THE OBSERVATORY

Vol. 133 2013 JUNE No. 1234

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2012 November 9 at $16^h 00^m$
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. Ladies and Gentlemen, I'm very happy to welcome you to the monthly Astronomy and Geophysics open meeting of the Royal Astronomical Society. I would like to remind you that the National Astronomy Meeting (NAM) in the year to come — which will also incorporate the MIST and UKSP annual meetings — will be held at the University of St Andrews, from 2013 July 1–5. Normally we've held the NAM in April, around Easter, but this year it's in July — and it will be spring in St. Andrews then! [Laughter.]

I'd like to pass to the programme of the meeting, and the first talk today is by Dr. Barbara Becker, from the University of California, Irvine: 'Unravelling starlight: William Huggins and the rise of the New Astronomy'.

Dr. Barbara Becker. One hundred and fifty years ago, on 1862 November 14, the Royal Astronomical Society convened its first meeting of the new season. According to the *Astronomical Register*, the "Meeting was very numerously attended, the rooms in fact being inconveniently crowded".

One of the items on the agenda was a paper titled 'Stellar Spectrum' read by a Mr. Huggins, who, in his eight years as a Fellow, had yet to make his mark or take on a leadership role in the Society. Unfortunately, the *Register* neither commented on his paper's content, nor recorded any discussion. Did the high attendance expose more people to news of his pioneering study, or did the crowded conditions simply make it harder to hear what had been said?

Although it is doubtful that any of those present suspected it — even Huggins himself — we now know that they were witnessing a watershed moment in the history of astronomy as profound as Galileo's turning a spyglass on the heavens. For after 1862, astronomers neither would, nor could, ever look at — or understand — the denizens of the celestial realm in the same way again. It is my great pleasure to join with all of you in commemorating the sesquicentennial of this transformational event and in celebrating the role of William Huggins in it. It was here in the RAS library that I began my research on the life and career of William Huggins over twenty years ago with the help and encouragement of Peter Hingley. I am sorry that Peter cannot be with us tonight, but I know he is here in spirit.

Today, the practice of astronomy is built on queries and methods that were unimaginable 150 years ago. Back then, positive knowledge of the physical and chemical structure of celestial bodies was presumed to be unattainable. One could entertain any number of untestable ideas about the origins of stars, or the reasons for their differences in colour and brightness, but such was not the stuff of science. All that could be known with certainty was the location of a celestial body on the sky. Gathering and interpreting this information defined the mission of the practising astronomer and determined the structure of his creative thought and work space. Everything else was relegated to the no-man's land of mere speculation.

Introducing the spectroscope into the astronomer's toolkit changed all that. In the mid-19th Century, the spectroscope was a staple in the laboratories of two groups of physical scientists: one analyzed the chemical properties of terrestrial materials; the other investigated the physical properties of light. William Huggins played a key role in adapting this chemical and physical instrument to astronomical purposes. In less than two decades, the so-called 'star-spectroscope' made possible the emergence of a 'new' astronomy.

Who was this man? What moved him to pursue such a risky and challenging venture? What prompted astronomers to follow his lead? The name of William Huggins may be familiar to few outside the confines of this Society. Those who think they recognize it often have him confused with the 17th-Century Dutch astronomer, Christiaan Huyghens. Our man Huggins was a self-taught amateur astronomer who was celebrated in his own lifetime as the father of astrophysics. Huggins went on to serve the RAS as a member of its Council, as its Secretary, Foreign Secretary, Vice-President, and President. He is one of only a handful of individuals selected twice to receive the Society's prized Gold Medal.

He gained renown as the first to observe emission lines in the spectra of nebulae, to analyze the spectrum of a nova, to attempt to measure the heat of stars, and to apply Doppler's principle to a star's light in order to determine its motion along the line of sight. These and his other scientific contributions earned him many awards and honours over his long lifetime.

In 1875, he married Margaret Lindsay Murray. Together they collaborated on many ground-breaking projects, always searching for new and untried applications for spectroscopy.

According to Huggins' own testimony, he underwent a dramatic change in his research interests and methods in the early 1860s. Feeling "a little dissatisfied with the routine character of ordinary astronomical work", he began to look for some new way to study the heavens. Hearing the news of "Kirchhoff's great discovery of the true nature and the chemical constitution of the sun" was, in his words, "like the coming upon a spring of water in a dry and thirsty land."

Huggins could describe the events that influenced his early career choices in such clear-cut terms because he was writing about them in 1897, some thirty-five years after the fact, in an essay titled *The New Astronomy*, which appeared in a popular magazine of the day. His captivating narrative carried the reader behind the scenes of scientific discovery in a very personal and dramatic way. After Huggins' death in 1910, this essay became the reference of choice for obituarists and biographers. His widow's vigilant watch over the content of everything written about his life and work may well have pushed authors to adhere strictly to Huggins' own published words. In time, *The New Astronomy* became something of an authoritative account of his role in the origins of astronomical physics. It has been invigorating for modern astrophysicists to trace their professional roots to a pioneer of such vision and daring — someone obviously on the inside track who was able correctly to assess the state of affairs and act to rectify things.

But the image of a sudden leap forward with no turning back that Huggins so vividly conveyed in this particular passage does not mesh with the patchwork of observations that he recorded in his observatory notebooks between 1860 and 1865. A careful examination of those and other contemporaneous documents reveals that Huggins' transformation was in reality a complex and gradual process.

William Huggins was born in London in 1824. He lived with his parents above the family's silk shop in Gracechurch Street. When his father's health began to fail, William dutifully assumed responsibility for running the thriving business. But selling cloth was not the only thing that occupied his time and attention. Despite a lack of formal scientific education or training, the young draper was busy exploring the unseen world made visible by optical instruments.

In 1852, Huggins became a fellow of the Royal Microscopical Society. A year later he purchased a 5-inch telescope. And in 1854, shortly after his thirtieth birthday, he was elected to fellowship in the Royal Astronomical Society, then a diverse group comprised in large part of amateurs like himself. Some timed celestial events and charted the heavens while others searched for new asteroids and planetary satellites, observed double stars, noted changes on planetary surfaces, counted and mapped sunspots, and documented coincident solar and terrestrial phenomena. It was a rich and varied observational agenda to which Huggins wished to contribute.

Living in the City — its sky frequently obscured and its horizon always obstructed — made astronomical study a challenging pursuit for even the most diligent observer. And so Huggins retired from commercial life and relocated to Tulse Hill — a well-heeled and growing suburb south of the Thames. These changes afforded him both the leisure time and the darkened skies necessary to pursue his growing interest in observing the heavens. He had an observatory built to house his instruments. In 1856, he began recording his observations in a bound notebook, cryptic jottings that, along with his occasional contributions to the Society's *Monthly Notices*, hint at the range of his celestial interests and his sophistication in pursuing them. His early notebook entries are those of a novice satisfied to observe whatever the heavens might display.

Like all new enthusiasts, Huggins needed expert guidance and encouragement in order to transcend his observational opportunism. Doubtless the RAS served him well in this regard. But in 1858, he attended his first meeting of the British Association for the Advancement of Science. While there, he came into contact with a wider circle of expert amateurs, most notably the Reverend William Rutter Dawes of Haddenham. Dawes sold his young friend an 8-inch object glass made by Alvan Clark of Cambridge, Massachusetts, which Huggins had mounted in an equatorial, clock-driven telescope built by Thomas Cooke of York. Dawes was a mentor and a model from whom Huggins acquired a lasting sense of purpose and commitment in his observational programme. As he metamorphosed from curious dilettante to confident, self-directed observer, he internalized the serious amateur's obligation to go beyond satisfying personal curiosity and contribute to the larger body of astronomical knowledge.

Meanwhile, on 1859 October 20, physicist Gustav Kirchhoff presented a paper to the Berlin Academy. In it he recounted his recent collaboration with chemist

Robert Bunsen to investigate Fraunhofer's lines, the perplexing dark lines that interrupt the otherwise continuous solar spectrum. Kirchhoff's observations led him to conclude that the dark lines "exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place."

Though it would be over two years before Huggins would hear this news, it quickly reached Henry Roscoe, Professor of Chemistry at Owens College in Manchester, a former student and collaborator of Bunsen's. Roscoe promptly alerted his colleagues about "Kirchoff's [*sic*] discovery". Three weeks later, he wrote: "I hear from Bunsen that he has ... mixed [some] salts together, put ... the mixture on the point of a pin — looked through a telescope & saw at one glance all the substances present! This," Roscoe exulted, "is something like Qualitative Analysis!"

In 1861 March, he gave a talk at the Royal Institution on recent developments in spectrum analysis. In June, his address to the Chemical Society prompted Warren De La Rue to proclaim: "[I]f we were to go to the sun, and to bring away some portions of it and analyze them in our laboratories, we could not examine them more accurately than we can by this new mode of spectrum analysis."

What really excited De La Rue, an amateur astronomer himself, was the potential this method portended for astronomy. He envisioned an emerging era of cooperation among physical scientists linked by the spectroscope. "It is not an uncommon thing for the physicist to tread upon the ground which a chemist thinks belongs to him, and for the chemist to tread upon the ground of the physicist", he declared. "Now, we have the chemist occupying the ground of the astronomer, and if the astronomer wants to know something of the constituents of the heavenly bodies, he must come to the chemist".

Months later at that year's British Association meeting, William Allen Miller, Chair of Chemistry at King's College, London, and a veteran spectroscopist, delivered a richly-illustrated evening lecture on 'The new method of spectrum analysis'. He credited Josef von Fraunhofer with establishing the 'what' and the 'how' of prismatic analysis nearly a half-century earlier and noted the German optician's suggestion that this method could be used to analyze the light of planets and fixed stars. But Miller was unwilling to jump to such conclusions himself. He acknowledged that Kirchhoff's recent interpretation of spectral patterns could provide a "further glimpse into the machinery of the universe", but he cautioned his audience, "it appears by some to have been hastily assumed, in a spirit of self-confidence, that we already have the key to everything upon this subject". Miller was sure that future research would ultimately teach investigators a "lesson of reverent humility".

He repeated this talk at a soirée of the Pharmaceutical Society of London on 1862 January 15, an event that stood out vividly in William Huggins' memory as a defining moment in his own career. Indeed, Huggins recalled being "seized" by a "sudden impulse" to invite Miller "to join" with him in attempting "to apply Kirchhoff's methods to the stars".

The two men lived across the street from one another. It is easy to imagine that their subsequent collaboration grew out of a conversation they shared during a carriage ride home that evening.

Huggins recalled that Miller agreed to come to work in his observatory "on the first fine evening" after the soirée. But it is difficult to date with certainty the onset of their collaboration. Miller was preoccupied at the time with his own research problems. And Huggins did not record any spectroscopic work with

Miller in 1862. Indeed, he recorded nothing at all in his observatory notebook between 1860 July 18 and 1862 March 31, a gap which is both perplexing and disappointing.

When he resumed his notebook entries, it was not to record spectroscopic observations, but to supply descriptions and drawings of Saturn. His fascination with Saturn is easy to understand. For one thing, the planet was favourably placed for viewing. But Saturn's real appeal to amateurs like Huggins stemmed from the fact that its ordinarily prominent ring system could then be seen nearly edge-on by earthbound observers. With Saturn's rings reduced to near invisibility, astronomers of all stripes — Huggins included — hoped to be rewarded with the discovery of a new satellite.

Our earliest and only contemporaneous hint that he had been doing more than observing Saturn during this period is the *Astronomical Register*'s note on the papers read at the RAS's inconveniently crowded meeting on 1862 November 14. More evidence soon followed as Huggins and Miller became pressed by priority concerns to publish their findings. In 1863 January, the translation of a paper on stellar spectra by Giovanni Battista Donati appeared in the *Monthly Notices*. In February, Miller and Huggins received word that similar research was being pursued in America by Lewis Morris Rutherfurd. Father Angelo Secchi at the Collegio Romano began his own systematic examination of stellar spectra soon afterward. It was a rapidly developing field.

To distinguish their work from that of their competitors, the two collaborators emphasized that they had examined the spectra of thirty to forty stars as well as those of Mars, Jupiter, and the Moon. They submitted schematic spectral maps of three stars — Aldebaran, Sirius, and Betelgeuse — compared to that of the Sun.

A little over a year later, they announced that they had extended their initial survey to include the spectra of some fifty stars, and they produced detailed maps of the spectra of Aldebaran and Betelgeuse (as can be seen in an original photographic print from the collection of the late Gerald Whitrow). If Huggins had let the matter rest there, that might have been the end of his role in our story. But of all the early celestial spectroscopists, it was William Huggins who did the most to build and shape the emerging disciplinary boundaries of astrophysics. As a recently retired entrepreneur, he thrived in the diverse community of late-19th-Century amateur astronomers. Free to select the objects he wished to observe, he happily pursued an eclectic and opportunistic observing programme throughout his career.

But in *The New Astronomy* Huggins portrayed his spectroscopic discoveries as originating in flashes of insight and developing one upon the other in a linear and logical way. New research questions flowed naturally and inevitably out of his unrelenting and skilful application of this chemical and physical instrument to old and seemingly intransigent astronomical problems. He depicted astronomy's traditional methods as being not so much obsolete as tired. And he painted himself as less a revolutionary than a cautious, yet visionary, shepherd guiding his colleagues toward greener research pastures.

Today, it is tempting to place this essay in a different category from his other written work: to see it as truer for its candour, as more accurate for its detail, as closer to the way things actually happened than any of his formal scientific papers. But it is not a deposition. It is a synthetic account composed of specially selected events recalled many years after the fact.

William Huggins was not a disinterested observer of those events. He was an active participant in them with much to gain from characterizing for posterity his own career development as synchronous and symbiotic with the wider growth of interest in subjecting the light of celestial bodies to prismatic analysis.

Reading *The New Astronomy* recalls the heroic tale of Theseus in the labyrinth, who, once he had solved that fearsome puzzle, easily escaped by retracing the winding path of the string he had laid on his way in. Over the course of his long career, Huggins, like Theseus, encountered obstacles, sought alternative routes, ventured educated guesses, made mistakes and back tracked. He took calculated risks and sometimes met up with dead ends, but he simply kept at it again and again and again. In *The New Astronomy*, the multitude of choices he faced at every crossroads no longer clutter the way. The problems he mentions there are the ones whose solutions mattered, and those for which satisfactory solutions were found. Projects that, in his view, had either failed or did not fit his narrative of discovery are omitted or simply dismissed as forgettable wastes of time. Eventually, only the challenges he found interesting or consequential remained. Many of those have faded from our collective memory in the retelling.

To restore the lost complexity and noisy confusion that energized the bustling and diverse scientific community of which Huggins was a part and which made possible the formation of a new species of scientific investigation requires looking well beyond Huggins' published version of events to locate less-filtered accounts available in contemporaneous records.

The Tulse Hill observatory notebooks open a unique, though imperfect window on the Hugginses' research efforts. They are preserved at Wellesley College, a private women's school near Boston, Massachusetts. Given to Wellesley by Margaret Huggins shortly before her death, those notebooks put meat on the bones of Huggins' published accounts of his research. They bring to light his considerable skill at manipulating temperamental instruments, the wide range of his observing interests, and his ability to transform tentative observational notes into confident published reports.

They also reveal for the first time the breadth and depth of Margaret's important contributions to the work of the Tulse Hill observatory. In published accounts, she successfully cloaked herself in the invisible garb of the proper Victorian lady, taking care that her collaborative assistance did not contradict or interfere with the image she had helped to create of her husband as the innovator and principal observer in the team. In contrast to that image, her entries in the notebooks make it clear that her presence and expertise not only strengthened, but also shaped the research agenda at Tulse Hill. From them we learn that she figured prominently in introducing photography into their methodological toolkit, in designing and modifying instruments and procedures, in selecting subjects for examination, and in communicating the results of their joint efforts in scientific journals.

The notebook record is supplemented by Huggins' extensive personal correspondence. It is in his letters, not in his published papers, that Huggins aired his methodological concerns, his reactions to controversy, and his anxieties about the accuracy of his measurements. They identify individuals like Thomas Romney Robinson, George Stokes, and David Gill whose advice and counsel he valued at various stages of his long career. To make these valuable documents more easily accessible to scholars, I am currently annotating my transcriptions of the letters I have collected over the past two decades. When completed in 2014 January, the work will be published in two volumes by Pickering & Chatto.

When evidence from Huggins' notebooks and correspondence is mapped onto the chronological grid of his published papers, we can see that his rise to prominence as a serious amateur astronomer was the result of a conscious career strategy pursued on a number of fronts by a bright and ambitious man. He may have been a risk-taker, but in keeping with his entrepreneurial background, his risks were always calculated to maximize success. He could not tame his research style to fit the selfless and disinterested stereotype of a man of science in the late-19th Century, but he could and did construct a more conforming public account of himself and his work.

I would argue that Huggins' greatest asset was his inclination, born of his entrepreneurial background, to treat each and every innovation and discovery as a commodity to be packaged and sold like an exotic bolt of cloth. He treated his colleagues in the scientific community like discriminating and sophisticated clientele, making every effort to instill in each individual a desire — even a need — to 'buy' and use his new-fangled methods. He executed the moves of this delicate dance with surefooted ease.

Although he was not always successful in achieving the aims of his research projects, he had a knack for diminishing the negative impact of his failures. He worked hard to control the public account of his work at every step. He personally corrected or retracted statements he later deemed to be erroneous rather than leave them for others to discover. He acted quickly to point out and rectify others' mistaken interpretations of his work. Indeed, he made certain that whenever others discussed his work, they did so in conformity with his own account of it.

Like any savvy businessman, he remained alert to threats from competitors. As others were attracted to the new field of astronomical spectroscopy, he took steps to preserve his place in the vanguard of this rapidly evolving research niche. He vigorously protected his claims on credit and priority for discoveries. He established strong alliances with influential astronomers, particularly Americans like Edward Holden, James Keeler, and George Ellery Hale, who had access to the world's newest and largest telescopes. He relied upon them to verify and support his observational and interpretational claims.

One hundred and fifty years ago, William Huggins introduced astronomers to the spectroscope, an analytical tool of laboratory science enabling them, at long last, to "unravel starlight", as he so aptly phrased it. Over the course of his long career, he showed his colleagues — professionals and amateurs alike — that deciphering the coded messages from the light of celestial bodies could and would provide access to certain knowledge of their chemical constituents and physical structure.

Paradoxically, the lack of clear explanatory guidelines in the fledgling science proved to be a help rather than a hindrance allowing a wide range of interpretive schemes to exist simultaneously and to interact fruitfully. The productive interdependence of laboratory and field observations provoked and sustained a level of discussion, comparison, criticism, and controversy necessary to develop confidence in the power of spectrum analysis to interpret the light of celestial bodies.

By the dawn of the 20th Century, astronomers found the questions they could ask about the bodies they observed and the methods deemed appropriate to examine them had changed in ways their predecessors could not have imagined.

