared with us anecdotes about the troubles that the Musa brothers stirred up, or offered a much more descriptive account of the scene at the court of al-Mamun, and the reaction of the courtiers, when the Musa brothers unveiled their full-size mechanical tea-girl that actually served tea!

Additionally, I am somewhat puzzled as to why Masood decided not to speak of the great 11th-Century scholar, al-Biruni, who is generally recognized as one of the very best scientists of the Islamic world. Al-Biruni's contributions span a number of different fields. Here, I will simply touch upon his contribution to the discussion of planetary motions, especially since it has some bearing on an issue that always arises when discussing the accomplishment of Islamic scientists: if the achievements are as significant and as advanced as claimed, why did this tradition not give rise to an Islamic equivalent of Copernicus? From al-Biruni's writings we know that, as early as the 11th Century, Islamic astronomers were well aware of the heliocentric description of the Solar System advocated by some of the Indian astronomers, and a number of scientists (*e.g.*, al-Biruni, al-Balkhi, al-Sijzi, *et al.*) seriously considered the possibility that it was the Earth and not the Sun that moved[3]. It is my understanding that the debate was resolved in one of two ways: some — like al-Biruni — argued that as far as astronomy was concerned the issue was moot, especially if the associated mathematical models resulted in similar predictions; they left it up to the philosophers and physicists to sort out the true description of the Universe[4]. This stance suggests that for a segment of Islamic astronomers at least, 'theory' likely meant something very different from what it does today, that those astronomers were not interested in constructing a model of the Solar System that approximated its true nature; rather, they were interested in models that best facilitated accurate calculations. Other astronomers rejected the heliocentric model because in the absence of compelling evidence (such as observations of stellar parallax) to the contrary, there was no reason to abandon the 'null hypothesis'. Contemporary scientists who are critics of the superstring theory as well as those who consider science to be a purely empirical endeavour will appreciate this argument.

The fourth section of the book (formally Part III) is its weakest section. Masood uses this section as a platform to discuss a broad range of issues, some of which are important and clearly relevant to the author's overall effort — like the current sad state of science and other scholarly undertakings in the Muslim world and some of the factors that drove this decline — and others that not only detract from it but also unfortunately threaten to undermine the author's credibility. As a result, the section comes across as unfocussed. A particular egregious example of the latter is a sequence titled "Where did we come from?" and "Speculating about evolution". For reasons not entirely clear to me, Masood veers away from the conservative approach that serves him (and the reader) well over most of the book to make a rather far-fetched claim that Islamic scientists were already toying with the idea of evolution back in the Middle Ages. In support of this claim, he offers quotes from poetry and philosophical literature of the period that asserts that minerals, plants, animals, and humans form a hierarchy of sentient beings. In the writings of Rumi and al-Nakhshabi, at least, this idea, which can be traced back to Aristotle, neither refers to the physical origin of human beings nor to the idea of physical changes in populations of biological organisms over time, and suggesting otherwise is a huge stretch. Clearly, the manuscript — and especially Part III — would have benefitted from a rigorous reading by a knowledgeable editor. Even the more relevant (and insightful) parts of this section could have benefitted from such oversight as the discussion often veers off on interesting but ultimately distracting tangents rather than staying on message. Masood would have done well to restrict the total number of issues to a few important ones and explore these more fully.

Setting aside the problematic Part III, the present book is very well written. It is simple, clear, and — for the most part — is structured in a way that highlights the very human nature of the individuals involved. This makes the journey through history come alive. The discussion of novel robot-like inventions by colourful characters like the Musa brothers, the story of the Andalusian ibn-Firnas's attempt to build a glider and take flight, of the genius of ibn-al Haitham and al-Tusi, all make it eminently clear that Islamic science was characterized by fervent creativity and that, both individually and institutionally, this creativity spanned a spectacular breadth. In this regard, Masood's book is thoroughly successful. And the author also convincingly illustrates that the 'new' scientific developments in 15th- and 16th-Century Europe did not occur in a vacuum but rather they were directly informed by the on-going debate among Islamic scholars of the various challenges with which they were trying to grapple, and by the new mathematical techniques that the Islamic scientists were inventing to address those challenges.

Still, if the legacy of the Islamic scholars rests entirely on individual, largely technical, innovations, and the impact that these innovations had on the first (and perhaps, the second) generation of Renaissance scientists in Europe, one has to wonder why this would be of interest to anyone other than historians of science (and perhaps, that segment of the world's population for whom all this has direct historical significance), especially since these accomplishments occurred in support of a description of nature that has long since been superseded. In other words, why should the treatment of Islamic scientists in textbooks used in introductory astronomy and physics courses be any different from the treatment of the Steady State theory of the Universe, or of the cadre of scientists who toiled to understand the inconsistencies in the theory of radiation in the late 19th Century but whose achievements, technical or otherwise, have since been eclipsed by the emergence of quantum mechanics? With such questions in mind, I wish Masood had drawn out more fully and more clearly the wider (and in my view, much more important) impact of the Islamic sciences on the venture of science as we conceive it today. The contemporary theoretical programme of developing detailed, sophisticated, self-consistent, predictive mathematical models of the natural world is one such legacy. So is the central rôle of empiricism in today's science, including the very idea of experimentation and observations as the only legitimate way to arbitrate between competing models. These paradigm shifts are perhaps not as tangible as the Tusi couple or waterclocks of ingenious design, but they are Islamic science's most significant lasting gifts to the grand venture of science.

Finally, I would like to bring attention to a particular thought that Masood draws out in the final section of the book, of which contemporary scientists would do well to take note: even though the study of natural phenomena is sanctioned and even encouraged in the *Qur'an*, the pursuit of science in the Islamic world was, in the final analysis, largely a top-down affair. It flourished because of the support provided by the various political leaders. It does not seem to have enjoyed broad public support and at times when the privileged position of science was challenged, as in Caliph al-Mamun's time, rather than bringing the wider population on-side by winning over their hearts and minds, dissent was suppressed, ruthlessly even. In contrast to the often-evoked legacy of persecution of the scientists by religious authorities in Christian Europe, here we have an example of rationalists (scientists and philosophers) persecuting the theologians. Such historical episodes call into question the highly simplistic generalizations about the relationship between faith and science offered most recently by individuals such as Richard Dawkins. Ideologues, it seems, do not spring up exclusively only from among the faithful and a heavy-handed approach based on polemics often works against the long-term interests of those who wield it. Following this thought through, I wonder — as does Masood — whether the lack of an effort to cultivate grassroots support played a rôle in the ultimate decline of science in the Islamic world and, at least indirectly, is responsible for the current state of affairs. And if so, there is a lesson here for those who are working towards a revival of the sciences in the Islamic world. There is also a lesson here for those of us who practise science in the West. Governments come and go, and national scientific policies are subject to a number of forces, including economic cycles and whims of individual personalities in power. Ultimately, though, the long-term support for science can only be guaranteed if the scientists themselves strive to cultivate deeply rooted support within the public at large. — ARIF BABUL.

References

(1) J. Johns, in J. McManners (ed.), *The Oxford History of Christianity* (Oxford University Press), 2002.
(2) H. Halm, *The Fatimids and Their Traditions of Learning* (I. B. Tauris, London), 1997.
(3) F. M. Donner, in J. L. Esposito (ed.), *The Oxford History of Islam* (Oxford University Press), 1999.
(4) S. H. Nasr, *An Introduction to Islamic Cosmological Doctrines* (Thames & Hudson, London), 1978.