Today we commemorate the pioneering efforts of William Huggins, who participated with vigour and vision in the rise of this new astronomy. And we celebrate the risky choices he made as he moved from the periphery of scientific London toward its inner circle, choices that expose the dynamic and often uncertain process by which the boundaries of acceptable research in the science of astronomy were redefined during his lifetime.

The President. Thank you. Questions, comments?

Professor D. Lynden-Bell. Was Huggins the first to use the Doppler effect in stars?

Dr. Becker. Yes, he was, in 1868; and he did it visually, if you can imagine that. He didn't begin using photography until after he was married, in 1875. And so his observations were all at the telescope, looking at a comparison spectrum using a battery set and sometimes a gas-filled tube, sometimes just a sparking mechanism, and he was looking at the hydrogen line in Sirius. He didn't get very good results! [Laughter.]

A Fellow. How did he support his research financially?

Dr. Becker. Ah, good point — he married well! [Laughter.] I gather that after he retired from the silk business, they were property owners. He complains in various letters, when he is seeking grants from the Royal Society, for instance, that rents have been low lately, and he needs a little money to finance his work. But Margaret had the money.

A Fellow. Where did you find that glass window?

Dr Becker. Oh, the glass window! This picture was taken at the place where it is housed, at Wellesley College. The Hugginses had designed this window, and it was in their house on Tulse Hill. Before she died, Margaret packed up three stained-glass windows, including this one, and sent them to Wellesley along with the notebooks, and a whole slough of other things, and she wanted them to be an inspiration for the young women. Wellesley is a women's college, and she was impressed that they had an observatory, and the Director of that observatory had actually visited William and Margaret Huggins in London, to ask their advice on what sorts of things they should include in their instructional programme. After William died, she had to pack up and clean the house because she was moving to a smaller apartment.

For years, until last year actually, this window was in a drawer in the observatory! It didn't inspire too many people at all. But when I first started my research on William Huggins, and I went to Wellesley College, the Director said, "you know, there is something else from the Hugginses that I thought people might be interested to see", and he opened up a drawer and pulled out this window, and I was just astonished! And I swore that, if I ever published anything, this would be on the cover of my book, and it is. But it's beautiful; if you ever go to Boston, go out to Wellesley and see it. It's unbelievably beautiful.

A Fellow. What is the STLL in the blue circle?

Dr. Becker. Well it's a part of an astronomical pair of windows. The first one shows the scene from the Bayeux Tapestry, where it says "*isti mirant stella*", and you see the little guys pointing up to Halley's comet; and on this one it says "*stella*" up at the top, and down at the bottom, the swirly thing is a nebula, a spiral nebula; so you wouldn't see a nebular spectrum, but they also show a nebular spectrum. And then there is the Fraunhofer spectrum of the Sun. And this little thing, that's kind of poking it's way out from behind the nebula, is Halley's comet, which actually did appear in Huggins' time.

The President. Thank you very much! [Applause.]

The next talk starts with a question some of us must have asked ourselves, from Professor Jay Melosh from Purdue University: 'Are we all Martians? Interplanetary exchange of living microbes in meteorites'.

Professor J. Melosh. [The speaker explained that he wanted to address the question of whether it was feasible that life could indeed have been transferred to the Earth from Mars. About 40 or so intact rocks from the surface of Mars, some ejected as recently as a million years ago, are known to have arrived undamaged on Earth. One of the best known is the Allan Hills meteorite, ALH84001, in which it has been claimed that there are signs of primitive life, including what appeared to be fossilized segmented organisms. The speaker concluded that the scientific community, for the most part, no longer believes that ALH84001 contains evidence for life on Mars since many key evidences have been challenged: for example, the 'bacteria' are too small to contain a single ribosome; the PAHs found have turned out to be terrestrial contaminants; and the tiny magnetite grains of supposedly biogenic origin have also been produced inorganically in the lab.

But ALH84001 is itself very interesting: it had its origin on Mars some four billion years ago — so it is older than any rock on Earth — and contains hydrothermal deposits; the presence of thermally fragile, magnetic grains in minerals indicates that the rock has never exceeded a temperature greater than 40° C in its entire history, including launch from Mars, transport, and entry through Earth's atmosphere. During the impact and ejection process, most material is raised to high pressures and temperatures, but a thin zone of material near the planetary surface — perhaps the most biologically active material — remains at zero pressure and receives a sufficiently large acceleration to escape Mars without damage. Orbiter images of Martian crater ejecta and deposits provide further supporting evidence for material being ejected intact at high velocities.

The geological study of Mars by rovers, including *Curiosity*, has led us to conclude that Mars was once warmer and wetter, not necessarily with oceans, but it did have extensive ice cover and contained conditions suitable for the origin of life perhaps at an earlier time than the Earth did — and certainly earlier in its history than the more hostile environment we find on Mars today. But for microbes to be transferred from Mars to Earth, they need to be able to survive such hazards as being ejected from the surface at high speed, exposure to solar UV radiation, cosmic rays, the vacuum of space, and low temperatures, and then entry through Earth's atmosphere and arrival on the surface.

The speaker described a variety of experiments to see if indeed dormant bacterial spores can survive such extreme conditions, and the bottom line is that they can — in early work, projectiles which had been inoculated with bacteria were fired from a pellet gun into plasticine to test the effects of rapid deceleration: some bacteria, such as *Deinococcus radiodurans*, actually appeared to thrive! Further experiments had been conducted using the *Ames Vertical Gun Range* to fire inoculated projectiles at high speed into vacuum: the particles were collected after impact and cultured, and it was found that significant populations of organisms had survived. This work suggests that microbes could indeed survive the strong acceleration and deceleration experienced by impact ejection from Mars, and arrival at Earth.

It can be estimated that of the material ejected from Mars, about 25% will be captured by Venus, and about 30% by Earth; similarly, Earth ejecta can reach Mars (though it is easier to transfer material from Mars to Earth), and Jupiter will expel from the Solar System significant amounts of Martian and terrestrial ejecta: it would appear that we are seeding the Galaxy with life. The ejected material is in transit for a long time, however — it can typically take 20–30 million years, for example, for ejecta to reach Venus from Mars. So whether spore-forming bacteria, which we think can survive thousands or millions of years, can really survive for tens of millions of years is a subject of debate amongst the biological community; but there are reports of careful research on the survival of bacteria in 25–40-million-year-old Dominican amber — somewhat reminiscent of *Jurassic Park*! — and even reports of salt-tolerant bacteria having been extracted and cultured from small droplets of water in salt crystals from a 250-million-year-old Permian salt bed in New Mexico; the organisms are closely related to extant bacteria, but they appear to have survived this long. The speaker also mentioned DNA-sequencing work to explore the mechanisms by which bacterial DNA can survive thermal degradation — normal DNA cannot last this long — and which also confer resistance to radiation damage and vacuum.

To survive long-exposure ages in space and high radiation doses, even such radiation-resistant bacteria as *Deinococcus radiodurans* still probably need to be protected within larger, at least metre-sized, rocks. And on delivery to Earth, meteorites get pretty hot at their surfaces but their interiors remain cool. In collaboration with Curt Mileikowsky, the speaker has conducted sounding-rocket tests to check the ability of spores to survive re-entry in rocks, and found that they are viable under such conditions. The speaker concluded that from researches in meteoritics, impact mechanics, microbiology, and extreme environments, it is overwhelmingly probable that viable microbes, if they ever existed on Mars, would be transferred from Mars to the Earth: and so it remains quite plausible that we really are all Martians.]

The President. Right — does anyone wish to dispute that? [Laughter.]

Dr. S. Jheeta. Thank you very much for the presentation — it was excellent, I loved it. I have no problem with the panspermia hypothesis *per se*, but the problem is always that you don't answer the question, the ultimate question of 'how did it get started?'.

Professor Melosh. Exactly.

Dr. Jheeta. So unless we can show some experiment being done in simulated space conditions — whereby a macro-molecule could be formed, and we can show some sort of progress towards that — how do we answer that question?

Professor Melosh. Oh, I couldn't agree more! This only puts off the ultimate problem of how life began. But it's a potentially testable hypothesis within our Solar System, and that lends extra interest to finding life and learning more about it on Mars. If it should happen that we find life on Mars, and we, for example, discovered it is using the same genetic code that life on Earth does — people think that the genetic code is probably contingent upon whatever happened — I think that would be strong evidence that life on Earth and on Mars are strongly related. And then we should be looking for life maybe not in vents in the deep sea but starting to think about what are the conditions on Mars if it really started there.

Professor I. A. Crawford. Just a follow-up: in a sense isn't the more serious problem that life isn't more obvious on Mars than it is? We know that there has been a lot of life on the Earth for a long time, and we know that Earth rocks will have transported life to Mars, so maybe there is life on Mars, but it is not staring us in the face. Is that a difficulty?

Professor Melosh. Fine, we don't really know ancient Mars at the same time; if we had visited Earth anytime before about a billion years ago, it would look an awful lot like ancient Mars — no plants, no visible sign of life, maybe a few stromatolites in the oceans, but that's it.

Professor Crawford. Well, except that the composition of the Earth's atmosphere has been changed.

Professor Melosh. Fine, it's been changed by photosynthetic organisms. There is some argument that there is methane on Mars, and maybe that it's biogenic — that remains to be seen. But I think it's a hypothesis that we should keep in mind when we're investigating Mars. And it also has some practical implications, since if we believe that life on Mars runs by the same genetic code, we have techniques that are orders of magnitude, maybe six orders of magnitude, more sensitive for finding Earth-like life, and traces of it, than to find something that we don't know anything about. So if you believe that Earth and Mars are related, we can devise techniques for actually detecting life on Mars that are much, much more sensitive than anything we're using now.

The President. I'm going to exercise my Chairman's right at this point, simply because we have to move on to the 2012 Gerald Whitrow Lecture, by Professor Andrew Liddle of the University of Sussex, on 'The Universe, Darkly'.

Professor A. Liddle. [It is expected that a summary of this talk will appear in *Astronomy & Geophysics*.]

The President. Questions?

Mr. A. Shukla. Earlier in your talk you've shown the graph of the standard cosmological model. There you show that presently, the dark energy is dominating. So does that symbolize the accelerated expansion of the Universe?

Professor Liddle. If you want to think of the transition to dark-energy domination and acceleration, that typically happens at about a redshift a bit less than one, when the Universe was a little bit more than half its present size. But it is maybe worth just noting from my subsequent plot that we actually don't know that much about what's happening right now, where I show all of the different models which are compatible with the existing data: you can see there is an enormous spread in possible predictions for what the dark sector is doing at the present day. And you might have thought that surely we should know what is going on today much better, but the trouble is there is not much volume to the local Universe, so there are not many measurements you can make very close, and most surveys have their statistical constraining power at relatively high redshifts at a half or one. It's not even certain, I think, that the Universe is still accelerating today — we're really saying it was accelerating at some point in the fairly recent past.

Dr. Becker. I have a question about coming to a conclusion on this matter, because we're in this phase like in early radioactivity studies where they really didn't have data, they didn't have theory, they didn't have really anything, they were moving around in the dark pretty much for a very long time. Can you conceive either an instrumental development or some sort of experimental situation that would give you some clear-cut answers that would help to open the door?

Professor Liddle. I think that is the hope. I should say, roughly speaking, the next decade will be a very good one for cosmology because of experiments already under construction. The *Dark Energy Survey*, which I am working on, has just begun operations; and at the end of the decade, the *Euclid* satellite will launch, and these are both designed to try and answer the questions I'm posing. What we don't know is whether the Universe is minded to let us answer them; it may not do so. But even in that case, I think, the experiments we have upcoming are good enough to say either we find something interesting or it's not that interesting to keep looking any harder — we've looked hard enough, we have not found something, we should accept it's just a cosmological constant, we don't know it's origin. The way I see the trend going is — no one here from a funding agency, I hope? [laughter] — that this should be the decade of theory in cosmology as well: the observers have had a lot of action over the last decade, and there is a lot of planned action in the next decade, but in fact observation in these areas of cosmology is way ahead of theoretical understanding. And once we get beyond the next phase I don't think we will learn more from having more data. We need more clever people working on theoretical models that can explain those data.

So I would like to see the pendulum drift back a bit from experiment to increasing the funding for theoretical exploitation and understanding. And I hope if I say it often enough someone will listen and do something.

Mr. M. F. Osmaston. My understanding is that dark energy, the acceleration, is inferred from applying the relativistic Doppler formula to a linear distance–redshift relationship. And that cuts down the distant velocities, because they must never exceed *c*, and leaves the near ones looking fast. But if the redshift is not a velocity, it's an invalid result. But if you say of course it's a velocity, may I point out that in 1968 an experiment was done by the US Naval Research Laboratory (see Sadeh *et al.*, *Science*, **161**, 567, 1968) which showed, using caesium clocks, that the redshift is a transition effect.

Professor Liddle. What I've discussed is a standard interpretation of the data. I think what you are saying refers specifically to data such as the supernova data, which is one part of the assembly of data that's brought to light on this. But of course what I've tried to stress in my talk is that there are significant problems of trying to find fundamental understandings of the data, and I said there should be more theoretical work.

The President. I'm going to draw a line there: we're out of time. I am going to thank our lecturer once more for the Gerald Whitrow Lecture. [Applause.] And remind you of the usual drinks reception in the library, around the corner at the RAS rooms immediately, starting now. Finally, I give notice that the next monthly open meeting of the Society will be on Friday the 14th of December.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2012 December 14 at $16^{h}\,00^{m}$
in the Geological Society Lecture Theatre, Burlington House

F. Honary, *Vice-President*
in the Chair

The Vice-President. Hello and welcome! As you can all see, our President David Southwood is not here at the moment. He is flying today from the USA, so he could be here at any minute, but in his absence I have the honour to welcome you all here for the Ordinary Meeting this afternoon. We'd like to start with announcements; you will all have heard of the death of Sir Patrick Moore, CBE, FRS, and Fellow of the Royal Astronomical Society. Patrick Moore was a former President of the BAA, but he was best known for his TV show *The Sky at Night*. He had been doing that for the last 55 years, and brought astronomy to everybody's home in the best possible way. The RAS has a Medal in his name, for a particularly noteworthy contribution to teaching astronomy or geophysics at secondary level, and at the Council meeting today there was a discussion about what other gestures the Society would like to make to commemorate his memory. I would like to ask everyone to stand up for one minute of silence, please. Thank you very much.

We have a very interesting programme this afternoon, with three excellent talks. The first speaker is Dr. Paul Spudis from the Lunar and Planetary Institute in Houston. The title of his talk is 'By the light of the watery Moon'.

Dr. P. Spudis. I want to give an overview about recent lunar discoveries and explain why the Moon continues to excite great interest.

The Moon is close — it appears as a world and not a point of light, but more importantly we can operate machines on its surface remotely from Earth with a minimum of time delay. It retains a record of the Earth–Moon history that is missing from Earth. Finally, and most importantly, it appears that the Moon has water — the most important factor for spaceflight, radiation shielding, energy storage and, of course, maintaining life.

Recent missions include the ESA *SMART-1* in 2003, the Japanese *Selene* craft, and the two recent Chinese missions *Chang'e I* and *II* which have been imaging the surface at several wavelengths. The Indians sent *Chandrayaan 1* in which I participated, working on the *Mini-SAR* radar-imaging experiment. Finally, the US sent the *Lunar Reconnaisance Orbiter* (*LRO*), which is still in operation, and the *Lunar Crater Observation and Sensing Satellite* (*LCROSS*) — an impactor experiment designed to check for the presence of ice at the lunar poles.

The poles are of special interest because there are a number of features which are in sunlight for more than 75% of the lunar day in the southern regions and which were first found by *Clementine* in 1994. At the North Pole, near the crater Peary, some features were thought to be in permanent sunlight, but we now know that the actual situation is more complicated. To understand the locations and durations of the sunlit areas we needed detailed topographic mapping. We now have recent data from the *Lunar Orbiter Laser Altimeter* (*LOLA*) carried by the *LRO*. The southern polar topography is more craggy and extreme than that of the corresponding region in the north. The South Pole itself is located just inside the rim of the largest impact crater on the Moon, the South-Pole–Aitken Basin. We can make maps showing the seasonal variation of sunlight near the South Pole — even in winter some small areas of high ground a few kilometres across stick up into sunlight. In the deep crater shadows, however, it was postulated that the temperature might plunge to 50 or 60 K, and when we actually went and measured those areas we found some parts were actually close to 25 K — colder than the surface of Pluto. If a water molecule gets into such a cold trap on the Moon, there is no known process that can remove it.

Where does the water come from? Probably from water-bearing meteorites, cometary cores, interplanetary dust particles, and also from atmospheric water molecules which have been caught up in the Earth's geo-tail and deposited on the Moon. We had previously assumed that this water would be lost to space. We have analyzed the volcanic glass beads from *Apollo 15* — this was primitive material which was squirted out into space from the lunar interior and which froze as little glass beads on falling back to the lunar surface. Inside these beads we found a number of volatile elements including water, and from this we can infer that the mantle contains water at the 250–700-parts-per-million level. The result suggests that 3·5 billion years ago the Moon had a significant supply of water in its interior. *Chandrayaan 1* looked at the areas from 65-degrees latitude to the poles and found that the strength of the 2·8-micron absorption band (monitoring OH and atmospheric H_2O) was strongly correlated with latitude.

It is possible that this water comes from the impact of solar-wind protons reducing the metallic oxides on the lunar surface and also by deposits from cometary and meteoritic debris. The mass spectrometer on *Chandrayaan 1* detected a hundred-fold increase in the amount of exospheric water whilst passing through 83-degrees latitude. The upper stage of *LCROSS* slammed into the Moon's crust and the resulting plume of ejecta was observed by a following satellite. We found methane, water, simple organic molecules, and carbon dioxide in Cabaeus crater, all of which constitute the right material for a cometary nucleus.

Radar waves can detect ice *via* the polarization properties of its diffuse reflections. In *Chandrayaan 1*, for example, the detector called *Mini-RF* emits left-circularly-polarized waves and receives both left- and right-circularly-polarized returns. The experiment I am involved with on *LRO* includes a similar detector called *Mini-RF* and it operates at 12·5-cm wavelength (S-band). Europa, Callisto, and Ganymede, which have icy surfaces, have high values of circular-polarization ratio (CPR, the received ratio of same sense to opposite-sense transmitted). The crater called Main L (14-km diameter) throws out a lot of angular material and acts like a lot of small corner reflectors, resulting in a high CPR. The 9-km-diameter crater on the floor of crater Rozhdestvensky has a high CPR inside but low CPR outside its rim. This high CPR is probably caused by water ice, because the floor of the crater is permanently dark and very cold. The crater that we expected to contain ice actually has the scattering properties we expect for those characteristics. *Lunar Prospector* was fitted with a low-resolution neutron detector allowing us to infer the presence of hydrogen because it is an absorber of neutrons. We interpret a low flux of medium-energy neutrons to mean large amounts of H_2. At the North Pole the ice layer must be about 2 or 3 metres thick and we obtain an overall estimate of 600 million cubic metres of ice. If we convert all that water into H_2 and O_2 it would enable us to launch from the surface of the Moon the equivalent of a space shuttle every day for 2200 years. We believe that the actual amount of ice at each pole exceeds 1 billion tons. However, it needs much less energy to extract polar ice on the Moon than to make water from the lunar regolith, and this finding makes it possible not only to go to the Moon but also stay there permanently. It can also become a service station for interplanetary travel. Water on the Moon completely changes the way in which we approach the problem of spaceflight. The Moon is therefore not just an object of scientific interest but also one of great practical value and usefulness.

The Vice-President. Thank you for an excellent talk! Any questions?

Mr. I. Ridpath. If the Moon is such a very good place, and I'm sure it is, why did President Obama decide to skip it and go on to the asteroids and Mars?

Dr. Spudis. He didn't know it was valuable [laughter]. It's because he flew to Florida with Buzz Aldrin. No, I'm serious. There was no serious thought given when the vision for space exploration was discarded. Another problem people had was that they thought that the *Constellation* project was too expensive, but it was fixable! We were able to devise an architecture where you can go to the Moon and do it for a lot less money than NASA wanted to spend, but they weren't interested in it.

Mr. Ridpath. So, if they knew now what they didn't know then, they'd have made a different decision?

Dr. Spudis. I would not like to speculate on that! [Laughter.] They seem serially incapable of learning from their mistakes.

Professor M. Rowan-Robinson. Is the limitation on human space flight fuel, or is it radiation?

Dr. Spudis. Well, it's both. In terms of practical things you want to do in space, fuel is the most immediate limitation. When you get to low Earth orbit, you typically have empty fuel tanks; that's basically what the rocket equation says. So if you're able to re-fuel in space, you suddenly have a much increased range. But water can address the second problem too, because water is a great radiation shield and if you can provide radiation shielding for materials you have in space, you don't have to lift it out of the gravity well of the Earth. If people are ever to live on the Moon, I suspect they will live in water-shielded habitats, either directly on or slightly below the surface, to protect them from Galactic radiation and solar events.

Professor S. Balbus. The fact that you've found water on the Moon and it was such a surprise, does that have implications for it being more widespread throughout the Solar System or, more generally, is there something special about the Moon?

Dr. Spudis. In fact, and you've probably read recently, it has been confirmed that the radar-bright material on Mercury is also pretty much pure water ice, which is almost as astonishing as finding it on the Moon. In fact there appears to be more on Mercury than on the Moon. And I think the question is why — I think it probably relates to the higher cometary flux the closer you are to the Sun, and also that Mercury's surface gravity is twice that of the Moon. But in terms of their axial orientation and the temperature of cold traps and the general configuration of the planet, the Moon and Mercury are very similar.

A Fellow. You showed a slide that had a number of missions that might have either been in orbit or have crashed into the Moon. I think I'm right in saying that the last soft landing on the Moon was *Luna 24* in 1976. Would you agree we're long overdue for a soft landing?

Dr. Spudis. Absolutely! While most of my colleagues want to see a sample returned from the South-Pole–Aitken basin, I would much prefer to see an instrumented rover go to the polar areas and measure the amounts and the composition of the volatiles *in situ*, so we can actually see what processes are occurring at the poles on the Moon. So, yes, a soft landing with a rover having an analysis capability is a very critical mission.

Dr. G. Q. G. Stanley. I could envisage that eventually we would be able to get ice-core samples, similar to what we do in Antarctica. If so, how far back in history do you think we could go, hoping we could pick up cometary history from this ice?

Dr. Spudis. We think that the polar cold traps result from the spin-axis configuration of the Moon. And dynamically, it's estimated that the Moon has been stable in that regard for about the last 2 billion years. It may be longer than that — it's a very poorly known figure. But at least for the last 2 billion years, we should have a record of the volatile history of the Moon. Beyond that, it's uncertain.

Mr. J. C. Taylor. Does the suggestion of significant levels of water in the interior of the Moon pose a difficulty to the giant-impact theory, and its origin?

Dr. Spudis. I think it does in its most simplified form. The giant-impact model is a very-high-energy event. People have said "well, we're finding these volatiles in the interior of the Moon and that casts doubt on such an origin", but only if you assume that the giant impact is an equilibrium process, in forming the proto-disc and the accretion of the Moon. If it was a heterogeneous, stochastic, non-equilibrium process, you could probably accommodate just about anything you want. One thing I have always been amazed about with the giant-impact model is that it can accommodate any new fact! [Laughter.] So it's clearly an elastic model.

The Vice-President. Thank you very much. [Applause.] The next talk is by Dr. Xavier Dumusque from the Observatory of Geneva, who is going to talk about 'The planet next door — an Earth-mass planet orbiting Alpha Centauri B'.

Dr. X. Dumusque. After centuries of wondering if other worlds were present beyond the Solar System, the first extra-solar planets were discovered about twenty years ago. With hundreds of billions of stars in our Galaxy, there was little doubt about the existence of other planets in our close neighbourhood. However, we had to find the first one to spark things off. The number of discoveries never stopped growing: at present, there are more than 800 extra-solar planets discovered, and over 2300 candidates awaiting confirmation. The first planets found around solar-type stars were 'hot Jupiters', *i.e.*, Jupiter-mass planets orbiting extremely close to their parent star, something inconceivable at the time. Then came the discovery of 'mini-Neptunes', 'super-Earths', 'lava worlds', and 'ocean planets'. All these types of planets are absent from our Solar System and show the incredible diversity in mass, in size, and in orbital period. A variety in architecture of the systems has also been observed, with the discovery of circumbinary planets, bodies that orbit not one but two stars, very compact multi-planetary systems, or planets orbiting on retrograde orbits.

Nowadays, several different techniques are used to search for exoplanets. The two major methods, Doppler radial-velocimetry and transit photometry, account for the big majority of detections. The first technique measures the gravitational stellar wobbles induced by a companion orbiting its parent star, and the second one detects the small decrease in stellar flux that occurs when a planet passes in front of the stellar disc. Radial-velocimetry and transit photometry are complementary, one giving the minimum mass of planets, the other their radius. The combination of both yields the planet's real mass and average density, which can then be used to infer the composition of these other worlds. In addition, when a planet transits its parent star, a tiny fraction of the stellar light will pass through the atmosphere of the planet, allowing us to obtain information about the principal constituent of the planet's atmosphere. Thus, sodium, hydrogen, carbon, oxygen, and water vapour have been found in the atmospheres of a few extra-solar planets.

The metre-per-second precision in radial-velocimetry, reached by instruments like *HARPS*, has revolutionized our understanding of exoplanets. Small-mass planets down to the size of the Earth have been detected, nevertheless, on short-period orbits. Gas giants similar to the ones in our Solar System have also been found. Unfortunately for planet searches, this precision also revealed previously unnoticed signal sources that are related to the stars themselves. Indeed, one starts to see the effect induced by pressure waves propagating on the surface of stars, convection, surface activity coupled with stellar rotation, and even magnetic cycles. There is a serious debate underway in the exoplanet community about whether it will ever be possible to derive a radial-velocity orbit with an amplitude of a tenth of a metre per second for a star like the Sun, as will be required to detect an Earth twin. The pessimists argue that such a planetary signal will be completely masked by intrinsic stellar signals. The optimists, though, think that with carefully designed observing protocols and data analysis, it will be possible to correct for those astrophysical effects.

An important and mandatory work, for planets like the Earth to be detected, is to characterize the different stellar signals that are affecting radial-velocity measurements, and to find ways to mitigate them. The first step was to find an optimum observational strategy that was capable of both averaging out high-frequency stellar signals, and sampling efficiently low-frequency signals to characterize them and finds ways to correct them. This observational strategy was then used to follow a small sample of ten stars that were all presenting at the time a very low level of radial-velocity variation. Out of this small sample, seven low-mass planets were discovered. Besides proving the efficiency of such an approach, this result also points out that low-mass planets are common. Recent statistical studies carried out on *HARPS* and *Kepler* results arrive at the conclusion that more than 50 % of solar-type stars harbour at least one planet.

Precise radial-velocity measurements obtained with *HARPS* allowed us to characterize the behaviour of activity-related stellar signals, such as variations induced by the presence of magnetic features on a rotating star, or radial-velocity perturbations produced by Sun-like magnetic cycles. The radial-velocity effect induced by a magnetic cycle depends not only on the strength of the cycle, but also on basic stellar properties like the effective temperature and the metallicity. Cool and metal-poor stars are less affected by magnetic cycles than hotter and more-metal-rich stars, making them better candidates to search for low-mass planets. Knowing the basic stellar properties and the strength of a magnetic cycle, it is possible to estimate the amplitude of the radial-velocity signal induced. Regarding the radial-velocity variation produced by magnetic features rotating with the star, it can be modelled by fitting sine waves at the rotational period of the star and its harmonics. In conclusion, the effects of activity-related stellar signals can be mitigated.

We observed Alpha Centauri B over four years using an optimum observational strategy to average out short-term stellar signals, and to sample appropriately longer-term perturbations related to activity. This unprecedented data set allowed us to characterize the different stellar signals and to develop correction techniques to mitigate their effects. After removing the contribution coming from magnetic features rotating with the star and from the magnetic cycle, it was possible to discover the lowest-mass planet found so far with the radial-velocity technique. Nearly having the mass of the Earth, this planet is also the closest one to the Solar System, as it orbits around Alpha Centauri B. This planet has been discovered on a short-period orbit of three days, being more a 'lava world' than a peaceful Earth twin. However, the precision reached after mitigating stellar signals would allow the detection of a planet four times more massive than the Earth in the habitable region of a solar-type star. This result represents a major step towards the detection of an Earth twin in the immediate vicinity of the Sun. We live in exciting times.

The Vice-President. We have time for a couple of quick questions.

Professor D. Lynden-Bell. When *GAIA* flies will we be able to see the wobble in proper motion, or not?

Dr. Dumusque. Of Alpha Centauri B?

Professor Lynden-Bell. Yes.

Dr. Dumusque. No. *GAIA* will be able to detect planets with the astrometry technique, and it will be good to detect some Jupiter-like planets, but the effect is too small to detect Earth-type planets. The astrometry technique is very good at detecting the planets that are very far away from the star, because the star will move a lot. In this case the planet period is just three days so the effect on the star is too small.

Dr. G. Morgan. Could you comment on what you think about the long-term orbital stability of your planet, because it's in a binary system? Would you expect to find planets like this in binary systems? Or is this unusual?

Dr. Dumusque. There have been some studies done on this, and in fact up to 1 to 2 AU from Alpha Centauri B, you don't have any effect from Alpha Centauri A, so basically the system is really stable.

Professor M. A. Barstow. You're going to put your new instrument on one of the *VLT* telescopes, but the *VLT* is not a single-user telescope. So how are you going to get enough time to do the cadence of monitoring you've been doing with *HARPS*?

Dr. Dumusque. This is a very good question, and I think it won't be possible to do it, basically, because we won't have enough time on the *VLT*. What we'd like to do is to have several other telescopes, not as big as the *VLT*, but for example like a 4-metre telescope, and several telescopes with instruments like *HARPS* with 1 m s^{-1} precision. With this, we still won't be able to detect planets like the Earth, but perhaps we can focus more on stars that are a little bit smaller than the Sun, for example, K dwarfs to M dwarfs, where the habitable-zone is closer. Around these types of stars, you should be able to detect an Earth-mass planet in the habitable zone. And in fact we have now two instruments already: we have *HARPS* in the south and in the north, but it would be very nice to have other instruments with this precision all around the world.

Professor D. W. Kurtz. I'm more optimistic than you are with *ESPRESSO* and *VLT*; your star is bright and I can imagine it being in the service queue to be observed quite frequently.

Dr. Dumusque. Yes, for sure, but in order to discover this planet we needed 500 measurements in total.

Professor Kurtz. They're quick though!

Dr. Dumusque. Yes, they're quick; I hope that it will be the case!

A Fellow. With your new instrument, will you be able to make any confident statement about whether your new planet has a moon around it or not? [Laughter.]

Dr. Dumusque. No, I don't think so! Do you want to find water on it? [Laughter.] I think 10 cm s^{-1} will really be the limit of the precision of the instrument, and this is enough to find an Earth. To find a moon, which is even smaller, you can use the transit technique to see if you have transit-time variations. If you have a small moon that is orbiting around the planet, you will have some very small shift in the time of transit, but for the moment we don't have the precision to detect such small bodies.

The Vice-President. Thank you very much! [Applause.] Now we come to the George Darwin Lecture, which will be presented by Professor Andrew Collier Cameron of St. Andrews. The title of his talk is 'Winds, tides and the migration of hot Jupiters'. [It is expected that a summary of this talk will appear in a future issue of *Astronomy & Geophysics*.]

The Vice-President. Are there any questions from the floor?

Reverend G. Barber. Are hot Jupiters found in multiple-star systems?

Professor Collier Cameron. A lot of us would like to know the answer to that! A number of them do have faint visual companions. In fact, there is a paper that came out just a few days ago, showing that WASP 12 has a close visual companion. They haven't found anything around WASP 18 yet; WASP 8, which has a planet in an eccentric orbit, does; HD 80806 has a known binary companion. So, some do, but not all. And I suspect that we will find, Nature being as cussed as it is, that not all of these things are going to have present-day companions. But Melvyn Davies has come up with the intriguing idea that in fact you can kick off this process much earlier in the evolution of the system: since a good many stars like the Sun are born in very rich open clusters, there is actually a sporting chance that you can have temporary binary arrangements that might kick these dynamical processes off. The companion then wanders away, and leaves a mis-aligned hot Jupiter orbiting an apparently single star. So again, ultimately, once we have instruments like *SPHERE*, that allow us really to probe for stellar companions close-in, I think the statistics of binarity among these things are going to be very, very interesting as a probe of the early dynamics.

Professor Barstow. So, in terms of future observation of these kinds of objects, is it about building up the sample and getting more examples, or is it more about detailed follow-up of individual objects?

Professor Collier Cameron. It's both. For the sort of statistical studies I've concentrated on today, looking at the eccentricities is very important. As I mentioned before, amongst the 98 *WASP* planets we have so far, there are only five that have measurable eccentricities. But those five are incredibly important because they are the ones that are part-way through the process, and coincidentally or not, they're also some of the most massive *WASP* planets. You can imagine that with all the extra orbital energy, and with the difficulty of stretching and squeezing a tightly-bound planet, that's maybe not entirely surprising.

Professor Barstow. So would *PLATO* give you access to these, or is it ground-based?

Professor Collier Cameron. We can do these from the ground; we're getting better at it all the time. We're getting down towards Saturn-to-Neptune-sized planets now. We've still got something like 31 million light curves in the *WASP* archive, most of which humans haven't even looked at yet.

Mr. M. F. Osmaston. I was interested in your discussion of the angular-momentum problem. The one I see is that if you are arguing that planets are migrating inwards, transferring angular momentum to the star, how do you equip the planet in the first place with that angular momentum?

Professor Collier Cameron. Ah! Well, the angular-momentum problem has two different facets to it. An accretion disc works by transferring mass in, and angular momentum out. For example, Jupiter is the reservoir where most of the proto-solar nebula's angular momentum finished up. For a planet to spin its host star up, it has to get close enough to the star to raise a tidal bulge that can exert a significant torque. To get there you have to rely either on viscous processes in the disc, or on having multiple planets in the system that undergo dynamical interaction in which one of them gets ejected, and the other falls onto a highly eccentric orbit. Or, alternatively, if you have a stellar companion that can pump Kozai oscillations, then you can create the eccentricity that way.

Professor Kurtz. In your picture of Daphnis' tidal tail, there certainly looked to be a third-dimensional out-of-plane component, both in the reflection and in the shadows. Is that a surprise? What is happening?

Professor Collier Cameron. It looks as though the velocity distribution of the particles is being stirred up in three dimensions. This causes them to rise up out of the ring plane and cast a shadow on the rings.

The Vice-President. We would like to thank you once more! [Applause.] I would like to invite you all to a seasonal drinks reception, which is in the RAS library, across the road. Also to remind you that the next monthly A&G open meeting will be on January 11 here, with the first James Dungey Lecture given by Peter Cargill.

RADIAL-VELOCITY OBSERVATIONS OF *HIPPARCOS* 'NEW HYADES CANDIDATES'

By R. F. Griffin
Cambridge Observatories

Perryman *et al.*, considering the application of their *Hipparcos* data to the Hyades star cluster, identified 39 previously unconsidered candidates for membership. The radial velocities of 21 of them were unknown; here the lack is to some extent made good in 15 cases. The 'score' is very poor: all but one of the 15 candidates have radial velocities that disqualify them outright as cluster members!

The opportunity is taken to discuss here 12 stars for which Perryman *et al.*'s tabulation appears to show a conflict between their own assessments of Hyades membership and the assessments of Griffin *et al.* In some cases it seems possible actually to resolve the conflict, and in others the tabulation appears to be misleading and no real conflict existed.

Readers will be well aware that, in a remarkably successful project under the direction of Dr. M. A. C. Perryman, the *Hipparcos* satellite obtained astrometric data (particularly parallaxes) of quite unprecedented accuracy for more than 100 000 stars. Soon after the *Hipparcos* data were published[1], Perryman himself, with a powerful group of collaborators, brought out in the light of the new data a comprehensive analysis[2] of the membership and properties of the Hyades star cluster. The present writer was not without interest in the subject, having been party to a previous analysis[3] masterminded by Dr. J. E. Gunn, starting from the standpoint of radial-velocity measurements. In respect of the Hyades those data[4], largely obtained with the Palomar Observatory's 200-inch *Hale Telescope* and the first digitally-operating radial-velocity spectrometer, represented in their field an advance in both quantity and quality that bears comparison with that achieved by *Hipparcos* in the astrometric field.

Perryman *et al.*'s table of Hyades-area stars, not all of which are claimed to be actual members of the cluster, includes 39 objects whose qualifications for membership had never previously been considered — or, if they had, they had been rejected without ever being discussed in print*. Of the 39 'new' objects, radial velocities were more or less known for only 18, and in all those cases tended to support the membership of those stars. There is, of course, no way for the reader to know whether there had been another 18, or even many more, other stars that equally fulfilled the astrometric criteria but whose known radial velocities clearly disqualified them from membership, so they never got as far as the table.

No systematic effort seems to have been made to supply radial velocities for the 21 stars that lacked them. Possibly that is because it could be seen, even without trial, as an unprofitable exercise! — almost all of the stars are at the

*That could easily happen, because some previous authors evidently harboured a prejudice (which has proved to be very well founded!) that cluster members had to have photometric properties that placed them in recognized sequences in the colour–magnitude diagram, so stars that fell elsewhere in the diagram were not seriously considered for membership at all.

extreme outskirts of the cluster in terms of their positions in the sky, and their actual linear distances from the cluster centre, determined with the use of the parallaxes and listed in Perryman *et al.*'s table, are almost all so great (some of them over 50 pc — further away than *we* are!) as almost to deny membership. Moreover, scarcely any of the stars would fall near the accepted sequences in the colour–magnitude diagram.