Mysteries and Discoveries of Archaeoastronomy: From Giza to Easter Island, by Giulio Magli (Springer, New York), 2009. Pp. 443, 24 × 16 cm. Price £24·99/$27·50/€27·50 (hardbound ISBN 978 0 387 76564 8).

Archaeoastronomy is a borderline subject lying half way between the exact science of astronomy (replete with meticulous observers who are steeped in scientific laws and computer hardware) and the human science of archaeology (with its 'Time Team' types on their knees in holes in the ground scratching away with small trowels and being jubilant when confronted with a bone fragment or potsherd). It is also a relatively new subject, being essentially founded by Sir Normal Lockyer (1836–1920) and then catapulted into notoriety by the likes of Gerald Hawkins (1928–2003) and Alexander Thom (1894–1985).

The proponents of the subject are drawn from the two disciplines mentioned above. Professor Magli, from the University of Milan, started in academia reading applied mathematics and then graduated to relativistic astrophysics, before "seeing the light". His book (first published in Italy in 2005) takes us on a guided tour of the world's archaeoastronomical monuments. We trip from Stonehenge to the Big Horn Medicine Wheel of Wyoming, USA, from the Taulas of Minorca to the Nazca zoomorphic geoglyphs of Peru, the Intiwatana stone of Machu Picchu to the Moais giants of Easter Island, the Egyptian Senmut astronomical ceiling to the Bent pyramid of Dashour.

His book is a comprehensive, well-illustrated, well-referenced, easily accessible intellectual joy. I loved the way Magli sidelined the "half-baked explanations" and "patronizing and ridiculous conjectures" that permeate archaeoastronomy, those being generated by people he refers to as "archaeo nutcases". I loved his common-sense approach and his insistence on data, accuracy, and observation. I loved his humility and humanity. We are not investigating the artefacts left behind by "howling barbarians". The constructors of Stonehenge and the Egyptian pyramids lived only a hundred or so generations ago. They were just as clever and thoughtful as we are today. The quest to understand their astronomy, their minds, and their motivations is difficult and rewarding and worthy of encouragement. And Giulio Magli's excellent book is an ideal place to start. — DAVID W. HUGHES.

The History of Western Astrology, by Nicholas Campion (Continuum, London), 2009. **Volume I: The Ancient World**, pp. 388, **Volume II: The Medieval and Modern Worlds**, pp. 371, 23·5 × 15·5 cm. Price £18·99 each (paperback; ISBN 978 1 4411 2737 2 and 978 1 4411 8129 9).

Astrology like astronomy has survived into the 21st Century. Well, I am convinced that the readers of *The Observatory* can all justify the survival of astronomy. But astrology?! What is wrong with modern society that astrology should flourish so? Is astrology just some anachronistic superstition that appeals to the sociologically marginalized and psychologically flawed? Should we all follow the likes of Bart Bok and George Abell and join the sceptics in CSICOP, the Committee for the Scientific Investigation of the Paranormal, or are we missing something due to an inbred myopia inculcated during our academic upbringing?

Historically there can be absolutely no doubt about the influence of astrological thinking. Civilizations started off convinced of the holistic intertwining of the earthly world of animals, humans, and physical surroundings, and the cosmic sphere of Sun, Moon, planets, and stars. Then we all thought that the cosmos could influence us, and by our rituals we could in turn influence it.

Nicholas Campion, an eminent scholar at the University of Wales, Lampeter, in the field of anthropology and cultural studies, starts at the very beginning when primitive astrology was the basis of early religions. We then progress carefully, thoroughly, and soundly through the intervening 5000 years or so. The first volume covers prehistoric myths and megaliths, the Mesopotamian and Egyptian cosmos, Platonic and Hellenistic astrology, and the Roman and early Christian view. Volume II concentrates on the decline after the fall of the Roman Empire and the 12th- and 13th-Century re-blossoming in the Arabic and European world. We then progress through the use of astrology in the Middle Ages, to the pagan revival of the Renaissance. The public thirst for predictions continued and famous astronomers such as Tycho Brahe, Johannes Kepler, Galileo Galilei, and John Flamsteed supplemented their incomes by dabbling. The subject is brought up to date by the discussion of topics such as genethliacal and Comtean astrology, Kabbalistic theories, the theosophical enlightenment, the Hermeticism and Neoplatonism of Carl Jung, and finally the New Age Movement.

Campion leaves few stones unturned and the large section of notes and the ample bibliography make these two volumes an excellent starting point for those who wish to dig deeper. The book, however, is not an in-depth investigation of the history of the nuts and bolts of astrology but more a history of the effect that astrology had and still has on politics and society. — DAVID W. HUGHES.

Full Meridian of Glory: Perilous Adventures in the Competition to Measure the Earth, by P. Murdin (Springer, Heidelberg), 2009. Pp. 187, 24 × 16 cm. Price £15·99/$27·50/€19·95 (hardbound; ISBN 978 0 387 75533 5).

If your interest was aroused by André Heck's reviews of Lequeux's book on François Arago (**128**, 501) and of Freriks' book on the Paris Meridian (**129**, 288), but your command of the French language is little better than that of this reviewer, then Paul Murdin's *Full Meridian of Glory* should help to satisfy your curiosity. Subtitled *Perilous Adventures in the Competition to Measure the Earth*, this modestly priced volume takes us on three journeys.

The first is across the globe in the footsteps of those French pioneers of geodesy, like Arago, who were determined to find an accurate scale for the hitherto woefully inadequate map of France and then go on to discover the true figure of the Earth, and all this amid the turmoil surrounding the French Revolution. And it's clear that the author did himself follow in some of those footsteps, at least in France, since his photographs of a number of significant locations accompany a well-written (but occasionally poorly proof-read) text.

The second journey is through time, in which the rôle of the meridian is discussed, particularly with regard to navigation, the subsequent haggling to choose a Prime Meridian — which, of course, finally fell to Greenwich — and the present 'GPS era' in which meridians, prime or otherwise, no longer have a strong practical importance.

And finally, we are taken on a journey across Paris, along the meridian, following the medallions dedicated to Arago, with sufficient detail provided that the book can act as a guide for those wishing to follow the trail for themselves. On the way, we are treated to some down-to-earth information for over-zealous devotees of *The Da Vinci Code*! — DAVID STICKLAND.

Rocket Science, by Alfred Zaehringer & Steve Whitfield (Apogee Books, Burlington, Ontario), 2008. Pp. 215, 15 × 23 cm. Price $21·95 (paperback; ISBN 978 1 894959 86 5).

Alfred Zaehringer coined, he says, the phrase "rocket science" midway in a life that has included building his own, serving in World War II, and rocket (*etc*.) development at Thiokol Chemical, Martin, LTV, and Ford Motor Company. He and Whitfield have produced an utterly charming and densely informative 215 pages of tables, illustrations (in the gloriously, knife-edged, crisp black and white that has largely vanished in the digital era), lists, and one-sentence paragraphs. And if they say it's a book, then it's a book. Where else would you go to find out who had more deck space per person, Shuttle astronauts or Columbus' crew; or the relationship among internal energy, enthalpy, and the Gibbs and Helmholtz free energies in the context of a rocket engine; or the details of the Proton *versus* Energia launch vehicles; or the total cost of a Mars mission compared to a major war?