Six stars, out of the un-prepossessing set of 21, do not feature in the radial-velocity results below. They are as follows. HIP 12031 is a white dwarf whose velocity could not be measured with the radial-velocity spectrometer. HIP 15368, 15374, 20319, and 21092, with V magnitudes ranging from $11^{m}\cdot4$ to $12^{m}\cdot4$, are too faint to be measured, at any rate at all readily, with the Cambridge instrument. HIP 15368 is actually a 1″ visual binary, discovered as such by *Hipparcos* itself, and has the colour index of a solar-type star despite being about four magnitudes fainter than solar-type Hyades members. It is hard to know why HIP 20319 would ever have featured in Perryman *et al.*'s table: although its listed parallax ($11{\cdot}64 \pm 3{\cdot}73$ arc-milliseconds) is about half that of a normal Hyades member, the *Hipparcos* catalogue shows it to have a huge proper motion of 0″·669 in RA, whereas the typical value for a Hyades star at its position would not be much over 0″·1, so its tangential velocity appears to be at least ten times too great for it to be a member.

The final object among the six not observed by the writer is HIP 28774, the fainter component of a wide visual binary. Its spectral type has been given[5] as B8p — earlier than that of any actual Hyades member, since the cluster is too old still to possess stars of such an early type — and its apparent magnitude of about $9^{m}\cdot6$ would suggest a distance modulus of about ten magnitudes, putting it at about 20 times the distance of the cluster. In this case the photometric distance estimate would be preferable to one based on the parallax, which is given[1] as $12{\cdot}81 \pm 12{\cdot}80$ milliseconds. (There must be something dreadfully wrong with the *Hipparcos* data for the object: the re-reduction[6] of the same original data has given a result of $39{\cdot}18 \pm 11{\cdot}54$ milliseconds!) It might be more profitable to look at the parallax of the visual primary, which in the original *Hipparcos* catalogue is given as $-0{\cdot}34 \pm 3{\cdot}82$ milliseconds, putting *that* star, at least, 'beyond infinity', though not significantly so; the revised value[6] is $0{\cdot}88 \pm 2{\cdot}65$ milliseconds.

We come now to the 15 stars for which new radial-velocity observations can be given. They are listed in Table I, and their means are set out, with some other information, in Table II. It should be noted that the velocities are listed just as they were given by the *Coravel*; they are intended to be on the scale originally (and still usually) adopted[7] in Cambridge. That scale has been found to be about 0·8 km s^{-1} more positive than other, probably more nearly correct, scales. Furthermore, stars as blue as most of these ones are (F stars) usually need a negative correction of several tenths of a km s^{-1} to bring them into accord with other velocities, so it should be no surprise if the listed velocities for F-type stars differ systematically by something like +1·5 km s^{-1} from velocities determined elsewhere. Such a discrepancy has little bearing on the (usually decisive) departures from the velocities to be expected for Hyades membership.

Some notes on the individual stars

HD 17383 B Secondary component of a wide visual binary first listed by Herschel[8]; it features in Burnham's double-star catalogue[9] (as BDS 1429 B) but was considered too wide to be acceptable for Aitken's[10]. This is another case

TABLE I

Radial-velocity observations of 15 of Perryman et al.’s new Hyades candidates

Star	*Date (UT)*	*Velocity km s^{-1}*
HD 17383 B		
	2009 Oct. 26·06	+24·4
	2011 Sept. 30·14	+23·7
	Dec. 4·94	+22·7
	2012 Jan. 3·85	+24·7
	23·86	+22·8
	Feb. 18·80	+23·7
	Sept. 7·14	+24·0
	19·15	+24·1
	Nov. 3·10	+23·0
	18·05	+24·1
	2013 Jan. 9·87	+23·6
BD −0° 559		
	2009 Oct. 26·08	+60·0
	2011 Sept. 30·16	+58·3
	Dec. 4·95	+60·3
	2012 Nov. 22·02	+59·7
	2013 Jan. 9·90	+60·0
HD 24098		
	2012 Nov. 22·03	+32·3
	2013 Jan. 9·91	+32·2
HD 25068		
	2009 Oct. 26·09	+31·3
	2011 Dec. 4·97	+30·1
	2012 Nov. 19·08	+30·6
	2013 Jan. 9·91	+31·4
BD +3° 564		
	2009 Oct. 26·10	+23·7
	2011 Dec. 4·99	+23·0
	2012 Nov. 19·09	+23·1
	2013 Jan. 9·92	+22·6
HD 27887		
	2006 Nov. 26·08	+0·6
	2009 Oct. 25·14	0·0
	2011 Dec. 5·02	0·0
	2012 Nov. 19·09	−0·4
	2013 Jan. 9·93	−0·3
HD 31734		
	2006 Nov. 26·11	−29·0
	2009 Oct. 25·16	−28·9
	2011 Dec. 5·02	−28·4
	2012 Nov. 19·09	−28·8
	2013 Jan. 9·99	−28·4
HD 32662	Spectroscopic Binary Orbit under investigation	
HD 33313		
	2006 Nov. 26·12	+36·7
	2009 Oct. 25·18	+37·1
	2012 Nov. 19·10	+36·5
	2013 Jan. 9·99	+36·8
HD 35967	Spectroscopic Binary Orbit in Paper 230[22]	
HD 36215		
	2006 Nov. 29·18	−14·9
	2011 Dec. 6·04	−15·1
	2012 Nov. 19·11	−14·9
	2013 Jan. 10·00	−15·0
HD 36707		
	2006 Nov. 29·18	+42·4
	2011 Dec. 6·05	+42·5
	2012 Nov. 19·11	+43·0
	2013 Jan. 10·00	+43·1
HIP 26844		
	2012 Nov. 19·13	+23·2
	2013 Jan. 10·00	+23·3
HD 38779		
	2011 Dec. 6·07	+36·7
	2012 Nov. 19·18	+41·8
	2013 Feb. 3·96	+35·7
HD 39251	Spectroscopic Binary Orbit under investigation	

TABLE II

Mean radial velocities and other properties of the new Hyades candidates

Hipparcos no.	*Other name*	*V m*	*(B−V) m*	*Sp. type*	*RV km s^{-1}*	*n*	*v sin i km s^{-1}*	*Hyades?*
13042	HD 17383 B	10·24	0·60	G5	+23·7	12	1	N
16377	BD −0° 559	10·20	0·50	F8	+59·7	5	3	N
17950	HD 24098	6·49	0·41	F2	+32·2	2	47	N
18617	HD 25068	10·11	0·49	G0	+30·8	4	3	N
19449	BD +3° 564	10·10	0·65	F8	+23·1	4	3	N
20626	HD 27887	7·86	0·43	F5	0·0	5	11	N
23205	HD 31734	8·21	0·45	F8 V	−28·7	5	10	N
23662	HD 32662	7·69	0·51	G0 V	+30·0	41	12	N
24021	HD 33313	7·43	0·43	F8	+36·8	4	2	N
25694	HD 35967	8·16	0·57	F8	+7·3	41	5	N
25871	HD 36215	7·42	0·62	F8	−15·0	4	5	N
26159	HD 36707	8·77	0·46	F5	+42·8	4	16	N ?
26844	—	10·57	1·46	K7	+23·2	2	3	N
27431	HD 38779	7·07	0·40	F2 V	+38:	3	62	N
27791	HD 39251	7·95	0·54	F8	−25:	8	40	N

like that of HIP 28774 mentioned above, where *Hipparcos* seems to have been confused by a wide visual binary. The parallax given in the original catalogue[1] (**5**, p. 262) is about 11 ± 17 milliseconds, while the re-reduction[6] of the data gave 7 ± 9 milliseconds (it does not seem worth reproducing all the exact numbers given in the original to a hundredth of a millisecond). Likewise the proper motion is very uncertain, being given[1] as $\mu_\alpha \sim 44 \pm 32$, $\mu_\delta \sim -37 \pm 30$ milliseconds. On that seemingly shaky basis, however, Perryman *et al.*[1] evidently thought to see the object as a possible Hyades member and even determined its distance from the cluster centre to be 51·5 pc — far enough in any case, one might think, to disqualify it. The radial velocity merely confirms that the star has nothing to do with the Hyades. The confusion of *Hipparcos* seems to have applied to its magnitude as well as its position: the *V* magnitude derived from the measures by *Hipparcos* itself is $10^{m}·63$, whereas that from *Tycho*[11] is $10^{m}·24$. A referee who is evidently knowledgeable on such matters has kindly advised that it is the *Tycho* value that should be trusted.

HD 24098 Noted as a double star in the *IDS*[12]; *Hipparcos* gives the separation as 4″·5 and the Δm as $3^{m}·9$. Eggen[13] already proposed the star as a Hyades candidate in 1985. Gontcharov[14] lists a radial velocity of +30·9 km s^{-1} from the literature, which the writer has not been able to retrieve.

HD 25068 Double star, first noted by Heintz[15] as Hei 215. *Hipparcos* gives the separation as 4″·3 and the Δm as $2^{m}·8$.

HD 27887 Bartkevičius & R. Lazauskaitė[16] classified the star photometrically from observations on the Vilnius[17] system as 'MD-F6 V', the 'MD' standing for 'metal-deficient'; they listed [Fe/H] as –0·45, agreeing with typical Hyades values no better than the radial velocity does. Nordström *et al.*[18] gave a single radial-velocity measurement, of –2·4 km s^{-1}, with a stated 1-σ uncertainty of 0·7 km s^{-1}. They gave also a $v \sin i$ of 11 km s^{-1}, agreeing exactly with the writer's finding that is noted in Table II here.

HD 31734 This is a visual double star, discovered in 1890 by Hough[19], who designated it Ho 222. It appeared almost unchanged when *Hipparcos* observed it a hundred years later, at a separation of 1″·8, position angle 222°, and with a Δm of $2^{m}·3$. The *Tycho* photometry given by Fabricius & Makarov[20] shows the secondary star to be redder than the primary by about the amount that would be expected for a physical main-sequence companion, helping to confirm that that is what it is. Nordström *et al.*[18] gave the radial velocity (mean of two measures with the OHP *Coravel*) as –30·0 km s^{-1} and the rotational velocity as 10 km s^{-1}, in agreement with the Cambridge results.

HD 32662 It is hoped to publish a full account of this object in a future instalment of the writer's long-running series on binary orbits in this *Magazine*, but here we simply note that it has a highly eccentric orbit with a period of just 1000 days, and a γ-velocity that precludes its membership of the Hyades. *Hipparcos* noticed the orbital motion, and the syndicate behind it had a shot at determining the orbit. The data were not adequate to determine all the elements, so a circular solution was adopted. That is so far from the truth that the reliability of the other elements cannot be guaranteed; in fact the orbital period, given as about 668 ± 42 days, is off by about eight times its quoted standard deviation.

HD 33313 The star features in a paper by Gray[21] on the spectral classification of stars of intermediate Population II. He found its type to be 'F6 V m-1·75', where the number after the m is a measure of metal deficiency on a qualitative scale developed in the paper. Nordström *et al.*[18] gave a mean of two radial velocities as +34·9 km s^{-1} and a rotational velocity of 4 km s^{-1}.

HD 35967 An account of the writer's work on this star will be found in his Paper 230[22] in this issue of this *Magazine*.

HD 36215 Sandage & Fouts[23] published one radial velocity, of –19·1 km s^{-1}, obtained at the 100-inch Mount Wilson telescope, albeit with a much larger uncertainty than is routinely obtained with radial-velocity spectrometers on much smaller instruments. Nordström *et al.*[18] found the velocity to be –17·8 km s^{-1} from two observations, and gave the rotational velocity as 7 km s^{-1}. Isaacson & Fischer[24], in a report on a 'California Planet Search', have listed a radial-velocity 'jitter' of 4·244 m s^{-1} (to a precision, therefore, of one millimetre per second!) for HD 36215; they somehow omitted to give any actual velocity, but their figure for its variability makes it look as if we can be confident that it is constant within the accuracy that we need (or to which we can aspire) here. (But see HD 39251 below!)

HD 36707 The *Simbad* main heading for this star is 'HD 36707 – Variable Star'. The proposal for its variability was made just in a table, by Hopmann[25], more than 90 years ago. It has never been supported since, and has actually been refuted: the *Hipparcos* 'epoch photometry' shows no variation, and the star is in fact specifically noted with a C (for 'constant') in the relevant Column 52. It seems absurd that, once an allegation of variability has been made, no matter how tenuous the evidence, that the label should dog the star for ever, even after it has been shown to be false. The thumbnail picture of HD 36707 in the *Simbad* bibliography shows it clearly to be a double star: there is a companion about 12″ north of the principal star. The pair seems not to have been specifically listed anywhere until Greaves[26] noted it in 2004 as a common-proper-motion pair; it is almost certainly a physical double star. Among the 15 objects that form the principal subject of this paper, this is the only one that could conceivably be related to the Hyades, although it is at about double the proper distance. Its radial velocity is compatible with membership; the parallax[6] is not as accurate as most *Hipparcos* ones, at 10·12 ± 1·82 milliseconds, yielding an absolute magnitude of +3^{m}·8 ± 0^{m}·8, which within its uncertainty is appropriate to the spectral type and colour index. The star is quite close to the vertex of the Hyades motion, but its proper motion appears to a layman in astrometric matters to be of the right order and in the right direction. The object has a substantial rotational velocity, 16 km s^{-1}, that is not out of place for a star of its type.

HIP 26844 At 10^{m}·6, this is much the faintest of the 15 stars. Vyssotsky[27] drew attention to it as an M-type dwarf; he found it from objective-prism spectra, designated it with his name and the number 467, classified it as M0, and deduced its absolute magnitude to be 8^{m}·5. The most recent observations are by Koen *et al.*[28]; they noted that its parallax put its absolute magnitude at 8^{m}·92 ± 0^{m}·10, and gave its photometry as V = 10^{m}·57, $(B-V)$ = 1^{m}·46, $(U-B)$ = 1^{m}·23. Identical colour indices and a V of 10^{m}·61 were obtained by Weis[29] 20 years ago. In 1996 Upgren & Harlow[30] published four radial velocities (two of them taken on the same night as one another) obtained with a CCD on the David Dunlap Observatory 74-inch telescope. They were in excellent mutual agreement and had a mean of +23·1 km s^{-1}, just as we find for the star now. That velocity (if nothing else) obviously rules it out as a Hyades member. The star is much nearer to us than normal Hyades ones: its parallax shows it to be only 21 pc distant, and its proximity was already recognized by Woolley *et al.*, who entered it in their *Catalogue of Stars within twenty-five parsecs of the Sun*[31] as no. 9188, although they did not know much else about it.

HD 38779 Like HD 24098, this star was suggested as a Hyades member by Eggen[13]. He was not aware of any radial velocities, and even if he *had* been, they would not be likely to be sufficiently trustworthy to distinguish between the star's actual velocity and the one that would be appropriate to a Hyades member. In fact, the rotational velocity of about 60 km s^{-1} makes the *Coravel* velocities a bit doubtful too, but not liable to an error as large as the ~5 km s^{-1} that would be necessary to allow the star to belong to the cluster. Nordström *et al.*[18] offer, as a mean of three measurements, a velocity of +36·3 ± 0·9 km s^{-1}; they were not actually those authors' own observations — it is evident that they are the same ones as were described in an earlier paper by Nordström *et al.*[32], where they are shown as having been obtained with the 1·5-m *Wyeth* reflector at Harvard, presumably by her co-authors from there. Those velocities agree well with the Cambridge value when allowance is made for the probable systematic difference, but it remains slightly *further* from the Hyades velocity than the present writer's observations do, so it can in no wise encourage anyone to suppose that the discrepancy could be attributed merely to observational error. Both of the Nordström *et al.* papers give the rotational velocity of HD 38779 as 70 km s^{-1}. The star appears to be confirmed as a dwarf photometrically by Cernis *et al.*[33].

HD 39251 This is another visual double star: it was first noted in 1827 by Struve[34], who listed it as his no. 807 and ten years later gave measurements[35] of it. He put the magnitudes of the components at $7^{m}·3$ and $9^{m}·3$, with a separation of 2″·15 and a position angle of 139°·7. There have been several subsequent measurements, 14 of which are summarized in just two mean values in Aitken's catalogue[10], in which the pair is called ADS 4463. Regrettably, *Hipparcos* was confused by the system, and it appears in its catalogue as a single object with uncharacteristically ragged data obtained through what is termed a 'stochastic solution'. There have, however, been a few other measurements since Struve's; they seem to show a marginal increase in separation and a distinct advance in position angle. Mason *et al.*[36] obtained (by speckle interferometry) a separation of 2″·44 and a p.a. of 149°·8. Sandage & Fouts[23] gave one radial-velocity measurement, from the 100-inch Mount Wilson telescope, of –35·2 km s^{-1}, and Nordström *et al.*[18] referred to two with a mean of –16·9 km s^{-1}. Since the object has now proved to be a binary system, the velocities are of little utility in the absence of knowledge of their dates. The star appears in the list of Isaacson & Fischer[24], where it is shown as exhibiting a radial-velocity jitter of 4·521 m s^{-1}. Since the present writer reckons to have seen velocity variations in excess of 20 km s^{-1}, he is at a loss to know how to reconcile his experience of HD 39251 with Isaacson & Fischer's. Guillout *et al.*[37] included the object in a survey of young field stars. They noted it as a spectroscopic binary but did not give any radial velocities; they gave a T_{eff} that is close to that of the Sun (but a colour index that suggests that it is appreciably hotter), and a log g that shows that it is a main-sequence star. They also gave a $v \sin i$ of 41·1 km s^{-1}; Nordström *et al.* say 40 km s^{-1}, so both sets of authors are in good agreement with the present writer.

Discrepancies in Hyades membership assignments

Perryman *et al.*'s table[2] of stars that have at least sometimes been considered as possible members of the Hyades includes columns not only for their own assessment of the probability of membership but also for the assessments of certain other authors. In particular, there is a column for the opinions of a syndicate[4] led by the present writer. The latter was, naturally, pained to notice cases in which Perryman *et al.*'s assessment was diametrically opposed to that of his own consortium. There are 12 such cases, listed here in Table III. Unlike some other syndicates, Perryman *et al.* do not indicate degrees of confidence, in the form of percentages or otherwise, in their assessments, but normally adopt rather an all-or-nothing approach by noting either a 1 (for membership) or a 0 (for non-membership). Exceptionally, however, in just the 21 cases first referred to in the third paragraph of this present paper, they allow themselves a question mark to indicate their own uncertainty that arises in those cases from the radial-velocity ignorance that the first half of this paper sets out to redeem. In reporting other people's conclusions, they allow probabilities 1 or 0 only, giving in some cases a result more starkly than the quoted authors themselves actually intended.