Now for the unfortunate downsides: the effective epoch is spring 2004 (and *JWST* launch was due in 2010), and allowance for inflation is erratic, letting Columbus cross the Atlantic for $14000. Also the authors are clearly not astronomers, claiming that asteroids and comets differ only in size and orbit shape, never mind formation processes or composition, and that you experience partial free-fall in an elevator as it accelerates rapidly upward. But I *think* it must have been a deliberate leg-pull that modified a well-known quotation to say "a billion here, a billion there, and you soon have substantial costs." With apologies to nearly all, the standard version is "and pretty soon you're talking about real money."

But enough carping, with so much more to be learned. It costs (say in 2003) $5000 a pound to launch anything (get back on that diet, guys); the most efficient chemical fuel-oxidizer combination is hydrogen + fluorine; NASA has ten research/space centres, which is probably too many (curable); of 30 (US and USSR) Mars missions before *Odyssey*, 21 failed in whole or in part, which is also probably too many (not curable, but recent major improvement); Martin (1909) is older than Lockheed (1913), and Douglas (1920) older than McDonnell (1939), but Wright (1903 — yes, those Wrights) was the oldest and

Ling–Temco–Vaught (LTV, 1960) the youngest of the major US aircraft, and later space, contractors. Now for short answers to the first four questions posed in the first paragraph: (*i*) about the same, 40 square feet; (*ii*) since the rocket absorbs almost no energy from its environment while firing, Gibbs = enthalpy and Helmholtz = internal energy; (*iii*) Energia has about four times the payload capacity of Proton, but has not been used nearly so much; and (*iv*) again about the same, a few hundred \$G. And the — official — answer to the question we all want to ask, is that it hasn't actually been tried, but six of ten standard positions would require an elastic belt to hold the partners together in free fall.

When my copy of *Rocket Science* came (it is a review copy from another journal), I had originally intended to skim and pass on to a colleague. But it is a keeper! — VIRGINIA TRIMBLE.

One Giant Leap: Apollo 11 Forty Years On, by P. Bizony (Aurum, London), 2009. Pp. 160, 27 × 24·5 cm. Price £16·99 (hardbound; ISBN 978 1 84513 422 8).

Those old enough to have known the 1960s will have their own recollections of the many seminal events that shaped the history of that decade. The assassination of JFK in 1963; the fall of Khrushchev the following year; the Soviet Union's crushing of the Prague Spring in 1968, and in that same year Colin Cowdrey's 100th test match and century at Edgbaston! Then came July 1969! Surely the sight, on our black-and-white TV screens, of the fuzzy pictures of Neil Armstrong descending the ladder of the *Apollo 11 Eagle* lander and setting foot on the Moon's surface, will figure as one of the most iconic moments in the history of mankind. In *One Giant Leap: Apollo 11 Forty Years On*, Piers Bizony has produced an excellent account of that momentous event, and the developments of the NASA programme that led to it.

The first two chapters provide an overview of the early phases of the space race between the USA and the Soviet Union (with the latter usually the winners, with *Sputnik*, Gagarin, *etc*.). These cover the formation of NASA, the initial rocket programmes, and the first manned spaceflights. Bizony gives a fascinating account of the internal politics within NASA, and highlights quite rightly the seminal rôle played by James Webb, as the NASA administrator during the development of the Apollo programme. He emphasizes the key characteristics of Webb as a consummate politician and business man that shaped his decision making, not least in 'spreading the benefits' amongst as many States as possible to ensure maximum Congressional support for the (expensive) NASA programme, both in the 30 000 government employees and the ten-times that number in private-contractor staff. This historical perspective is fascinating reading. Other chapters describe the technology developments of the rocket launchers (leading to the 'cathedral-size' *Saturn V*), the spacecraft, orbiters and landers, ground-stations, and, not least, the choice of the pioneering astronauts themselves, who would quite literally risk life and limb. Each chapter is lavishly illustrated with superb colour photographs and Bizony enhances his history with numerous quotes from the players involved — astronauts, politicians, scientists, and celebrities. He concludes with a synopsis of current plans in NASA, ESA, and other agencies to return to the Moon, and possibly Mars, and leaves the reader with no doubt as to his view that this is a must for mankind. The final section of the book is a wonderful archive of colour photographs of the *Apollo 11* mission itself, many of which have not hitherto been published. It is timely that this book is published in 2009 — the International Year of Astronomy. It is very competitively priced and would form a marvellous present for any aspiring young astronomer, as well as enhancing the library of professional scientists. Highly recommended! — ALLAN WILLIS.

Ancient Light: A Portrait of the Universe, by D. Malin (Phaidon Press, London), 2009. Pp. 128, 29·5 × 25·5 cm. Price £29·95/$49·95 (hardbound; ISBN 978 0 7148 4932 4).

At the time that the specification of the *Anglo-Australian Telescope* was being finalized, in the late 1960s, the detector of choice was the photographic emulsion. The new hope, which came into play even while the telescope was being built, was Eastman-Kodak's IIIa, which had improved resolution and dynamic range. The *AAT* was built with a prime-focus camera to image the sky with these emulsions, and cameras for the spectrographs at the Cassegrain focus (and indeed a sky-imaging camera at that focus, which was only ever used a couple of times). The *AAT*'s observatory building was equipped with darkrooms on every floor, for easy access from every focal station. In 1974 the telescope had been completed and Joe Wampler, the first AAO Director, was near the end of his first tour of the observatory building. He came across a wooden crate on the floor in the control room, which he kicked, sourly asking no one in particular "What's this? Another darkroom?"

Of course, now CCDs are the main detectors everywhere in professional astronomy, totally replacing photography. Wampler brought his electronic detector (the IDS) to the *AAT*, and it took off into the electronic era in 1975, its first year of operation. However, having been delivered a telescope with excellent photographic capability, Wampler hired a photographer, David Malin, to exploit it. His career spanned the last decades of professional astrophotography. In this book, Malin, now retired, says that he hasn't made a picture in a darkroom since 2001, but between 1974 and then he became the world's premier professional astrophotographer, best known for his colour pictures made by the three-colour-addition and other processing techniques.

Colour photography is Malin's main fame and this book is startling because it has no colour pictures in it at all. But in reality Malin is the complete astrophotographer's astrophotographer and his new book has about 60 black-and-white pictures. They are spectacular pictures of galaxies, clusters, and nebulae, mostly from the *AAT*, taken originally for scientific purposes and processed by Malin, their celestial context set by black-and-white pictures of the constellations made by Akira Fujii. The book celebrates the 'silver century' of photography between its first applications to astronomy in the 1880's by Gill, Common, and Draper and its demise. Each photo has a well-drafted caption of a couple of hundred words describing the astronomical object in non-technical language. Malin has written an essay as a preface, describing the history of astrophotography and the part he played in it.

The production quality of the book is outstanding, a credit to Malin's originals and to the publisher, Phaidon Press. The pictures are large-format, high-resolution, and bright — it is amazing that the printing process can have reproduced the pictures at almost the same quality as the photographic process could deliver. The design is elegant, understated, and discreet, complementing the perfect pictures. Even the dust-jacket is a work of art, being matt black, punched through with a scattering of stars (the brighter ones with diffraction spikes) through which the book's cover shines white, shimmering when you handle the book as the dust-jacket waves into contact with the cover. If this does not satisfy your collector's instincts, a specially bound limited-edition is available with a signed, numbered, original photographic print of IC2188 — presumably Malin will have to go back into the darkroom one further time to make them.

Malin's book is a eulogy of the photographic technique that advanced astronomy for about 120 years. It is a book to possess, admire, and treasure. — PAUL MURDIN.

Star Vistas: A Collection of Fine Art Astrophotography, by G. Parker & N. Carboni (Springer, Heidelberg), 2009. Pp. 168, 30·5 × 25·5 cm. Price £31·99/$39·95/€39·95 (hardbound; ISBN 978 0 387 88435 6).

When a book has forewords by Arthur C. Clarke, Patrick Moore, and Brian May, you might expect something special, and you would not be disappointed. In one sense, this is a coffee-table book, full of beautiful pictures with only a small amount of text. But when you start to read the text you will begin to discover how remarkable these pictures really are — they were all taken from a back garden in the New Forest by an (admittedly very dedicated) amateur astrophotographer, with quite modest equipment, and yet they look at first glance, and indeed on closer inspection, as though they could have been taken with a large professional telescope.

The achievement has been made possible by a remarkable Internet collaboration between the two authors, who have never met or even spoken on the telephone. Greg Parker, a professor of photonics at the University of Southampton, is a keen amateur astronomer in his spare time and began deep-sky imaging in 2004, using Starlight Xpress colour CCD cameras coupled to an 11-in Celestron telescope and/or a 90-mm Takahashi refractor. Initially, he processed his images himself, but didn't particularly enjoy the amount of work involved in getting the best out of his raw images. Through a friend, he became aware of the work of Noel Carboni, who had already developed a reputation on an Internet forum ('Our Dark Skies') for transforming raw astronomical images into works of art, often using software written by him for this purpose. Although himself an astrophotographer, Carboni's main interest is in the processing of the images, so the two men complement each other's interests perfectly — and this book is the result.

The full-colour images are extremely impressive, ranging from detailed pictures of the Moon (one of which has had a starry background added, from a separate exposure) to a double-page spread of the Andromeda galaxy (pp. 64–5), which is an amalgam of some 30 hours of exposure over several years but looks like a single deep image, and images of distant clusters of galaxies. Most of the images are of star clusters and gas clouds in our own Galaxy, with a few images of individual bright stars. All the pictures have a short commentary, and full technical details of the exposure time and equipment used. The commentaries are well-written, generally accurate (I found only two minor errors: Polaris (p. 24) is of course in Ursa Minor, not Ursa Major; and the Sun will become a red giant (p. 7) in about 7 billion years, not 5 billion), and contain some interesting nuggets of information (did you know the Pleiades were called Freya's Hens by the Vikings? and can you find the three asteroid trails on the 8-hour exposure on pp. 91–2?). Some wide-field images are made up of a mosaic of several images, but the knitting together is so skilful that without the captions it would be impossible to tell which ones. Altogether, this is a book to savour — highly recommended. — ROBERT CONNON SMITH.

Capturing the Stars: Astrophotography by the Masters, by R. Gendler (MBI Publishing, Minneapolis), 2009. Pp. 160, 24 × 28·5 cm. Price £16·99 (hardbound; ISBN 978 0 7603 3500 0).

It's a mark of just how popular astronomical imaging has become when the reviewer, whilst not a practitioner himself, but trying to keep abreast of developments, finds that he has not heard of the large majority of the 25 individuals and five teams of two who form the contributors to this book. This may be because most of the contributors come from the USA with only one (Damian Peach) from the UK and most of the remainder from Europe.

Needless to say, since the editor is Robert Gendler (see the colour section in the 2007 February issue of this *Magazine* for examples of his work), the images themselves are spectacular and some of the best the reviewer has seen, the more admirable because the majority are taken by non-professional observers. There are contributions from *HST*, *CFHT*, and the *AAT* in the person of David Malin, and the standard reached here is truly remarkable. The pictures cover the whole gamut from aurorae and meteors, the Sun, Moon, and inner planets, comets and Jupiter, right out to clusters of galaxies.

It is difficult to pick a favourite but the wide-angle image of the Pleiades showing the extent of the nebulosity is impressive, and the barred spiral NGC 1300 with what appears to be a mini-spiral galaxy at its centre is also noteworthy.

Also welcome is the size and layout. This is a relatively small volume that does not need a coffee table for support and all the images are shown without reverting to printing across the spine of the book. There is a potted biography of each contributor, but those who want technical information about the equipment used will find no help on that front. This is purely a picture album but a very-well-produced one. Highly recommended. — ROBERT ARGYLE.

Shrouds of the Night, by D. L. Block & K. Freeman (Springer, Heidelberg), 2008. Pp. 456, 23·5 × 31 cm. Price £19·99/$39·95/€29·95 (hardbound; ISBN 978 0 387 78974 3).

In her preface to this book Vera Rubin suggests it is 'unconventional', containing as it does a mix of history, geography, physics, geometry, biography, art, poetry, botany, and religion interwoven with the basic astronomical theme. The astronomical theme itself is based upon the authors' well-known and lifelong studies of galaxies and their dusty contents (the shrouds). It ranges from the basic mechanisms of dust formation through the advent of photography and its impact upon galaxy classification to the part dust shrouds play in determining the morphology, dynamics, and evolution of galaxies, and the recent and dramatic 'lifting of the shrouds' occasioned by the use of infrared imaging.

Rubin's 'unconventional' epithet is perhaps an understatement. An eclectic mix of subsidiary contributions can often serve as a flavour-enhancing garnish to the main theme and thereby turn a potentially dry text into an entertaining as well as informative and often very personal tale. In this instance, though, the extras are so significant that they form a very definite and substantial part of the recipe. As such the result may not be to everyone's taste. I suspect you will either like the bombardment of new and interesting flavours and textures or you will consider them offensive and irritating distractions from the basic ingredient. In the case of the latter, a suitably standard textbook is probably not far away.

If the contents are unconventional, I found the physical and style formats just frustrating. It has the form of a traditional, high-quality, coffee-table book (heavy, large, and glossy) and that should be reserved, I think, for books that do, by dint of their overwhelmingly visual nature, only warrant the occasional or leisurely flick through. The content of this book deserves closer attention, but trying to read it with anything less than a firm and spacious desk at hand proved nigh impossible. With over 200 images, a significant proportion of the content is indeed visual, but woe betide anyone who happens upon an interesting one while in browse-mode. To ascertain any information about the images requires either a close inspection of the text (which might be several pages displaced either way) or, even worse in most cases, moving to the end of the (unwieldy) book where all the figure captions are grouped together. No doubt that format was used for convenience, but one wonders whose. Interesting contents, shame about the packaging! — DAVE PIKE.

From Fossils to Astrobiology: Records of Life on Earth and the Search for Extraterrestrial Biosignatures, edited by Joseph Seckbach & Maud Walsh (Springer, Heidelberg), 2008. Pp. 545, 23·5 × 15·5 cm. Price £180/$299/€199·95 (hardbound; ISBN 978 1 4020 8836 0).

Perhaps the oddest thing about this volume is that there is no discussion of exoplanets or habitable zones around stars or in the Milky Way. The Big Bang, yes (with primary conclusion that it was an uncaused event, rather than merely a state of very high temperature and density about 14 Gyr ago). Comet/asteroid impacts, yes (not the cause of most major extinction events and maybe not the whole story even for the Cretaceous–Tertiary boundary). Mars as a former habitat, yes (and the place to look is perhaps deep down, by analogy with some microbial communities in subterranean Earth). Even a chapter on regarding the Sun as a living entity (the author says yes!). But nothing about the item that most astronomers would say has been the major advance in astrobiology in the past 20 years.