Below, the opportunity is taken to review individually, in as nearly as possible a non-partisan fashion, the cases where Perryman *et al.*'s membership assessments of particular stars contradict those of Griffin *et al*. The latter included separate assessments based respectively on μ (proper motion), ϕ (photometry), and ρ (radial velocity) as well as their final conclusion (H?); Table III reproduces the individual as well as the final evaluations. It also reproduces the annotation 'B' that was appended by Griffin *et al.* to identify binaries, both where the individual criteria (visual duplicity, variable radial velocity) indicated it and also in the final conclusion. The last two columns show Perryman *et al.*'s transcription of the Griffin *et al.* assessments, and then their own, which are seen (in these selected instances) to be the exact reverse: in all cases except the last, Perryman *et al.* have accepted the membership of objects that they report Griffin *et al.* as having rejected.

TABLE III

Stars with conflicting assessments of probability of Hyades membership

HIP	*Other name*	*Griffin et al.*				*Perryman H?*	
		μ	*ϕ*	*ρ*	*H?*	*Griffin*	*Own*
12709	HD 16909	0	0	0B	0B	0	1
18692	HD 25153	0	0	1	0	0	1
19365	HD 26090	0B	0	?	0B	0	1
20187	G7 167	0?	1	0	0?	0	1
20553	vB 50	1	1B	?	1?B?	0	1
20601	vB 140	1	1?	?B	?B	0	1
21482	J 301	0	0	0B	0B	0	1
21788	vB 110	0	1?	0	0	0	1
22271	BD +25° 733	0?	1	1	?	0	1
23044	vB 149	0B	0	0	0B	0	1
23701	vB 151	1	1	0?B	?B	0	1
24020	vB 132	1?B	1?	1?	1B	1	0

HD 16909 This is a star for which an orbit had been given in an earlier paper by Griffin *et al.*[38]. They mentioned — in the present context, perhaps 'admitted' would be the appropriate word! — that "the space motion of the star is similar to that of the Hyades". It was also explained that the object had been drawn to attention by its large proper motion of about 0″·4 annually — "about four times the value typical of members in the center [*sic* — this was published in the US] of the cluster. This is because HD 16909 lies on the outer fringes of the cluster: in terms of angular distance from the convergent point, it is nearly twice as far removed as the cluster center, while in terms of linear distance it is only about half as far from us, and each of these circumstances results in a doubling, approximately, of the proper motion." The rejection by Griffin *et al.*[38] in the proper-motion and photometry categories was largely because they did not feel able to countenance as a member a star that was so far away (two hours in right ascension) from the cluster centre and was so much brighter (by about

2^m, owing to its relative nearness) than a member-star of its spectral type ought to be. But in the radial-velocity column, the rejection is very definite. When they determined the orbit (and thereby the γ-velocity of about +32 km s^{-1}) of HD 16909, Griffin *et al.*[38] remarked, "The γ-velocity … is only compatible with the Hyades motion if HD 16909 is approximately 45° from the convergent point; that condition would require the convergent point to be located at a right ascension of less than 6 hr" — which at the time they could not deny, although it seemed unlikely. The subsequently-determined[4] convergent point is beyond $6½^h$. In their paper, Griffin *et al.*[4] refrained from tabulating the radial-velocity residuals (discrepancies) from the values expected for membership wherever those residuals would exceed 5 km s^{-1}, on the grounds that such residuals would be irrelevant because the stars concerned are obviously nothing to do with the Hyades. There is no entry for HD 16909, so the computed residual must have been more than 5 km s^{-1}; the rejection from membership, therefore, is very secure. When HD 16909 was first proposed as a Hyades star (perhaps by Eggen[39], as a Hyades *group* member), its radial velocity was believed[40] to be +27 km s^{-1}, which would be more in keeping with expectation for a member.

HD 25153 Rejected by Griffin *et al.*[4] on proper-motion and photometric grounds, although the radial velocity was found acceptable for a Hyades member. The $(B - V)$ colour index of $0^m{\cdot}48$ is far too blue for a Hyades star of its V magnitude of $8^m{\cdot}24$. The parallax and proper motion, however, are both about half what a Hyades star could normally be expected to have, so if one were willing to accept as a member a star at double the proper distance, HD 25153 might qualify. Perryman *et al.* do in fact note a distance of 45·9 pc between the star and the cluster centre, and their acceptance of its membership simply shows that they are willing to countenance members far beyond the ~10-pc tidal radius of the cluster. In fact Perryman *et al.* themselves remark that the star "is only just contained within our kinematical selection limit. With a low metallicity according to Breger, further studies may well confirm [*sic*] it as a non-member."

HD 26090 A close visual binary with an orbital period of about 60 years. The present writer published a complete paper[41] about it in this *Magazine* a dozen years ago, after he had diligently observed a periastron passage during which the spectrum could be measured as double-lined. He provided an independent spectroscopic orbit (although the period was a bit uncertain and was fixed in the light of various considerations, one of which was the visual period). The paper, which was published *after* the one by Perryman *et al.*[2], devoted two complete pages to the question of Hyades membership, carefully referring to the attitudes adopted, respectively, by Griffin *et al.*[4] and the Perryman syndicate. There is no further light to be shed on the issue, and little purpose would be served by addressing it anew now, so the best thing to be done here is to quote the conclusion, which is as follows. "The writer accordingly reaffirms, in the light of the present better informed, more detailed and more individual discussion, the rejection asserted by Griffin *et al.*[4]."*

G7 167 This was one of a set of 35 Hyades-area stars, mostly about the ninth magnitude (G7 167 itself is among the faintest of them, at $9^m{\cdot}81$) which constituted a set of standards that were observed particularly carefully during the Palomar radial-velocity programme; it was measured in 12 separate observing seasons, and gave a mean velocity that had a standard error of 0·09 km s^{-1} and

*I have taken the liberty of substituting the reference number appropriate to the present paper for the '19' that was printed in the original.

was about 2 km s^{-1} away from the velocity appropriate to a Hyades member at that position in the sky. The issue of its membership was specifically discussed by Griffin *et al.*[4], who concluded as follows. "[G7-167] has a velocity, constant within observational uncertainty, that is about 1 km s^{-1} outside the range comfortably admissible for members. If that were the only thing 'wrong' with it, the idea that G7-167 is a long-period binary member might well be entertained; but, in fact, its proper motion is agreed by all concerned to be about 50% larger than would be expected for a Hyades member." Perryman *et al.* themselves note that the star was rejected as a Hyades member by the astrometric experts van Altena[42] and Hanson[43].

van Bueren 50 This star was a rated as a "very probable" member of the Hyades (the highest accolade that was offered) in the very first paper that ever discussed the issue — the astrometric work of Kapteyn & de Sitter[44] in 1904. Griffin *et al.*[4] were slightly hesitant about accepting it as a member, the conclusion in their table (reproduced in Table III above) being "[yes]B?"*. That conclusion was not correctly reported by Perryman *et al.*'s '0', implying rejection. In fact Griffin *et al.* passed the star as a member on astrometric and photometric grounds; their hesitation arose only from the radial velocity. The mean velocity was well over 2 km s^{-1} off the proper value for a Hyades star, but there was evidence — not absolutely conclusive — that it had varied slightly during the 15 years of the Palomar observing campaign and had become 'worse' during that time. The velocities did not agree as well as they ought to have done: the listing in Griffin *et al.* shows a χ^2 of 44·53 from nine observations, so *something* was clearly wrong or out of control. After discussing some van Bueren[45] stars whose membership they were obliged to reject, Griffin *et al.* continued as follows. "There is one additional star, vB 50, which stands appreciably off the velocity expected for a Hyades member at that position in the sky, but in the case of vB 50 the discrepancy is not too large to arise from duplicity, and indeed both Hardorp (1980) and Carney (1982) have postulated duplicity on photometric grounds." Finally Mason *et al.*[46] succeeded in resolving vB 50 as a 'visual' binary by speckle interferometry, and when the *Hipparcos* catalogue[1] was published in 1997 it showed that the system had been resolved by the satellite even before Mason *et al.* had observed it. There soon ensued a critical time for the system, which in 1995 passed a periastron passage in an orbit of extreme eccentricity and a period somewhat exceeding 100 years. Continuing double-star measurements have documented an arc of well over 300° of the 'visual' orbit, but it was not until 2008 that Zirm[47] published an orbit based on them. Naturally, the present writer was watching carefully to see what vB 50 would do after the conclusion of the Palomar work, and has recently, in a comprehensive 172-page paper[48] in another journal, given a completely independent spectroscopic orbit that is very agreeably similar to Zirm's astrometric one. The γ-velocity that has now been established differs from the Hyades velocity expected by Griffin *et al.*[4] by just $+0{\cdot}34 \pm 0{\cdot}07$ km s^{-1}, removing any remaining doubt about the system's credentials as a Hyades member.

van Bueren 140 This is another star for which Griffin *et al.*'s membership conclusion of '?B' is not correctly reported by Perryman *et al.*'s '0'. Griffin *et al.* noted approval of its proper motion, approval with a question mark of its photometry, but a query followed by B for Binary — just as in their overall assessment just quoted — as regards its radial velocity, which stands off the

*The 'yes' symbol was a tick in that paper; it is represented in Table III here by a 1, in analogy with Perryman *et al.*'s usage.

'proper' value by +1·48 km s^{-1}. The orbit is very eccentric (0·85), remarkably so at the comparatively short period of 156 days; it was determined in 1985 by Griffin *et al.*[38]. When the form of the orbit was known from observations that were effectively single-lined ones of the primary star, radial velocities of the very weak secondary were observed in an exciting run on the Palomar 200-inch reflector in twilight time begged at the critical epoch from a succession of dark-run observers who (together with the Director and administration of the Observatory) cooperated magnificently in allowing and facilitating that irregular operation. Moreover, the editor of the *Astronomical Journal*, who published the paper giving the orbit, found the diagram thereof so remarkable that he selected it for reproduction on the front cover of the relevant issue of his *Journal* — the astronomical equivalent of being on the cover of *Time Magazine*! The 1985 authors[38] happily asserted that "the γ-velocity established here must represent almost conclusive evidence that we are dealing with a true member of the Hyades." They[4] (it was the identical consortium) sobered up a bit when they had determined[3] the convergent point and found that the velocity expected for a Hyades member at the somewhat anomalous position (only +4° declination) of vB 140 was not quite what they had previously supposed, so they qualified their acceptance with a query in their final assessment[4]. But what they still really thought about it may be gathered from the flag that they placed against its entry to show that it was one of "the 140 stars constituting the dynamical sample of Hyades members used to determine the convergent point of the cluster in [Gunn *et al.*[3]]." Thus the apparent conflict with Perryman is largely illusory.

Johnson 301 Another binary, this time of very short period (1·79 days), whose orbit was determined by Griffin *et al.*[38]. An elaborate historical and scientific discussion in that paper will not be repeated here, apart from saying that that J 301 is a BY Dra variable, and has a 16^m c.p.m. companion about 2′ away. It has Hyades-like space motion but is only 18 pc away from us — a foreground object only about ⅖ of the way to the centre of the Hyades cluster. On those grounds it was rejected from cluster membership in the astrometric and photometric departments by Griffin *et al.*[4]. Still more telling was the γ-velocity, which stands just over 3 km s^{-1} away from the value proper to that position in the sky, an altogether unacceptable discrepancy. It is fair, however, to recall that although the orbit fitted perfectly well the authors' own Palomar and Cambridge observations taken in 1973–83, it did not represent satisfactorily certain measurements previously published by others. Although there was no indication of any change in the γ-velocity during the time that the 1985 authors were watching the star, there could be misgivings that higher multiplicity could contribute to the mis-fitting of the earlier observations, possibly falsifying the γ-velocity determined from what might be only a relatively small part of a long-period 'outer' orbit. That is, however, pure speculation, and the available data would not suffice for any meaningful investigation into the possibility of further multiplicity. In retrospect it now seems unfortunate that the object has not been retained on the writer's observing programme, at a low level of one or two observations per season, these past thirty years.

van Bueren 110 This star has a proper motion in RA that is too small for a Hyades star at its position in the sky, and its radial velocity is almost 5 km s^{-1} 'off'. Definitely a non-member!

BD +25° 733 Griffin *et al.*[4] were uncertain as to whether the proper motion of this star was sufficiently close to that expected for a Hyades member (it must be remembered that they were writing before *Hipparcos* was even launched), and they put a question mark in the 'proper motion' column of the membership assessments in their paper. They gave ticks for photometry and radial velocity, and their final conclusion was another question mark, reported by Perryman *et al.* as a rejection. The latter authors' approval of the star astrometrically, however, now favours membership.

van Bueren 149 This star, far from the centre of the Hyades (almost on the celestial equator, in Orion) was discovered by Aitken[49] 100 years ago to be a very close, equal, visual binary. *Hipparcos* gave a position angle about 180° away from Aitken's, but it appears that that was only because it adopted the opposite star as the primary — obviously correctly, since it found a difference of about $0^{m}\cdot3$ in the magnitudes. It was rejected from cluster membership by Griffin *et al.*[4] in all three criteria. Its proper motion is too small, and the star is too faint in relation to its colour index (notwithstanding that its luminosity is practically doubled by duplicity). There might seem to be a prospect of viewing those defects as redeemed, and the object to be at least a fellow-traveller with the Hyades, now that the parallax shows it to be nearly twice as far away as the centre of the cluster. The radial velocity, however, remains an insuperable obstacle to membership — being more than 5 km s^{-1} too low, it did not even merit an entry in Griffin *et al.*'s 'residual' column.

van Bueren 151 Another star for which the writer[48] has recently provided a spectroscopic orbit. Griffin *et al.*[4] indicated slight hesitation over its cluster membership, approving it in terms of proper motion and photometry but questioning it on radial-velocity grounds. On the basis of their model of the cluster, it has a velocity residual of 1·88 km s^{-1}. (All the same, they flagged it as one of the 140 stars that were included in their dynamical model of the cluster, so it can be seen that in their hearts they accepted its membership!) The γ-velocity given in the recent paper differs by just 0·03 km s^{-1} from the value listed 25 years ago, so it does not alter the situation at all: whether one approves the star as a member is entirely dependent upon the level of velocity offset that one sets as the limit of acceptability.

van Bueren 132 This is the only case in which Perryman *et al.*[2] were more pessimistic than Griffin *et al.*[4] — the former rejecting it while the latter approved it. The star forms a wide visual binary with vB 131. Griffin *et al.* accepted it, with some unease indicated by suffixing a question mark to their tick, in all three adjudications (proper motion, photometry, and radial velocity). In the last of those, they listed a velocity residual of 3·18 km s^{-1}. For a single star, that would be a discrepancy fatal to membership, but ignorance of the visual orbit and the possibility that its γ-velocity might prove to be nearer expectation for cluster membership may be held to keep hope alive in this instance.

References

(1) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997.
(2) M. A. C. Perryman *et al.*, *A&A*, **331**, 81, 1998.
(3) J. E. Gunn *et al.*, *AJ*, **96**, 198, 1988.
(4) R. F. Griffin *et al.*, *AJ*, **96**, 172, 1988.
(5) E. K. Kharadze & K. B. Chargeishvili, *AJ*, **99**, 379, 1990.
(6) F. van Leeuwen, *Hipparcos, the new reduction of the raw data* (Springer, Dordrecht), 2007.
(7) R. F. Griffin, *MNRAS*, **145**, 163, 1969.
(8) W. Herschel, *Mem. RAS*, **3**, 177, 1828.
(9) S. W. Burnham, *General Catalogue of Double Stars Within 121° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1906, part I, p. 28.
(10) R. G. Aitken, *New General Catalogue of Double Stars Within 120° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1932.
(11) [Announced by] E. Høg *et al.*, *A&A*, **355**, L27, 2000.
(12) H. M. Jeffers & W. H. van den Bos, *Publ. Lick Obs.*, **21**, 1963.

(13) O. J. Eggen, *PASP*, **97**, 807, 1985.
(14) [Announced by] G. A. Gontcharov, *Astr. Lett.*, **32**, 759, 2006.
(15) W. D. Heintz, *ApJS*, **58**, 439, 1985.
(16) A. Bartkevičius & R. Lazauskaitė, *Baltic Astr.*, **5**, 1, 1996.
(17) V. Straižys, *Vilnius Astr. Obs. Biul*, **36**, 3, 1973.
(18) [Announced by] B. Nordström *et al.*, *A&A*, **418**, 989, 2004.
(19) G. W. Hough, *AN*, **125**, 5, 1890.
(20) [Announced by] C. Fabricius & V. V. Makarov, *A&A*, **356**, 141, 2000.
(21) R. O. Gray, *AJ*, **98**, 1049, 1989.
(22) R. F. Griffin, *The Observatory*, **133**, 156, 2013.
(23) A. Sandage & G. Fouts, *AJ*, **93**, 592, 1987.
(24) [Announced by] H. Isaacson & D. Fischer, *ApJ*, **725**, 875, 2010.
(25) J. Hopmann, *AN*, **214**, 425, 1921.
(26) [Announced by] J. Greaves, *MNRAS*, **355**, 585, 2004.
(27) A. N. Vyssotsky, *AJ*, **61**, 201, 1956.
(28) C. Koen *et al.*, *MNRAS*, **403**, 1949, 2010.
(29) E. W. Weis, *AJ*, **105**, 1962, 1993.
(30) A. R. Upgren & J. J. B. Harlow, *PASP*, **108**, 64, 1996.
(31) Sir R. Woolley *et al.*, *Royal* [*Greenwich*] *Obs. Ann.*, no. 5, 1970.
(32) B. Nordström *et al.*, *A&AS*, **126**, 21, 1997.
(33) K. Cernis *et al.*, *Baltic Astr.*, **7**, 625, 1998.
(34) F. G. W. Struve, *Catalogus Novus Stellarum Duplicium et Multiplicium* (Typographia Academia, Dorpat), 1827, p. 21.
(35) F. G. W. Struve, *Stellarum Duplicium et Multiplicium Mensurae Micrometricae per Magnum Fraunhoferi Tubum Annis a 1824 ad 1837 in Specula Dorpatensis* (Typographia Academia, Petropolis), 1837, p. 64.
(36) B. D. Mason *et al.*, *AJ*, **124**, 2254, 2002.
(37) [Announced by] P. Guillout *et al.*, *A&A*, **504**, 829, 2009.
(38) R. F. Griffin *et al.*, *AJ*, **90**, 609, 1985.
(39) O. J. Eggen, *MNRAS*, **120**, 540, 1960.
(40) A. H. Joy & S. A. Mitchell, *ApJ*, **108**, 234, 1948.
(41) R. F. Griffin, *The Observatory*, **121**, 214, 2001.
(42) W. F. van Altena, *AJ*, **74**, 2, 1969.
(43) R. B. Hanson, *AJ*, **80**, 379, 1975.
(44) J. C. Kapteyn & W. de Sitter, *Publ. Astron. Lab. Groningen*, no. 14, 1904.
(45) H. G. van Bueren, *Bull. Astr. Inst. Netherlands*, **11**, 385, 1952.
(46) B. D. Mason *et al.*, *AJ*, **105**, 220, 1993.
(47) H. Zirm, *IAU Commn. 26 Inf. Circ.*, no. 166, 2008.
(48) R. F. Griffin, *JA&A*, **33**, 29, 2012.
(49) R. G. Aitken, *Lick Obs Bull.*, **8**, 52, 1914.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 230: FIVE SHORT-PERIOD DOUBLE-LINED BINARIES: HD 25788, HD 32704, HD 35967, HD 45191 (V455 AUR), AND HD 213896 (LL AQR)

By R. F. Griffin
Cambridge Observatories

The five stars came to the writer's attention in different ways, but they share the properties of being both double-lined and of short period, and are probably of at least as much interest and significance as most of those treated in this series of papers. Their short periods practically guarantee that they are all main-sequence systems. Two of them are eclipsing systems that have received variable-star designations.

HD 25788 is a nearly equal binary ($q \sim 1{\cdot}03$), probably with types close to F9 and G0, in a circular orbit whose period is about 3·8 days and is determined to a fraction of a second. The components' axial rotations are synchronized to their orbital revolution. There is a systematic change of the velocity residuals towards negative values over the duration of the observing campaign, indicating the existence of a third component of the system in a long-period 'outer' orbit, which we can do little to elucidate at present.

HD 32704, classified as G8 V and located south of the celestial equator, shows only a slightly greater disparity between its components than HD 25788; it too is in a circular orbit, which has a period slightly over 10 days. Disconcertingly, the component that has the larger radial-velocity signature has the smaller mass; it alone appears to rotate in synchronism with the orbit, while its companion rotates four times as rapidly.

HD 35967 is a late-F pair whose components are barely distinguishable from one another. It has an orbit with a small but definite eccentricity and a period near 11·4 days that is good to three seconds. Its γ-velocity of about +7 km s^{-1} demonstrates that the system, which was proposed by the *Hipparcos* syndicate as a candidate for membership of the Hyades, is certainly *not* a member. The axial rotations are synchronized.

HD 45191 (V455 Aur) is a bright ($7^{m}{\cdot}3$) triple system whose two observable components are somewhat unequal F stars in a slightly off-circular orbit with a period very close to π days. That orbit has already featured in this series of papers, in Paper 160; it is refined here, with the standard error of the period, particularly, being substantially reduced — it now stands at about a tenth of a second. When Paper 160 was written, the γ-velocity had changed

by no less than 10 km s^{-1} between the two seasons in which the system had been observed; in order to determine the 'inner' orbit, it was necessary to adopt some approximation to the 'outer' one. The circular approximation that was adopted was entirely serviceable for that purpose but was far from the truth; the real 'outer' orbit is now shown to have a period of 4205 ± 17 days and the high eccentricity of 0·73.

Finally, HD 213896 (LL Aqr), another southern-hemisphere object, is a noticeably unequal pair, probably of types F and G, in a 20-day orbit of moderate eccentricity (0·31). The eclipses occur when the velocities of the two components differ by about 31 km s^{-1}, so the spectra of the components are then quite separated and the eclipsed one can be seen to fade out. This system (with HD 32704 only, among those treated in this paper) has had its radial velocity measured previously by others, but the orbit then obtained depended on very few data points.

Introduction

The five stars came to attention in different ways, but they all share the characteristics of having double-lined spectra (and therefore small Δm) and short orbital periods. Also, in relation to their interest they have been accorded remarkably little attention in the literature: their *Simbad* bibliographies extend only to 5, 6, 4, 12, and 16 papers, respectively. In other respects there are considerable differences between them. For example, whereas V455 Aur is agreeably bright, at $7^{m}\cdot28$, and passes only five minutes of arc from the Cambridge zenith, HD 32704 and LL Aqr are southern objects that culminate at about 56° zenith distance, where the sky and seeing tend not to be so good; moreover, those stars are considerably fainter, being respectively a little brighter, and a little fainter, than ninth magnitude.

HD 25788

Ten years ago, Griffin & Suchkov published a substantial paper[1] that referred to more than 100 stars, the majority of which had proved to be binaries, that had been identified as such by Suchkov's ΔM_{c_0} 'criterion of over-luminosity', whereby[2] the luminosity corresponding to the *Hipparcos* parallax was significantly greater than was to be expected on the basis of *ubvyβ* photometry. While the principal investigation[1] was in train, Suchkov provided the writer with a listing of additional binary candidates that the photometry had also suggested to be metal-deficient. One of them was HD 25788, an 8^{m} star in the Hyades field, though not itself a member of the Hyades. It is to be found about 3° preceding γ Tau (the star at the apex of the 'V' asterism that includes Aldebaran), and is only ½° north-preceding HR 1279, a 4″ visual binary known to Hyades *aficionados* as vB[3] 11/12.

The star seems never to have been measured in *UBV* or to have received an MK classification. We have reasonably accurate values of $V = 8^{m}\cdot15$, $(B-V) = 0^{m}\cdot52$ derived from *Tycho* measurements as re-determined in *Tycho 2*[4]. The *HD* type is G0; the colour index would ordinarily suggest F8, but the reduced line-blanketing in the spectrum of a metal-deficient star would make the colour index bluer, so G0 may well be the best type to adopt, pending a careful modern classification. The star features in Nordström *et al.*[5], where it is accorded an [Fe/H] value of –0·54, corroborating the metal-deficiency found by Suchkov, although in both cases the abundance estimates were derived photometrically rather than spectroscopically and so cannot be considered independent. They are, moreover, derived on the implied basis that the spectrum is that of a single star, but the components are sufficiently similar that that ought not significantly to vitiate the result. The (revised[6]) *Hipparcos* parallax yields a distance modulus of $4^{m}{\cdot}25$, with an uncertainty of $0^{m}{\cdot}2$, and thus an absolute magnitude near $3^{m}{\cdot}90$, half a magnitude brighter than might correspond to a type of G0 V. That agrees with Suchkov's conclusion that the object is 'over-luminous', as would readily be explained by duplicity.

Radial-velocity observations were begun with the Cambridge *Coravel* in 2002 and soon showed the object to be double-lined (*cf.* Fig. 1), giving two velocities that could be as much as 150 km s^{-1} apart. The orbital period of about 3·83 days was divined as soon as a connected series of observations was made, in the second observing season; it is now determined, by the 51 observations which are set out in Table I, within a standard error of four millionths of a day, or about a third of a second. In the solution of the orbit, the velocities obtained from the slightly weaker 'dips' given by the secondary star have been weighted 0·8 relative to the primary's. When the twin velocities were very different from one another, they have often been measured in separate integrations which, however, have been short enough and have followed one another so quickly that the assignment of their mean time to both of them does not increase the apparent observational errors significantly. Those errors, whose r.m.s. values are a little over 1 km s^{-1} for both components, are much larger than are usually obtained with the *Coravel*, for two reasons. One is that the dips are not only shallow but also considerably broadened by the axial rotation of the stars, but there is a second cause, which is evident from the run of the residuals seen in the last two columns of Table I. There is a clear trend, from mainly positive values in 2003 to mainly negative ones in 2011/12, no doubt arising from a variation in the γ-velocity of the system owing to motion in a long-period 'outer' orbit opposite an additional component star that is not directly detected.

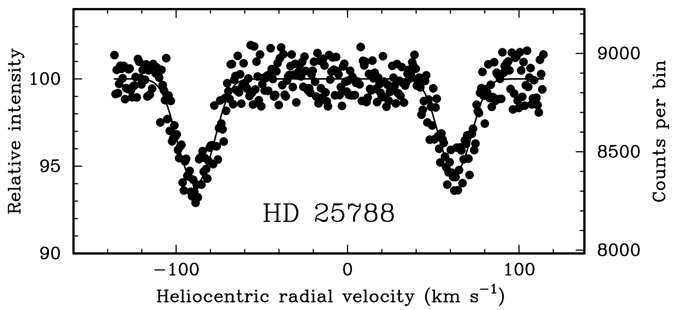

FIG. 1

Radial-velocity trace of HD 25788, obtained with the Cambridge *Coravel* on 2003 October 27. Its somewhat noisy appearance arises in part from the shallowness of the 'dips', which are seen to be only about 6% (primary, left-hand one) and 5% deep; the noise is actually little more than Poisson statistics render inevitable.

TABLE I

Radial-velocity observations of HD 25788

All the observations were made with the Cambridge Coravel

Date (UT)			*MJD*	*Velocity*		*Phase*	*(O–C)*	
				Prim. *km s⁻¹*	*Sec.* *km s⁻¹*		*Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
2002	Dec.	11·094	52619·094	−12·2		0·740	—	—
2003	Jan.	25·835	52664·835	−37·5	+13·1	12·693	+1·7	−0·9
	Sept.	18·149	900·149	+18·0	−42·4	74·185	+1·2	+1·2
		29·101	911·101	+58·6	−86·3	77·047	+0·2	+0·1
	Oct.	12·131	924·131	−81·9	+59·5	80·451	+2·2	−0·7
		17·045	929·045	−14·1		81·736	—	—
		18·119	930·119	+62·6	−88·4	82·016	+1·4	+0·9
		25·139	937·139	+32·0	−58·7	83·851	+0·9	−0·4
		27·106	939·106	−61·5	+39·1	84·365	+0·7	+1·4
		27·973	939·973	−76·4	+50·2	·591	−0·8	−1·3
		28·121	940·121	−63·7	+38·9	·630	+0·3	−0·7
		28·219	940·219	−53·8	+30·9	·656	+0·9	+1·0
	Nov.	17·058	960·058	+27·4	−51·0	89·840	+0·5	+3·0
	Dec.	16·045	989·045	−76·2	+54·0	97·415	+0·9	+1·0
		17·982	990·982	+53·5	−80·3	·921	+1·0	+0·1
		21·993	994·993	+60·7	−88·1	98·969	+0·5	+0·1
2004	Oct.	22·139	53300·139	−31·6	+8·6	178·709	+0·4	+2·0
2005	Jan.	16·863	53386·863	−63·0	+41·9	201·371	+1·5	+1·8
	Sept.	29·121	642·121	+55·3	−79·7	268·075	+1·8	+1·7
	Dec.	18·000	722·000	+58·7	−84·2	288·949	+1·0	+1·5
2006	Nov.	29·123	54068·123	−70·7	+48·5	379·397	+1·7	+0·4
	Dec.	9·059	078·059	+61·8	−88·5	381·993	+0·3	+1·1
2007	Oct.	21·097	54394·097	−80·5	+54·2	464·579	−2·0	−0·2
2008	Feb.	11·851	54507·851	−38·0	+13·8	494·305	+0·4	+0·7
2009	Jan.	6·880	54837·880	−82·7	+62·2	580·548	+1·5	+1·9
	Dec.	20·940	55185·940	−86·9	+64·3	671·502	+0·6	+0·6
2010	Jan.	31·970	55227·970	−88·6	+64·7	682·485	−1·4	+1·3
	Oct.	7·152	476·152	−51·1	+28·1	747·339	+1·6	+0·2
	Dec.	11·974	541·974	−84·6	+61·6	764·540	+0·6	+0·2
2011	Sept.	30·186	55834·186	+46·7	−77·3	840·900	−0·6	−2·3
	Oct.	7·137	841·137	−29·5	+2·6	842·716	−0·9	−0·6
		20·129	854·129	+43·0	−73·7	846·111	−1·1	−2·0
	Nov.	28·070	893·070	−32·6	+4·6	856·287	−2·3	−0·2
		29·992	894·992	+5·3	−32·4	·790	−0·1	−0·6
	Dec.	5·008	900·008	+45·9	−74·3	858·100	−1·4	+0·7
		28·952	923·952	−59·8	+33·4	864·357	−0·3	−1·5
2012	Jan.	3·894	55929·894	+50·2	−78·6	865·910	+0·2	−0·8
		10·901	936·901	−14·0		867·741	—	—
		12·867	938·867	−13·3		868·255	—	—
		14·879	940·879	−0·7	−28·9	·781	−2·0	−1·3
		16·958	942·958	−46·6	+20·1	869·324	−0·2	−1·3
		21·976	947·976	−62·8	+36·2	870·635	−0·6	−1·5

TABLE I (*concluded*)

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
2012 Jan.	22·957	55948·957	+42·7	—	870·892	−2·2	—
	23·974	949·974	+28·2	−57·8	871·157	+0·2	−2·6
	28·889	954·889	−82·6	+58·7	872·442	0·0	0·0
	31·853	957·853	+1·8	−28·7	873·216	−0·9	+0·4
Feb.	1·932	958·932	−89·0	+61·5	·498	−1·5	−2·2
Sept.	8·183	56178·183	+6·4	−34·0	930·792	−0·2	−0·9
	12·176	182·176	+24·9	−54·4	931·836	−0·4	−2·1
Nov.	6·123	237·123	+11·6	−40·4	946·194	−1·0	−1·2
	11·047	242·047	−89·0	+62·3	947·481	−2·0	−0·9

The only radial velocities known to have been obtained for HD 25788, other than those in Table I, are six that were noted by Nordström *et al.*[5] as showing that the star was a binary with a mean velocity of −12·5 ± 1·0 km s^{-1}. The data were not published and so cannot be included in this discussion; the mean velocity that is given is a γ-velocity, probably not obtained from an actual orbital solution but by averaging the twin velocities from occasions when the system was found to be double-lined, or else by what is known as Wilson's method[7]*. The elements given by the Cambridge observations are as follows:

P	= 3·826762 ± 0·000004 days	$(T_0)_{524}$	= MJD 54621·4856 ± 0·0016
γ	= −12·97 ± 0·13 km s^{-1}	$a_1 \sin i$	= 3·924 ± 0·013 Gm
K_1	= 74·56 ± 0·24 km s^{-1}	$a_2 \sin i$	= 4·037 ± 0·019 Gm
K_2	= 76·72 ± 0·36 km s^{-1}	$f(m_1)$	= 0·1647 ± 0·0016 $M_\odot$
q	= 1·029 ± 0·006 (= m_1/m_2)	$f(m_2)$	= 0·1795 ± 0·0025 $M_\odot$
e	≡ 0	$m_1 \sin^3 i$	= 0·698 ± 0·008 $M_\odot$
ω	is undefined in a circular orbit	$m_2 \sin^3 i$	= 0·678 ± 0·006 $M_\odot$

R.m.s. residual (unit weight) = 1·16 km s^{-1}

The orbit is plotted in Fig. 2. When the eccentricity is allowed as a free parameter, it takes the value 0·0007 ± 0·0025, so there is no case for considering the orbit to be other than circular. The 'true' orbital period (measured in the rest-frame of the system) is 3·826927 ± 0·000005 days; it differs from the observed period by some 36 standard deviations, so the difference is very significant statistically. There is, however, a sense in which the difference is only of 'academic' significance: the application of the true period in the 'derived' quantities (on the right in the informal table above) does not produce results differing appreciably from those that would be obtained with the observed value, because the difference is masked by the relatively large proportional uncertainties of the velocity amplitudes $K_{1,2}$ which are also involved in those quantities.

It has not proved possible to pursue very far the matter of the variation of the γ-velocity of HD 25788. Major parts of the data set are concentrated at the beginning and end of the observing campaign, in 2003 and 2011/12.

*The velocities of the components are plotted against one another and fall more or less close to a straight line, whose gradient gives the mass ratio and whose intersection with the 45° line $V_1 = V_2$ gives the γ-velocity. The method is quite an obvious one, but was described in a short paper specifically dedicated to it by Wilson[7]. A variant of it was, however, previously utilized by Joy, who in his investigation of U Sge[8] plotted the velocities of the two components against phase, and deduced the mass ratio from the relative gradients of the two lines and the γ-velocity from their intersection.

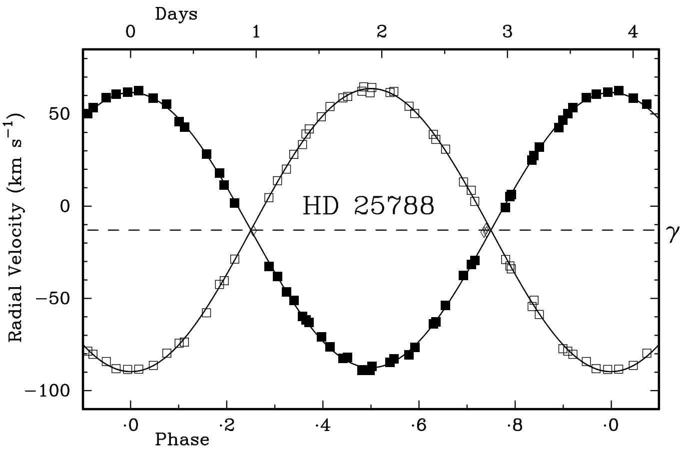

FIG. 2

The observed radial velocities of HD 25788 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Filled squares represent the primary star, open ones the secondary. There are four open diamonds, difficult to see near the phases where the curves cross at the γ-velocity, that represent close blends that were reduced as single-lined and not used in the solution of the orbit. All observations were made with the Cambridge *Coravel*.

Separate orbital solutions restricted to just those intervals yield γ-velocities of −12·37 ± 0·18 and −13·94 ± 0·14 km s^{-1}, respectively; the difference of 1·57 ± 0·23 km s^{-1} is obviously extremely significant statistically, being nearly 7σ. A new solution of the orbit, in conjunction with the assumption of a linear downward trend in the γ-velocity, reduced the sum of squares of the (weighted) deviations from about 125 (km $s^{-1})^2$ (the value obtained with the presently adopted solution, set out in the informal table above and in Table I) to about 80. That demonstrates again the cast-iron statistical significance of the variation in γ: the loss of just one degree of freedom (the slope of the trend) has gained 45 (km $s^{-1})^2$, while the remaining 80 (km $s^{-1})^2$ represent the total contribution of the other 88 degrees of freedom (93 observations fitted with the five elements of a double-lined circular orbit). Those numbers yield a variance ratio, $F_{1,88}$, of ~50; the 1% point of $F_{1,88}$ is about 7 and the 0·1% point less than 12, so 50 is altogether off the scale!