The book is not the outcome of a conference (you can tell because none of the pictures of the chapter authors shows them with wine glasses in their hands) but the result of deliberate choice of topics and authors by the editors. Most of the authors are biologists, most of the chapters deal with aspects of the Earth's fossil record, and there are lots of stromatolites. In general, over the years, astronomers and physicists (and some chemists) have been optimistic about the possibilities for life, even intelligent life, elsewhere, and biologists pessimistic. Not surprisingly, therefore, this is in some sense a pessimistic book, with even one astronomer–author who is a strong supporter of SETI programmes concluding that intelligence is not generally selected for. — VIRGINIA TRIMBLE.

Finding the Big Bang, edited by P. J. E. Peebles, L. A. Page, Jr. & R. B. Partridge (Cambridge University Press), 2009. Pp. 592, 24·5 × 17 cm. Price £40/$72 (hardbound; ISBN 978 0 521 51982 3).

The names listed above are both authors and editors, for they have attempted the remarkable task both of compiling recollections of most living (and some deceased) scientists involved in the discovery of the cosmic microwave background radiation and of providing introductory material on the state of cosmology around 1960 and a summary of what has happened since 1970, ending with lessons, lists of measurements, missions, acronyms, references, and a useful index.

A question that has simmered for decades is who, if anyone, actually had a CMB detection before the 'Nobelable' one. Their answer is only Edward A. Ohm and his colleagues at Bell Telephone Labs a few years before Penzias and Wilson, though at least two or three sky-background measurements from the same period could have been pushed to detections if the measurers had known there was something interesting to look for. Sadly, Ohm declined to add his words to those of the 46 authors contributing, though his colleague David Hogg is here. At the time the compilation began in 2001, only Ralph Alpher of the famous predictors (George Gamow, Robert Herman, Robert Dicke, and to a certain extent Yakov Zel'dovich) was still alive, but he also is not among the contributors. Sections were provided by David Wilkinson, Donald Osterbrock, and Ronald Bracewell early in the process, and the living contributors include most of the folks you would expect, and some you might not (Judith Pipher, Geoffrey Burbidge, Michele Kaufman, Jayant Narlikar, and David Layzer). Oh, and there are two paragraphs from V. Trimble telling her story of "Gamow and the graduate student".

Perhaps what we most need reminding of is just how hard many people worked from 1965 to about 1970 to get information on the true shape of the spectrum and the first evidence for (dipole) anisotropy. This hard work continues — I write just hours after the *Planck* satellite (a mostly-CMB effort) and the *Herschel* satellite (aimed at infrared radiation from star and galaxy formation) successfully separated after their joint *Ariane-5* launch. Clearly no future book will be able to contain so nearly a complete collection of reminiscences from significant contributors to the later work. But I leave you with a question: was the Prof. Jakob L. Salpeter of Adelaide, who doubted the possibility of measuring preferred-frame effects from the CMB, the father of Edwin E. Salpeter? — VIRGINIA TRIMBLE. [Dr. Trimble informs us that Professor E. E. Salpeter's widow has confirmed that the answer to the last question is 'yes'. — Ed.]

Introduction to General Relativity, by L. Ryder (Cambridge University Press), 2009. Pp. 441, 25 × 19·5 cm. Price £35/$75 (hardbound; ISBN 978 0 521 84563 2).

Ryder's book on Quantum Field Theory is a classic text, so it is with a sense of anticipation that one opens his latest book on General Relativity. It is not a disappointment. As is almost inevitable with any good book on GR, much of the material is fairly standard, so it is the extras which become particularly relevant. In this case, there is some interesting historical commentary, and some illuminating discussions linking physical principles, such as on Mach's principle. To give an example, the fact that Lorentz boosts do not form a group, requiring rotations to be added, leads to Thomas precession. It is a nice example of the sort of illuminating remark which Rindler's books do so well, and in which this book also excels. All of this is backed up by a solid mathematical treatment. The level is reasonably high for an undergraduate on a physics programme, and will be suited to those who are most mathematically capable, or postgraduates with a fair degree of mathematical fluency. For less mathematically confident students, another of CUP's undergraduate textbooks, *General Relativity*, by Hobson *et al.*, is preferred.

In Ryder's book, the typical pattern of a chapter is a brief introduction of background, history, and motivation, followed by well-presented mathematical treatment of problems, some of which are covered in some depth and detail, and some relatively sophisticated situations are tackled. I think there are better and more comprehensive treatments of cosmology and the microwave background than here, including more on recent findings, but the development of the gravity theory is excellent, and there is a fabulous bonus for readers at the end, in the form of a final chapter on what might lie beyond General Relativity in a quantum theory of gravity. Of course, such a chapter is not the final word, but covers what might be called relevant considerations. For example, it explores very elegantly and persuasively the striking similarities between General Relativity and non-Abelian gauge theories, amongst other remarks on the Higgs mechanism, inflation, and superconductivity. It is fascinating. — ALAN HEAVENS.

The Galaxy Disk in Cosmological Context (IAU Symposium 254), edited by J. Andersen, J. Bland-Hawthorn & B. Nordström (Cambridge University Press), 2009. Pp. 512, 25·5 × 18 cm. Price £68/$135 (hardbound; ISBN 978 0 521 88985 8).

IAU symposium proceedings have gradually settled down into a fairly rigid format, enforced by publisher, assistant general secretary, and so forth. Thus, we have here the written versions of most of 70 oral presentations, running 6–12 pages each, and a list of most of the 120 poster presentations, which will appear with the on-line version, where there are also colour illustrations that appear here in glorious black and white. The (coloured) conference photograph allows us to see the faces of roughly the first three rows, enough of their upper bodies to note that at least two of the three editors were conventionally dressed, and 2–8-pixel images of the rest of the participants. The symposium and proceedings are dedicated to Bengt Strömgren, whose 100th birthday would have occurred in 2008 January, and who was president of the American Astronomical Society in 1966–67, when I gave my very first AAS talk at the end of the last afternoon session, for which he generously remained.

It is perhaps the reproduced old photographs, showing events of 1929, 1936–37, and 1957, in which Strömgren participated, that add most to the value of the text. The real-time photographs include Aage and Hans Bohr, Ole Strömgren, Ben Mottelson, Adriaan Blaauw, and some younger folks, in backgrounds indicating a couple of rather nice social events at Copenhagen Town Hall and the former Strömgren residence at Carlsberg (yes, the brewery, where taps used to run with various strengths of beer. Perhaps they still do. My last visit was in 1971.)

The participants included many of the best-known workers on galaxy formation and evolution, perhaps most succinctly indicated with the names of just the first, review, speaker from each of six sessions, S. D. M. White (unfortunately represented by only an abstract), K. C. Freeman, J. Bland-Hawthorn, Georges Meynet, J. Silk, and A. Burkert, with introductory and concluding remarks from Bengt Gustafsson and Rosemary Wyse. Undoubtedly the symposium had some 'wow' moments, and some fairly firm disagreements among speakers. These always happen, but they have left no obvious traces in the proceedings. American readers are likely to learn most from Johannes Andersen's discussion of ASTRONET and the European 'Roadmap' plans for international cooperation. No corresponding discussion of the US decadal survey appears. Half a dozen future or on-going surveys and missions are presented, all but SEGUE (SDSS follow-up) largely European. — VIRGINIA TRIMBLE.

Stellar Spectral Classification, by R. O. Gray & C. J. Corbally, SJ (Princeton University Press, Woodstock), 2009. Pp. 592, 25·5 × 17·5 cm. Price £59·95/$100 (hardbound; ISBN 978 0 691 12510 7), £38·95/$65 (paperback; ISBN 978 0 691 12511 4).

The foundations of stellar spectral classification can be traced back at least to Fr. Angelo Secchi, and the genesis of the present methodology lies in work carried out a century ago at the Harvard Observatory. However, these venerable antecedents don't negate its present-day significance, lying at the foundation of many more-obviously-quantitative studies, and continuing to develop hand in hand with the exploration of new physical domains (*cf.* the extension to L and T dwarfs) and with new observational technologies (exemplified by comparative spectral morphology in the IR, UV, and even X-ray domains).

The proceedings of occasional specialized meetings have partly documented some of these developments, but there has long been a conspicuous absence of an authoritative, graduate-level monograph on the subject. The Jascheks' *The Classification of Stars* has done service in that rôle, but even if readers don't share the lack of enthusiasm for it expressed in the review published in this *Magazine* (**108**, 29, 1988), its bias towards photographic techniques which were already dated at the time of its publication has rendered it of limited value for much of its life.

Enter Gray & Corbally, with a modern review of the field. An introductory chapter sets out not only the key historical developments leading to the MK system (today virtually synonymous with stellar spectral classification), but also the widely underappreciated philosophical principles which underpin it, and which have come to be identified with the 'MK process'. This chapter, and the following one, which provides an overview of both the basic two-dimensional classification scheme and the associated astrophysical processes, make for background reading that should engage anyone interested in these topics.

Subsequent chapters address in detail classification along the familiar OBAFGKM sequence, subtype by subtype, together with L- and T-type dwarfs, Wolf–Rayet stars, and 'Endpoints of stellar evolution'. The last is a bit of a *pot-pourri*, ranging from proto-planetary nebulae to supernovae, but otherwise discussion of subclasses (such as chemically peculiar stars, T Tauris, carbon stars, *etc.*) is integrated seamlessly into the main text at the appropriate points. While this core of the book is, arguably, destined principally for a specialist readership, or for reference, I was impressed that it could convey so much detail without descending into a mere catalogue of tedious minutiae. As one would expect, the pages are liberally illustrated with spectral sequences and other examples, the data for most of which the authors have made available in digital form through their web site — a valuable resource for reference, and for developing laboratory experiments.

The book concludes with discussions of "other classification systems" (essentially, just the BCD system, so pervasive and successful is MK classification), and classification of wide-field, low-dispersion spectra. It's salutary that the summary of automated methods of spectral classification leads inescapably to the impression that the most sophisticated algorithms in pattern recognition and correlation analysis still aren't competitive with what the eye and brain of the practised human classifier achieve with relative ease.

This volume is elegantly written, comprehensive, and authoritative (the authors' very considerable personal expertise is augmented by specialist contributions from Adam Burgasser, Margaret Hanson, J. Davy Kirkpatrick, and Nolan Walborn). Its utility is enhanced by extensive tables of classification standards, scrupulous referencing of primary sources, and thorough general and object indexes. The book stands head and shoulders above anything comparable, and will stay within my easy reach both for reference and to browse. Highly recommended — and you don't have to take my word for it: Princeton University Press (who justifiably describe the work as "definitive and encyclopedic") have made Chapter 1, *The History and Philosophy of Stellar Spectral Classification*, freely available on their web site (and have given Google Books permission to make pretty much the entire book accessible on-line). Do have a look. — IAN D. HOWARTH.

Transiting Planets (IAU Symposium 253), edited by F. Pont, D. Sasselov & M. Holman (Cambridge University Press), 2009. Pp. 571, 25·5 × 18 cm. Price £68/$135 (hardbound; ISBN 978 0 521 88984 1).

These are the proceedings of the 'Transiting planet' conference held at Harvard during 2008 May 19–23. Being unable to attend, I was really looking forward to their release and I was not disappointed! The volume is divided into four parts: (*i*) photometric searches for transiting planets, (*ii*) observational studies of transiting planets, (*iii*) planet formation, evolution, and atmospheres, and (*iv*) poster papers. It is worth emphasizing that this conference was concerned with information gained from *transiting* planets and not that from radial-velocity searches, and this is reflected in the content of the talks.

The first three sections take the form of 36 review talks giving the state of the subject at the time of the conference. I especially enjoyed Dave Charbonneau's 'Rise of the Vulcans' introductory and scene-setting talk. As this is a young and fast-moving area of research, some of the reviews have been overtaken by developments. Nonetheless, most are worthy starting points for new researchers to the field. I found the 66 poster contributions to be especially interesting as these, as is usually the case, contain much work in progress. Some of this is still to be published. Overall, this is a beautifully edited and produced volume that all serious libraries will want to possess.

Since IAU Symposium 253, we have seen the discovery of the first small planet from *CoRoT* and the launch of the *Kepler* mission, both of which are space-based experiments (but needing substantial ground-based follow-up). The subject will, no doubt, advance beyond all recognition over the next few years and I find myself already looking forward to the next dedicated conference. — DON POLLACCO.

Numerical Modeling of Space Plasma Flows: ASTRONUM–2008 (ASP Conference Series, Vol. 406), edited by N. V. Pogorelov, E. Audit, P. Colella & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2009. Pp. 324, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 692 9).

This volume contains about 40 eight-page papers based on presentations made at the 'ASTRONUM–2008' conference devoted to researches using computational methods in astrophysical and space-plasma systems. The brevity of these contributions combined with the wide diversity of research topics and methods discussed does not provide sufficient coherence for serious pedagogical purposes, but does give an intense flavour of the breadth and depth of current research in this area. Astrophysical topics include, for example, high-resolution cosmological simulations of galaxy formation including baryon gas and dark matter, MHD models of disc accretion onto magnetized stars, and general-relativistic MHD models of core collapse in massive stars leading to the formation of neutron stars and black holes. Nearer to home, a particular focus is on modelling the interaction of the solar-wind plasma with the local interstellar medium, the partially-ionized nature of the latter requiring consideration not only of the interaction of the plasma-field components, but also of the neutral interstellar atoms that interact with the solar wind on long mean-free-paths through charge-exchange reactions. Interest in this topic is stimulated not only by the on-going return of data from the now-distant *Voyager* spacecraft, but also by the launch in 2008 of the NASA *IBEX* mission that seeks to detect the energetic neutrals resulting from these reactions. In addition to overviewing recent results from such simulations, a substantial volume of the book is also given over to recent researches on numerical methods directed towards fluid, kinetic, and hybrid computations, and on the visualization of the often staggeringly large data sets generated thereby. Anyone wishing to gain a vivid snapshot of current activity in these areas could usefully browse this volume. — STAN COWLEY.

Deep Sky Video Astronomy, by S. Massey & S. Quirk (Springer, Heidelberg), 2009. Pp. 201, 23·5 × 15·5 cm. Price £22·99/$34·95/€34·95 (paperback; ISBN 978 0 387 87611 5).

Professional astronomers who read this review might believe that they do not need this book, but if they are involved in either education or public outreach then there is much in it which they will find useful. The enthusiasm and competence of the authors is obvious on every page and as an 'how to do it' book it is exemplary. With a few exceptions near the end of the book, the authors deal exclusively with the use of CCTV and video cameras to obtain images of deep-sky objects. They do not discuss obtaining high-resolution planetary images.