Actually, the imposition of a linear trend does not fit the observations very satisfactorily: there is no compelling evidence of any change in the γ-velocity before 2011. The run of data is not nearly long enough to permit the fitting of an 'outer' orbit, as is done in the case of HD 45191 below; the best that can be done, if a linear trend does not seem an adequate approximation, is to try to match the variation of γ to some part of a sine wave 'by eye', which is in fact exactly how HD 45191 itself was treated[9] when the variability of its γ-velocity was first discovered. The best that the writer has been able to do with HD 25788 is illustrated in Fig. 3, where the observations (nett of the 3·8-day variation) are plotted with the sine wave that is deemed to represent an approximation to their trend (though only over the interval that they cover). That reduces the sum of the squares of the residuals to about 72 (km $s^{-1})^2$; of course it is not even to be *hoped* that another reduction as dramatic as the first one could be made, because fewer than 30% of the total number of observations lie between the two

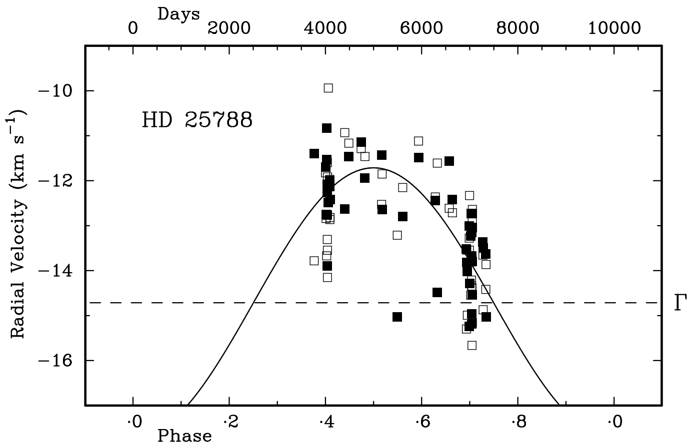

FIG. 3

An effort to mimic the variation of the γ-velocity of HD 25788 by a Keplerian velocity curve representing motion of the observed binary system in a long-period orbit opposite an unseen companion. The curve is based on a circular orbit with P = 10 000 days, T = MJD 53900, γ-velocity = –14·75 km s^{-1}, K = 3 km s^{-1}. Filled and open squares represent the radial velocities of the primary and secondary components, respectively, with the velocities related to the short-period orbit, computed from the adopted orbital elements, subtracted from them. Thus each point represents the γ-velocity implied by the observation. The curve matches tolerably well the part of the outer orbit that has been seen, but cannot be expected to have any predictive power; it is exactly analogous to Fig. 5 (upper panel) of Paper 160[9], which pertains to V455 Aurigae at a correspondingly early stage, with its velocities matched by an 'orbit' that, though far from the final one presented below in Fig. 9, did reasonable justice to the data then available.

principal seasons at the beginning and end of the observing campaign. It will be seen that in Fig. 3 there is one specially anomalous measurement — a low point about halfway through the data set, which here — though not in Table I — has a residual nearly twice as bad as any other. If that one is rejected, the sum of squares falls to 63, just half the number given by the straight double-lined solution. The halving of the sum of squares might be expected to imply that the standard errors of the orbital elements will all be reduced by factors of about $\sqrt{1/2}$, and that does indeed prove to be the case (except for the γ-velocity itself, which can be determined only when the outer orbit has been seen completely round). But for the time being a conservative stand is taken — the orbital solution that is put forward in this paper is the straightforward double-lined one, with no cognizance taken of any change of γ and no observation rejected.

The mean equivalent widths of the two 'dips' seen in radial-velocity traces (defined just as in spectra, but here the unit of width is the km s^{-1} instead of the Å) are 1·23 for the primary and 1·10 for the secondary. The secondary star is (according to supposition) slightly later in spectral type and therefore matches the (K2) mask in the *Coravel* a bit better than the primary, so the difference in the brightnesses of the stars is somewhat greater than the difference in dip strengths. In the 'Suchkov paper'[1] it was found appropriate to adopt for stars near solar type a factor of 1·15 by which to multiply the magnitude difference that corresponds arithmetically to the ratio of dip strengths — in this case $0^{m}\cdot14$ — to obtain the difference of V luminosities. Thus the relative dip strengths suggest a ΔV of about $0^{m}\cdot16$ between the components. The slope of the main sequence in the vicinity of type G0, in terms of V magnitude, is such that that magnitude difference corresponds to just about one spectral sub-type.

Another measure of the difference between the components of HD 25788 is the mass ratio derived directly from the solution of the orbit. The rather sharp lower envelope shown by Andersen's graph[10] of log(mass) against colour index shows a quite constant gradient that is equivalent to a change of –0·013 per spectral sub-type. The logarithm of the observed q value is almost exactly 0·013, thus corroborating very satisfactorily the type difference of one sub-type implied by the radial-velocity traces. Judged purely on the basis of the colour index, and allowing for it to be slightly bluer than normal owing to the metal-deficiency of the system, the actual types could be expected to be about F9 and G0.

A pair of stars with the proposed magnitude difference of $0^{\rm m}\cdot16$ is[11] $0^{\rm m}\cdot68$ brighter in sum than the primary component individually, so the absolute magnitude of $3^{\rm m}\cdot9 \pm 0^{\rm m}\cdot2$ derived from the parallax[6] leads to components with M_Vs close to $4^{\rm m}\cdot6$ and $4^{\rm m}\cdot7$. They would imply types near G1 and G2, but the uncertainty of the parallax is such that there is no serious conflict with the assessment based on the $(B-V)$ colour index. The orbit gives the masses of the stars, multiplied by the factor $\sin^3 i$, as just under 0·7 $M_\odot$; if the actual masses are estimated as being near to 1·1 $M_\odot$ then $\sin^3 i \sim 0\cdot62$, $\sin i \sim 0\cdot85$, $i \sim 58°$. The (constant) linear separation of the components is seen to be about 8 Gm/$\sin i$ or about ¹⁄₁₆ of an AU, so at HD 25788's distance of about 70 pc it will subtend less than a single millisecond of arc.

The *Coravel* records yield the rotational velocities of the stars as well as the radial velocities. The former repeat quite well from one observation to another, despite the shallow nature of the dips, and give $v \sin i$ values close to 12 and 11 km s^{-1} for the primary and secondary components, respectively. Allowance for the $\sin i$ estimated in the preceding paragraph shows the actual equatorial velocities to be about 14 and 13 km s^{-1}. If the stars' axial rotations are synchronized to their orbital revolution, as is likely, their implied radii are close to 1·1 and 1·0 $R_\odot$, which are just what one would expect for stars of their putative types. Thus everything that we have tried to determine about the HD 25788 system hangs together in a reassuringly agreeable fashion.

HD 32704

HD 32704 is an inconspicuous star in one of the two north-following corners of Eridanus, very close to the boundary with Orion. It is a little over a degree north-preceding β Eri, a star that to the uninitiated naked-eye observer appears to be part of Orion, some 3° from Rigel at about 'one o'clock' as seen near the meridian from the northern hemisphere. The magnitude and $(B-V)$ colour index of HD 32704 are given by *Simbad* from *Tycho 2* as $8^{\rm m}\cdot63$ and $0^{\rm m}\cdot78$. Zajtseva[12], who used the star as a comparison object for the variable UX Ori (only 10′ north-preceding HD 32704 but in the adjacent constellation) gave the photometry as $V = 8^{\rm m}\cdot73$, $(B-V) = 0^{\rm m}\cdot74$, $(U-B) = 0^{\rm m}\cdot28$. Xing & Xing[13], however, without any explanation or attribution, list the V and $(B-V)$ in their Table 1 as $8^{\rm m}\cdot66$ and $0^{\rm m}\cdot759$. They also list a spectral type of G8 V, which for all that the reader is told may be quoted from Kazanasmas[14]. The re-classification[15] of the *HD* stars in the relevant declination zone, however, puts the type at G6 IV.

Other types that have been mentioned for the star are G6–7[16] and G9[17]. Energetic processes probably occurring in coronal material led to the star's featuring in catalogues of objects detected by *EUVE* in a wavelength band variously given[18,19] as 70–130 Å and 58–174 Å. There are two papers[13,17] referring to the λ6708-Å Li I line, giving discordant values of its equivalent width as 24 and 70 mÅ respectively. Unfortunately the parallax of HD 32704 has not been measured, but the shortness of the orbital period, determined below to be about 10 days, tends to confirm the main-sequence classification[14] of the system.

It appears from the paper[16] by Pribulla *et al.* that photometric variations were noticed in HD 32704 by *MOST*, a small (~2-cubic-foot) Canadian satellite[20] kindly launched from Russia. The nature and amplitude of the variations are not mentioned in the paper[16], and the results of *MOST* seem not to be accessible by the general public. Be that as it may, it was data from *MOST* that galvanized Pribulla *et al.* (a consortium working at the David Dunlap Observatory in Toronto) into observing HD 32704 spectroscopically. They mention that the spectra were taken with a medium-resolution spectrograph operating at the Cassegrain focus of the 1·88-m reflector, but to discover that they were obtained with a CCD involves a trawl through their previous papers until one[21] that actually describes the system is located. Anyway, they discovered that HD 32704 is double-lined, but before they could establish an orbit for it they suffered the misfortune to have the observatory closed permanently over their heads. Their paper refers to a computer-accessible tabulation, however, which proves to give radial velocities for the two components of HD 32704 on five dates; there are two additional dates with no velocities against them. To determine the velocities from their spectra, they used an idiosyncratic method that they called 'BF', which in this case stands for 'Broadening Function'; it seems unable to deal with double-lined spectra unless the component spectra are fully resolved from one another, thus explaining the two 'empty' dates in the table. A note in the *DDO* paper reads, "HD 32704 – relative intensity of components in BFs was found to significantly [*sic*] vary."

It was after belatedly noticing Pribulla *et al.*'s discovery that HD 32704 is double-lined (and those authors' inability to follow it up any further) that the present writer placed the star on the Cambridge observing programme. He has made 36 measurements, all but one of which have been reduced as double-lined. (The cross-correlation method, that was originally developed by the writer and has been utilized by him ever since, often allows twin velocities to be determined reasonably reliably even from heavily blended spectra.) The velocities are set out in Table II.

Fig. 4 shows a radial-velocity trace of HD 32704. It illustrates what seems to be an extraordinary feature of the system: despite the areas of the two dips, and the masses of the components, being tolerably similar to one another, the profiles of the dips are quite different (demonstrating, no doubt, greatly differing rotational velocities for the stars). Their mean equivalent widths (areas) are $2{\cdot}02 \pm 0{\cdot}03$ and $1{\cdot}75 \pm 0{\cdot}04$ km s^{-1}, the deeper one being the one with the larger equivalent width. Not only is there an overwhelming (5σ) statistical difference in the mean values, but in all except two cases the individual traces yield for the two dips equivalent widths that differ in the same sense. The present author is ready to attribute the discrepancies between different traces to observational errors, and sees no compelling support for Pribulla *et al.*'s claim that there are significant variations in the relative strengths of the components; he notes, however, that it has been necessary to reverse those authors' assignment of the components in one of their five observations — a fact that substantially weakens their case.

TABLE II

Radial-velocity observations of HD 32704

The sources of the observations are as follows:
2007/8 — Pribulla et al.[16] *(weighted ½ in orbital solution);*
2011/2 — Cambridge Coravel (weight 1)

Date (UT)	*MJD*	*Velocity* *Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$	*Phase*	*(O – C)* *Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$
2007 Nov. 23·299	54427·299	+26·2	+86·2	$\overline{138}$·596	−0·9	−1·9
2008 Jan. 28·129	54493·129	93·1	27·9	$\overline{131}$·072	+0·8	+2·3
Feb. 1·065	497·065	21·6	94·0	·460	0·0	+0·7
29·041	525·041	+58·0		$\overline{128}$·212	—	—
Mar. 6·029	531·029	58·0		·801	—	—
13·026	538·026	21·4	96·0	$\overline{127}$·489	+1·0	+1·6
17·028	542·028	86·2	29·6	·883	−0·1	−1·7
2011 Sept. 28·184	55832·184	72·4:	47·8:	0·807	+0·9	+2·3
30·204	834·204	95·5:	23·1:	1·006	−0·6	+1·1
Oct. 1·191	835·191	89·0	28·9	·103	+0·5	−0·3
7·173	841·173	44·0:	69·9:	·691	−0·6	−1·4
16·145	850·145	24·6	90·3	2·574	+0·2	−0·4
20·158	854·158	95·7:	25·5:	·969	+0·3	+2·8
24·105	858·105	34·7	79·7	3·357	0·0	−1·1
Nov. 18·082	883·082	73·0	42·9	5·814	−0·1	−1·1
23·071	888·071	45·3	67·3	6·305	−0·1	−3·2
28·048	893·048	69·9	49·1	·795	+1·2	+0·9
Dec. 5·059	900·059	20·9	96·2	7·484	+0·4	+1·9
8·005	903·005	63·5	50·9	·774	−0·5	−1·8
14·974	909·974	21·7	93·0	8·460	+0·1	−0·4
17·043	912·043	38·9	77·7	·663	+0·3	+0·7
28·987	923·987	78·7	39·2	9·838	+0·5	+0·1
2012 Jan. 3·947	55929·947	24·3	90·4	10·425	−0·2	−0·1
10·991	936·991	86·1:	28·9:	11·118	−0·1	−2·5
12·893	938·893	45·5	69·4	·305	0·0	−1·1
13·918	939·918	26·8	86·1	·406	0·0	−2·2
14·907	940·907	19·9	94·1	·503	−0·5	−0·4
16·919	942·919	47·3	68·7	·701	+0·6	−0·6
21·925	947·925	70·9	49·0	12·193	−0·5	+3·4
23·899	949·899	29·0	86·3	·388	−0·4	+0·5
26·938	952·938	43·2	73·1	·686	−0·3	+0·8
28·862	954·862	84·1	31·5	·876	−1·0	−1·0
Feb. 7·847	964·847	82·1	34·8	13·858	+0·1	−0·6
11·817	968·817	58·1		14·249	—	—
18·835	975·835	94·0	24·2	·939	+0·6	−0·4
Nov. 3·145	56234·145	35·8	80·0	40·351	+0·1	+0·2
6·135	237·135	34·7	81·7	·645	−0·4	+1·3
11·089	242·089	83·8	32·2	41·133	+0·1	−1·7
19·052	250·052	90·6	26·7	·916	−0·4	−0·2
29·081	260·081	89·0	27·4	42·903	−0·3	−1·1
30·073	261·073	95·6	22·2	43·000	−0·5	+0·2
2013 Jan. 3·010	56295·010	38·7	77·0	46·339	+0·6	−0·5
9·977	301·977	+95·8	+21·7	47·024	+0·1	−0·7

In what follows, the component that gives the dip that is much the deeper, and has somewhat the larger area, is deemed to be the primary star. The mean values

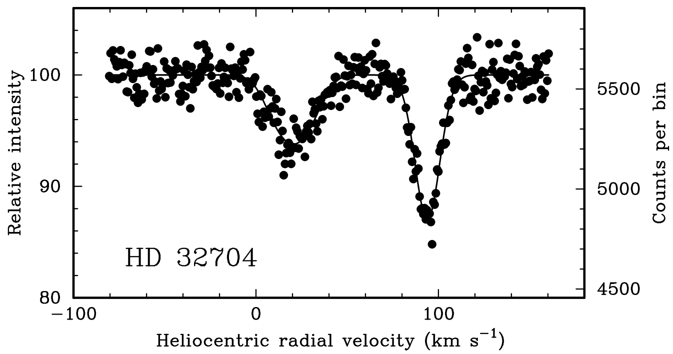

FIG. 4

Radial-velocity trace of HD 32704, obtained with the Cambridge *Coravel* on 2012 November 30.

of $v \sin i$ derived from the Cambridge traces are 3·1 km s^{-1} for the primary and 12·3 km s^{-1} for the secondary, with standard deviations of 0·5 km s^{-1} for each of the means. Those are formal error estimates derived from the inter-agreement of the individual values and do not include any allowance for differences in 'turbulence' or other factors between different stars. The value for the primary is likely to be slightly over-estimated, because several of the individual values are zero, and of course no lower values are permitted even though on occasion the dip profile may appear slightly narrower than the standard zero-rotation one.

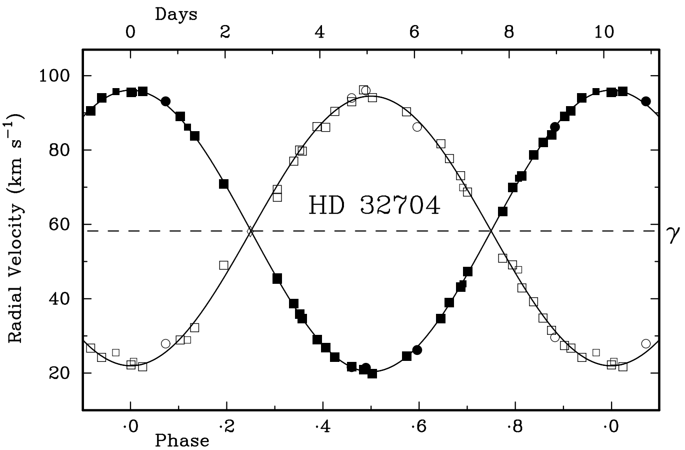

FIG. 5

As Fig. 2, but for HD 32704. The coding of the points is the same, but in this diagram there are in addition a few smaller symbols denoting half-weighted velocities, and also circles, representing measurements by Pribulla *et al.*[16]. Also plotted is one *Coravel* observation reduced as single-lined and not used in the solution of the orbit: it is shown as an open diamond (exactly where the velocity curves cross at phase ·25).

The five pairs of velocities given in Pribulla *et al.*'s table are reproduced at the head of Table II; they have been accorded an empirical adjustment of +2·0 km s^{-1} to bring them into systematic agreement with the Cambridge data, and in the solution of the orbit they have been given half-weight. The two 'empty' dates are also listed just so that the corresponding orbital phases can be shown: they do indeed prove to be near to times when the two components would have similar velocities. The Cambridge velocities of the secondary star, being derived from 'dips' that are relatively shallow and wide compared with those of the primary, have needed to be globally weighted ¹⁄₁₀ in the solution; the same factor more or less suits the Pribulla data too. Five Cambridge observations that were noted as particularly uncertain at the time that they were taken are marked in Table II with a colon and have been half-weighted. The orbit is plotted in Fig. 5 and has the following elements:

P	$=$ 10·16480 ± 0·00016 days	$(T_0)_4$	$=$ MJD 55895·136 ± 0·005
γ	$=$ +58·23 ± 0·07 km s^{-1}	$a_1 \sin i$	$=$ 5·293 ± 0·015 Gm
K_1	$=$ 37·88 ± 0·11 km s^{-1}	$a_2 \sin i$	$=$ 5·07 ± 0·05 Gm
K_2	$=$ 36·28 ± 0·35 km s^{-1}	$f(m_1)$	$=$ 0·0574 ± 0·0005 $M_\odot$
q	$=$ 0·958 ± 0·010 ($= m_1/m_2$)	$f(m_2)$	$=$ 0·0504 ± 0·0015 $M_\odot$
e	$\equiv$ 0	$m_1 \sin^3 i$	$=$ 0·211 ± 0·005 $M_\odot$
ω	is undefined in a circular orbit	$m_2 \sin^3 i$	$=$ 0·220 ± 0·003 $M_\odot$

R.m.s. residual (unit weight) $=$ 0·43 km s^{-1}

The choice of the circular solution for the orbit is definitely warranted: its relaxation results in an eccentricity of only 0·0022 ± 0·0029 and reduces the sum of the squares of the residuals only trivially, from 14·85 to 14·73 (km s^{-1})^2. The 'true' period, 10·16283 ± 0·00016 days, differs from the observed one by 12 standard deviations.