They deal thoroughly with all aspects of choosing both the astronomical and electronic hardware and the software. They compare the advantages and disadvantages of various makes of camera, from simple video cameras to frame-integrating cameras which can store many hundreds of images within the camera itself and then update the display every integration interval. Many examples are given of small telescopes producing near-real-time images of stars down to near eighteenth magnitude, hence the applicability to public outreach. For those who wish to produce a stored final image they go through the whole image capture, sharpening, and processing routines of several software packages and every step is liberally illustrated with many images. When it is considered that many of the cameras considered cost a few hundred, not thousands of, pounds, that much of the software is free, and that generally small amateur telescopes have been used, then some of the results are remarkable. The clear resolving of Sirius B from Sirius A with a 20″ *f*/5 telescope or the equally clear resolution of Phobos and Deimos from Mars are just two examples. Their selection of both monochrome and colour examples of images obtained with this technology shows with aplomb just what can be obtained, and the images compare favourably with those obtained with conventional CCD cameras with cooling and long integration times.

Towards the end of the book the authors give examples of scientifically useful measurements which can be undertaken with this technology. These include occultation timing, astrometry, supernovae searches, finding meteor radiants, and even collimating the telescope.

This reviewer found only one error and that is a relatively trivial typo for the time in the top right-hand image in Fig. 7·2. I can thoroughly recommend this book. — E. NORMAN WALKER.

Legends of the Stars, by Patrick Moore (The History Press, Stroud), 2009. Pp. 185, 22·5 × 14 cm. Price £12·99 (hardbound; ISBN 978 0 7524 4902 9).

Everyone likes a good story, and few stories have proved as enduring as the ancient Greek myths. Many of the best-loved characters from these ancient tales of daring, romance, egotism, and treachery are illustrated in the sky in the form of constellations. Take Orion for starters. A great hunter, he boasted he was superior even to Diana, the goddess of hunting. But he failed to see a venomous scorpion crawling from the ground behind him, which stung him on his bare heel — hubris brought low by a mere insect. Jupiter elevated Orion to the sky, placing his nemesis the scorpion on the opposite side of the celestial sphere where it can harm him no more.

Then there is the epic tale of Perseus, sent to bring back the head of Medusa, the Gorgon with the literally petrifying gaze. Divine assistance helped him succeed in this apparent suicide mission. As an encore, on his way back with the monster's head, he even found time to rescue the unfortunate Andromeda from certain death in the jaws of a sea monster. In the sky, the head of the ghoulish Gorgon is marked by the star Algol but this name comes from the myth, not because of any ancient knowledge of the star's variability.

Another hero who achieved superhuman success was Hercules, set a string of seemingly impossible tasks by a despotic king. Hercules himself is not a prominent constellation, strangely for such a superman, and only a handful of his labours are commemorated in the sky, most noticeably the lion which he killed with his bare hands and skinned to make a cloak.

Also among the stars we see the dolphin that saved the musician Arion from drowning; beautiful Callisto changed into a bear by Jupiter's jealous wife; and the most epic tale of all, the voyage of the Argo, a once-magnificent ship that now lies dismembered in southern skies.

In this reprinting of a book from 1964, Patrick Moore relates these celestial stories for children with the charm and grace of a natural story-teller — an ideal way to introduce youngsters to the sky, and a fitting reminder in this International Year of Astronomy of our subject's immense cultural heritage. — IAN RIDPATH.

Tours du Monde. Tours du Ciel. Une exploration de l'univers à travers les âges, film by Robert Pansard-Besson (Arkab Productions & EDP Sciences, Paris), 2009. Set of 4 DVDs + booklet of 274 pp. Price €49 (about £42) (ISBN 978 2 7598 0357 6).

During 1987–1990, Robert Pansard-Besson produced top-quality scientific television programmes telling of our progressive understanding of the Universe over the centuries (up until the 1980s). Numerous sites around the world were filmed; astronomers and physicists were interviewed, as well as engineers, ethnologists, Egyptologists, archaeologists, historians of science, *etc*.; highlighting commentaries were added throughout the films, especially *via* dialogues between astrophysicist Pierre Léna and philosopher Michel Serres — all this with a background of original music by Georges Delerue. The series was awarded the 1990 Jean Perrin Prize. This recently issued set of four DVDs is a 're-mastered' version of Pansard-Besson's original films, presented in ten segments each of 52 minutes. The package comes with a booklet including the main dialogues and an extensive glossary (of about 40 pages). Because of their historical contents, the films are relevant and their viewing is warmly recommended to anyone wishing to put into perspective today's astronomical investigations. The interviews are in their original languages, many of them in English, subtitled in French. An option for hearing-impaired viewers enables the display of French subtitles for the entire films. Given their intrinsic high value, subtitles ought, however, to be also provided in other languages. — A. HECK.

THESIS ABSTRACTS

RELATIVISTIC MAGNETOHYDRODYNAMICS

By Konstantinos Nektarios Gourgouliatos

Many, yet unresolved, questions in astrophysics are associated with the presence of electromagnetic fields. These include problems of solar magnetohydrodynamics, where a significant amount of observational data is available, but also relativistic systems involving the launch of jets from γ-ray bursts. Motivated by these phenomena, we study a series of problems of magnetohydrodynamics. Our task is to study systems where analytical solutions are possible so that we obtain an understanding of their physical behaviour.

For this purpose, we study separable solutions of force-free magnetic fields and we then apply them in systems of arcade topology. This problem is motivated by the arcade structures observed on the surface of the Sun before coronal mass ejections. We assume a magnetic arcade, emerging from the surface of a spherical conductor where the magnetic field is radially self-similar. Then, because of differential rotation on the surface of the conductor, the field lines are twisted and energy is injected into the system. Assuming force-free magnetic fields, the system reacts in two ways: a toroidal component is introduced and the poloidal flux expands. No matter how slow the rate of differential rotation is, the predicted expansion velocity becomes very rapid at late stages. This is the limitation of the non-relativistic magnetohydrodynamics approximation.

The rest of this thesis is about problems of relativistic magnetohydrodynamics. We present the analogue of force-free magnetic fields for systems of spherical geometry, first derived by Prendergast[1], and we expand it to systems of cylindrical geometry. We derive analytical and semi-analytical solutions for electromagnetic fields emerging from a central explosion in vacuum and in the presence of a co-expanding fluid. The mathematical description of this problem leads to a set of non-linear partial differential equations. As it is impossible to find general solutions for this set of equations, we assume self-similar solutions. We discuss applications of these explosions to γ-ray bursts. — *University of Cambridge; accepted 2009 May.*

Reference

(1) K. H. Prendergast, *MNRAS*, **359**, 725, 2005.

Galaxy Formation and Evolution Using the Virtual Observatory

By Paresh Prema

Galaxy formation and evolution is a long-standing problem in astronomy. The current models to describe this are monolithic or hierarchical. The monolithic approach describes today's elliptical galaxies as having their most vigorous episodes of star formation at high redshifts of $z > 1$. The hierarchical model postulates that the large elliptical galaxies today are a result of galaxy mergers. There are two issues here: firstly, how large elliptical galaxies formed into the sizes seen today; secondly, how this is correlated with the star-formation history. Both models are still debated today, with current theory through observations and cosmological simulations favouring the hierarchical approach.

Observational astronomy has entered a new era of large, multi-wavelength data sets that can probe far into the cosmos. This is increasing our understanding of galaxy formation and evolution through larger samples of objects, but more importantly across the electromagnetic spectrum. A major factor has been the advances in detector technology for observations, and computing power for cosmological simulations. This is causing a data avalanche in astronomy. The virtual observatory (VO) is providing a means to make the data easily accessible through VO-compliant services. The VO concept is to provide interoperability standards between VO projects around the world, as well as tools and applications to process data, making data access easier for any user. In this thesis we look to exploit VO technology to fit model spectral-energy distributions (SEDs) to observational photometric data of galaxies at high redshift ($z \sim 3$). A thorough look at how such a technique would be implemented in a VO environment is studied.

The VO, however, is a project in early development, though many VO projects now have access to many different data sets, old and new. In its current form, it is still far from what an average research astronomer might use. The tools and applications that are presently available are not yet at a stage that can be used to do research at the highest level. Despite these current issues, one major goal has been achieved by the VO: the easy access to data. Also, development of standards is essential for producing a system that would widely be used by astronomers. Thus, many technological challenges still exist which need to be addressed before complex scientific procedures (or workflows in the context of the VO) in astronomy can be used in the VO environment.

The technique of fitting SED models to photometric observations of galaxies is now well established from $z \sim 2 - 6$. The SED-fitting technique is applied to samples of Lyman-break galaxies (LBGs). LBGs are found in abundance through the colour–colour selection technique, and with the large data sets available now, larger samples can be studied. Photometric measurements only provide limited information about the properties of galaxies, but through the fitting of SED models, physical parameters such as stellar masses, ages, and star-formation rates (SFR) can be estimated. The estimates of the physical properties of these galaxies lead to constraints on galaxy formation and evolution. Two samples of LBGs have been studied here that have enabled the usefulness of the technique to be assessed. The results were shown to be consistent with similar work in the area, with median values for age of ~ 200 Myr, for stellar mass of $\sim 10^{10}\ M_{\odot}$, for SFRs of a few $M_{\odot}\ \mathrm{yr}^{-1}$, and $E(B-V) \sim 0{\cdot}15$. However, the results are very much dependent upon the sample brightness. — *University of Cambridge; accepted 2009 July*.

A full copy of this thesis can be requested from pareshprema@gmail.com

Likelihood Analysis of the Cosmic Microwave Background

By Samira Hamimeche

Cosmic microwave background (CMB) temperature and polarization power spectra are analysed using likelihood techniques. An accurate likelihood analysis allows us to constrain the cosmological parameters reliably from observational data. Calculating the likelihood exactly on the full sky is in principle very straightforward. However, from partial sky data, the calculations become computationally prohibitive. With the improvements in the amount and quality of data that the next generation of experiments will provide, an accurate and fast likelihood analysis is very crucial. Moreover, the temperature and polarization fields are correlated, partial sky coverage correlates power-spectrum estimators at different l, and the likelihood function for a theoretical spectrum given a set of observed estimators is non-Gaussian. Therefore, an accurate analysis must account for all these properties when modelling the likelihood function. Most existing likelihood approximations are only suitable for a temperature-only analysis, and cannot reliably handle temperature–polarization correlations.

In the first half of this thesis, we derive a lower limit to the accuracy required to obtain unbiassed parameters. We then test the existing likelihood approximations in their full-sky form, and show that some approximations outperform their other counterparts. We propose a new general approximation applicable to correlated Gaussian fields observed on part of the sky. This approximation models the non-Gaussian form exactly in the ideal full-sky limit, and is fast to evaluate using a pre-computed covariance matrix and a set of power-spectrum estimators. In fact, this is the first approximation that successfully models the polarized likelihood function consistently. We first perform intensive comparisons between the new approximation, a fiducial Gaussian (a Gaussian distribution with a fixed fiducial covariance), and existing approximations with partial-sky simulations and isotropic noise. We then test the approximations on simulations with realistically anisotropic noise and asymmetric foreground mask. The results demonstrate that the new approximation is suitable for obtaining significantly accurate results at $l \geq 30$ where an exact calculation becomes impossible. They also show that some Gaussian approximations give reliable parameter constraints even though they do not capture the right shape of the likelihood function at each l.

In the second half, we investigate the optimality of hybrid pseudo-C_l CMB power-spectrum estimators. These estimators are a combination of pseudo-C_l with different weighting functions. For simple cases with azimuthal symmetry, we compare the inverse variance of these estimators with optimal results (the Fisher errors), and show that the loss of information is neither negligible nor is it enough to have a large effect on parameter constraints.

Finally, we assess the number of samples required to estimate the covariance from simulations, with and without a good analytic approximation. In particular, we investigate the usefulness and efficiency of the shrinkage technique, a weighted combination of an approximate analytic model and simulated samples. We conclude that shrinkage might be useful if applied to separate subsets of the covariance but require far more simulations compared to a model-fitting approach, such as the *WMAP* method. — *University of Cambridge; accepted 2009 July.*

OBITUARY

Tao Kiang (1929–2009)

Born Jiang Tao in Yangzhou, China, 1929 February 6, he was the eldest son of Jiang Zhen Guang, an artist and calligrapher, and his wife, Wang Xin Ru, a teacher. As the eldest son he was sent to Europe at the age of 15 to escape the conflict between China and Japan. He was unable to visit China again until 1964, a trip that brought joy to him and his family. Tao was appointed as an assistant at the University of London Observatory. After studies he obtained the B.Sc. and Ph.D. degrees and was appointed a Lecturer in Astronomy at University College London. He moved to the Dublin Institute for Advanced Studies in 1966.

His Chinese background enabled him to read the ancient Chinese annals. He re-examined the records of apparitions of Halley's comet, and improved the interpretation of the records when he could. Finally he recomputed the orbit with perturbations over the last 28 revolutions. His scientific interests also included statistical work. He studied the luminosity function of galaxies and the spatial distribution of clusters of galaxies in large groups of clusters. He was interested in statistical problems connected with asteroids. He was a prolific publisher, and minor papers included one that summarized advances in the study by others of the ancient Chinese records during two decades, and another that described the ancient Chinese method of deriving the volume of a sphere.

The publication *Acta Astronomica Sinica* had started in 1953 and had resumed after an interlude. A translation with Tao as translator was started with Volume 1, Number 1 for 1977. *Acta Astrophysica Sinica* started in 1981. From that date Tao translated both journals and the translation became *Chinese Astronomy and Astrophysics*. He continued as translator for the rest of his life. He also became an important member of the Chinese community in Dublin and his visits to China became more frequent as the years went by. In 1975, he co-founded the Irish–Chinese Cultural Society, which is dedicated to strengthening ties between the two countries.

Tao retired in 1993 although he subsequently maintained a busy schedule. He died on 2009 March 26. His wife Trudi, son Ingmar, and daughters Sophie, Tanya, Jessica, and Rosalind survive him. — ROY GARSTANG.

Here and There

A COMMON-USER FACILITY?

A search for multi-planet systems using the Hobby-Eberly telescope — *ApJ*, **182**, 2009 May, front cover.

WOBBLY SCIENCE

… the radial velocity method, which looks for the wobble induced on a star by a planet as it rotates on its axis, like a spinning top. — *Astronomy Now*, 2009 July, p. 13.

SLIP OF THE TONNE

The three-stage Saturn V rocket, carrying a million tonnes of fuel … — *Astronomy Now*, 2009 July, p. 24.