An immediately unsettling feature of the elements is that the star that has been deemed here, through its having a distinctly larger signature than its companion in the radial-velocity traces, to be the primary component turns out to have (almost equally decisively — by more than 4σ) the larger radial velocity and thus the *smaller* mass. There is no obvious way of accounting for the contradiction. Late-G dwarf stars in a 10-day circular orbit have no business to have evolved or to have indulged in mass transfer.

The masses given by the orbit, where they are multiplied by the factor $\sin^3 i$, are about a quarter of those that stars of their (G8) type may be expected to have, so we must suppose that $\sin i \sim \sqrt[3]{1/4}$ or 0·63, so $i \sim 39°$. If we now account for the value of $\sin i$ in the measured projected rotational velocities, we obtain about 4·9 km s^{-1} for the star that according to its dip signature should be the primary, and 19·5 km s^{-1} for the other component. As indicated above, the former value is liable to be slightly over-stated, but it nevertheless is very close to the rotational velocity that would be expected for a star almost the size of the Sun rotating in synchronism with the 10-day orbital period. The other, slightly more massive, component is evidently rotating much (four times) more quickly; even in the face of the classification of the system as being[15] of type G6 IV, it seems absurd to suggest that it, too, could be synchronized but be four times the radius, because in that case it would have to be *very* cool, otherwise it would be much 'too bright'.

HD 35967

This 8^{m} star is to be found about 2° preceding the bright B star ζ Tau. Being about an hour of right ascension following the centre of the Hyades, it could perhaps be grudgingly admitted to be more or less in the field of that cluster, of which it was actually proposed as one of 39 possible 'new' members by Perryman *et al.* in their comprehensive paper[22] setting out the application of 'their' *Hipparcos* data to the problems of the membership and dynamics of the Hyades. Their criteria were astrometric (parallax and proper motion); they knew nothing about the star's radial velocity. There are some other stars in Perryman *et al.*'s Hyades list whose membership is rendered doubtful through the lack of radial velocities; some light is shed on that question in the preceding paper[23] in this issue.

Even now, there seems to be almost *no* additional information available about HD 35967; some spectrophotometric indices exist in a table referred to by Robinson *et al.*[24] in a paper devoted to metal-rich stars, but even there its atmospheric parameters seem not to have been deduced. *Simbad* offers its magnitude and $(B-V)$ colour index, derived from *Tycho 2* photometry, as $8^{m}{\cdot}16$ and $0^{m}{\cdot}57$, respectively. There has not even been a spectral classification since Miss Cannon's F8 in the *Henry Draper Catalogue*; the colour index might suggest G0. The (revised[6]) parallax of $10{\cdot}08 \pm 1{\cdot}04$ milliseconds puts the object about 100 pc away (and therefore further from the centre of the Hyades than *we* are!); with a distance modulus of almost exactly five magnitudes, it must have an absolute magnitude of about $3^{m}{\cdot}15$, the uncertainty of the parallax contributing a 1-σ uncertainty of about $0^{m}{\cdot}2$ to the luminosity. Allowing three-quarters of a magnitude for its duplicity, we find that its individual stars must have absolute magnitudes of $3^{m}{\cdot}9 \pm 0^{m}{\cdot}2$, corresponding, it must be said, better to the *HD* classification of F8 than to the G0 that was adumbrated above on the basis of the colour index. If the system really is metal-rich, as its inclusion in the Robinson *et al.* paper[24] could be expected to imply, at least part of the discrepancy (which in any case is not significant) might thereby be bridged.

The *Hipparcos* authors' lack of a radial velocity for HD 35967 seemed to be a major omission that the writer, finding after several years still not rectified in the literature, set out in 2006 to redeem. Within seconds of initiating that operation, he discovered that the object was double-lined, with nearly equal components (see Fig. 6 for an illustration); that the system must have a short period, because the components' velocities differed by more than 100 km s^{-1}; and that its radial

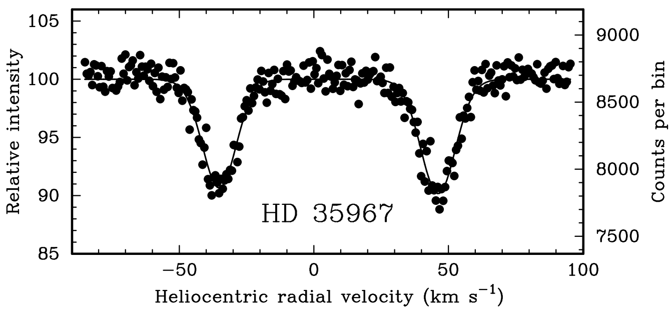

FIG. 6

Radial-velocity trace of HD 32704, obtained with the Cambridge *Coravel* on 2012 February 19.

TABLE III

Radial-velocity observations of HD 35967

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* Prim. km s⁻¹	Sec. km s⁻¹	*Phase*	*(O – C)* Prim. km s⁻¹	Sec. km s⁻¹
2006 Nov. 29·167	54068·167	+60·8	−47·2	0·517	−0·3	−0·2
Dec. 6·142	075·142	−20·8:	+34·1:	1·129	−1·6	0·0
9·136	078·136	+62·3	−47·6	·392	+0·4	+0·2
10·071	079·071	+65·1	−51·0	·474	+0·2	−0·1
12·019	081·019	+30·3	−15·9	·645	+0·2	−0·2
17·139	086·139	−31·0	+46·6	2·095	+0·1	+0·6
2007 Jan. 15·005	54115·005	+34·8	−20·7	4·629	−0·6	+0·3
Feb. 3·975	134·975	+60·8	−47·3	6·382	+0·2	−0·8
Apr. 11·860	201·860	+27·2	−13·6	12·254	−0·5	−0·3
Oct. 21·169	394·169	−16·4	+31·4	29·137	0·0	+0·1
2008 Feb. 16·989	54512·989	+51·7	−36·4	39·568	−0·1	+1·2
Mar. 31·851	556·851	+64·2	−50·4	43·419	−0·2	0·0
Oct. 28·243	767·243	−49·0	+63·9	61·889	−0·2	−0·1
2009 Mar. 26·854	54916·854	−48·9	+63·6	75·024	+0·1	−0·6
Dec. 23·075	55188·075	−36·2	+50·4	98·834	−0·1	−0·7
2010 Jan. 29·997	55225·997	−6·7	+20·8	102·164	−0·4	−0·2
31·017	227·017	+27·8	−12·8	·253	+0·3	+0·3
Feb. 5·986	232·986	−17·6	+32·1	·777	0·0	−0·3
Nov. 28·103	528·103	+15·4	−2·1	128·686	−0·6	−0·6
Dec. 12·139	542·139	−53·5	+68·6	129·918	−0·6	+0·5
2011 Oct. 16·172	55850·172	−54·7	+70·2	156·960	+0·2	+0·1
Nov. 23·085	888·085	+39·3	−23·8	160·289	+0·1	+1·1
28·154	893·154	−2·4	+16·1	·734	−0·7	−0·3
Dec. 5·089	900·089	+53·2	−39·6	161·343	−0·3	−0·3
8·030	903·030	+43·4	−29·4	·601	−0·2	−0·1
15·065	910·065	+14·9	−0·2	162·218	+0·1	+0·1
29·015	924·015	+65·8	−51·4	163·443	+0·4	0·0
2012 Jan. 2·112	55928·112	−26·1	+41·4	163·803	+0·2	+0·1
11·019	937·019	+47·8	−33·8	164·585	0·0	−0·2
12·932	938·932	−8·6	+23·5	·753	+0·1	0·0
17·031	943·031	−24·7	+39·7	165·113	+0·4	−0·3
21·996	947·996	—	−41·9	·548	—	−0·1
26·993	952·993	−53·3	+70·1	·987	+0·4	+1·2
28·920	954·920	−8·3	+23·7	166·156	+0·8	−0·2
Feb. 19·910	976·910	−33·5	+47·8	168·087	0·0	−0·7
Mar. 1·922	987·922	−40·5[R]	+58·3	169·054	+2·2	+0·5
Nov. 3·154	56234·154	+21·7	−7·1	190·670	+0·2	−0·1
6·179	237·179	−54·2	+69·6	·936	+0·1	+0·1
11·128	242·128	+58·3	−44·3	191·370	−0·5	+0·4
Dec. 5·028	266·028	+65·8	−50·9	193·469	+0·6	+0·2
26·073	287·073	+47·1	−32·7	195·316	0·0	+0·2

[R] Rejected

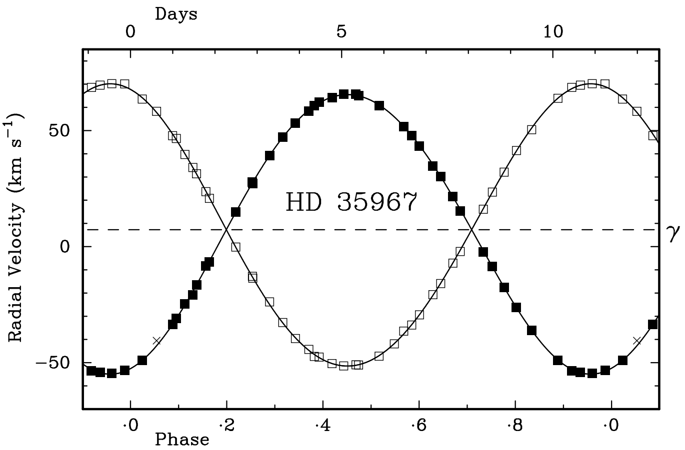

FIG. 7

As Fig. 2, but for HD 35967. A cross represents a rejected observation of the primary star.

velocity showed it *not* to be a member of the Hyades. (Unlike a single-lined binary, a double-lined one, especially one with equal components, gives a good estimate of the γ-velocity from a single observation.) Within three weeks he had a preliminary orbit and already knew the period of about 11·4 days to within less than an hour. Now there are 41 observations, listed in Table III, to support the derivation of the orbit. One observation, whose velocities are noted with colons and are half-weighted in the orbit, was curtailed by the arrival of an overcast, and in one instance the radial-velocity trace was corrupted in the vicinity of the primary dip, whose result has been rejected. Otherwise all observations of both components were given the same weight in the solution of the orbit, whose elements are given below and which is portrayed in Fig. 7.

P	= 11·39072 ± 0·00003 days	T_{110}	= MJD 55315·26 ± 0·06
γ	= +7·30 ± 0·05 km s^{-1}	$a_1 \sin i$	= 9·428 ± 0·016 Gm
K_1	= 60·23 ± 0·10 km s^{-1}	$a_2 \sin i$	= 9·518 ± 0·016 Gm
K_2	= 60·81 ± 0·10 km s^{-1}	$f(m_1)$	= 0·2579 ± 0·0013 $M_\odot$
q	= 1·0096 ± 0·0024 (= m_1/m_2)	$f(m_2)$	= 0·2655 ± 0·0013 $M_\odot$
e	= 0·0352 ± 0·0012	$m_1 \sin^3 i$	= 1·052 ± 0·004 $M_\odot$
ω	= 196·5 ± 1·9 degrees	$m_2 \sin^3 i$	= 1·042 ± 0·004 $M_\odot$

R.m.s. residual = 0·43 km s^{-1}

The true period, in the rest-frame of the system, is 11·39044 ± 0·00003 days.
It differs from P by about 9σ.

The orbit is seen to have a very small eccentricity (which is, however, nearly thirty times its own standard deviation and so is *very* definitely non-zero). The proximity of the orbit to a circle causes the time and position of the periastron point to be rather uncertain, although the uncertainty of T does not by any means imply a corresponding uncertainty in the placing of the velocity curve along the time axis. In such cases it is useful to give the epoch T_0, the time of maximum velocity of recession, which in this case is MJD 55309·0414 ± 0·0021. It is seen to be 6·22 days earlier than T, equivalent to 0·546 of an orbital cycle or 196° of phase, in agreement with the value of ω yielded by the eccentric solution in the informal table.

The minimum masses of a little over 1 $M_{\odot}$ are close to the actual masses expected of F8/G0 stars, so the value of $\sin^3 i$ (the factor multiplying the masses in the table above) cannot be expected to be less than about 0·9. The corresponding minimum orbital inclination is about 75°; the maximum is about 86°·5 — the limit above which there would have to be eclipses, which have not been recognized in HD 35967.

The mean equivalent widths of the dips in *Coravel* traces of HD 35967 are 1·77 and 1·65 km s^{-1} for the primary and secondary, respectively; the standard error in each case is less than 0·03, so the difference in the dips is significant. It corresponds arithmetically to a magnitude difference of about 0^{m}·07, and thence, through the 'rule of thumb' multiplication by 1·15 mentioned in the section above on HD 25788, to a ΔV of about 0^{m}·09. That translates to scarcely half a sub-type in spectral type. The mass ratio differs from unity by about 4σ, which again is clearly significant, but the difference is equivalent only to about a third of a sub-type. Unlike the case of HD 32704, this time the twin criteria of dip area and mass agree not only qualitatively (as to which component is the primary) but also quantitatively. For practical purposes both components could be assigned the same type, whether it be F8 or G0 as briefly discussed above. The dips are slightly wider than those seen for many other stars, to extents that are quantified in their means at $v \sin i$ values of 4·8 ± 0·5 km s^{-1} for the primary and 4·8 ± 0·4 km s^{-1} for the secondary. With $\sin i$ constrained to be between 0·965 and 0·998 according to the indications reviewed above, the actual equatorial velocities must be practically the same as the observed projected ones. Rotation of a star having a radius of 1·1 $R_{\odot}$ in HD 35967's orbital period of 11·4 days would require an equatorial velocity of 4·9 km s^{-1}, so it seems extremely likely that the rotations of both components of the system are indeed synchronized to the orbital revolution.

HD 45191 (V455 Aurigae)

HD 45191 is one of many stars that were discovered by *Hipparcos* to be hitherto-unrecognized photometric variables. It is a seventh-magnitude object, about 5° south-following δ Aur, in the second-most-northerly of the *four*(!) north-following corners of Auriga. It exhibits almost daily eclipses about 0^{m}·3 deep, yet had escaped detection as a variable star previously. Despite its brightness, it has been virtually ignored in the literature even since *Hipparcos* drew attention to it, and seems not to have any ground-based *UBV* photometry nor any MK classification. *Simbad* records the *Tycho 2* photometry as giving $V = 7^{m}{\cdot}28$, $(B - V) = 0^{m}{\cdot}43$; it may be hoped that those are intended to be out-of-eclipse values rather than mean ones. The *HD* type is F2, but the colour index would be more consonant with F5; the parallax[6] equates to a distance modulus of 4^{m}·40 ± 0^{m}·10 and thus indicates the absolute magnitude of the system to be +2^{m}·88, with much the same uncertainty of just a tenth of a magnitude. The principal luminosity of the system comes from two stars of mutually comparable brightness, which would need to have absolute magnitudes close to 3^{m}·6 each; the corresponding main-sequence type[25] is F6.

In a praiseworthy feat of organization, the *Hipparcos* team collaborated with the IAU variable-star authorities in Moscow to bring out a catalogue[26] of more than 3000 new variables already properly named with their final designations, in conformity with the *GCVS*[27]. One of them was HD 45191, re-christened by *Hipparcos* as its no. 30878 and by Moscow as V455 Aur, and classified as being

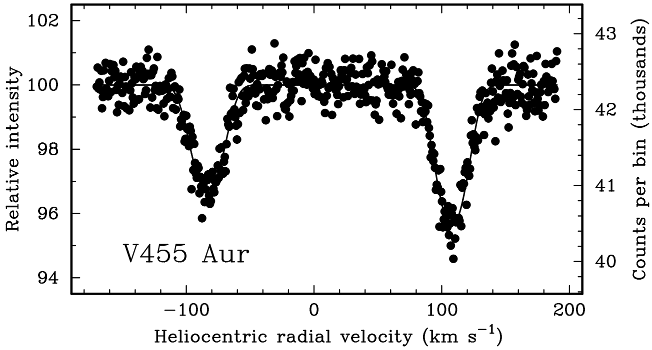

FIG. 8

Radial-velocity trace of HD 45191 (V455 Aurigae), obtained with the Cambridge *Coravel* on 2011 December 10.

of type 'EA' (eclipsing variable of Algol type). Its period was not determinable from the *Hipparcos* observations, whose distribution in time was by no means ideal for that purpose (and had of course not been designed for it — even the photometry itself was only a by-product, albeit an extraordinarily valuable one, of the satellite's principal astrometric purpose). The *Hipparcos* photometry is plotted in Vol. **12** of the *Hipparcos Catalogue*[28], p. C44; it shows that just two eclipse minima, about 17·26 days (now recognized to be 5½ cycles) apart, were (partially) observed. There is one other low point, far removed in time and plotted with a symbol that identifies it as one that is suspected of being erroneous.

Radial-velocity observations of the star were begun soon after the Cambridge *Coravel* was operational; the system was almost immediately found to be double-lined, giving two weak and wide 'dips' that exhibited velocity changes having generous amplitudes of about 100 km s^{-1} apiece. Fig. 8 shows a trace obtained near a node of the orbit. The orbital period (remarkably close to the quantity π when expressed in days) was initially determined from a mere seven observations obtained in the first observing season (2000 February/March). It was a surprise to find, in the ensuing season, that there had been a substantial alteration in the γ-velocity, and by the time that the Cambridge work on the orbit was written up (very promptly — it was already in print[9] in 2001) the γ-velocity had changed by as much as 10 km s^{-1}.

To deal with such a system, consisting of a double-lined binary with a varying γ-velocity that implied that the observed pair was in an orbit of much longer period opposite an unseen third component, a new orbit-solving programme was written, because no such system had been encountered by the writer previously. There was no possibility, at that time, of determining the nature of the 'outer' orbit — the one responsible for the variation in the γ-velocity — so it was necessary to adopt an approximation which fairly represented that variation as far as it had then been witnessed*. A circular orbit performed that function quite satisfactorily; it had a period of 1350 days and an amplitude of 8·3 km s^{-1}. The first season's observations covered a negligible fraction of the period and appeared as just a single clutch of points nearly halfway down the declining 'branch' of the sine wave representing the velocity curve of the adopted outer

*That is exactly analogous to the proposal, in this present paper, for HD 25788.

orbit, while the second season's observations, which covered about ⅛ of the supposed period and seemed to show a flattening-off of the drift in the γ-velocity towards the end of the season, appeared nearly at the minimum of the curve.

Of course the observer did not wish to leave a question-mark hanging over the actual nature of the outer orbit for longer than could be helped, so V455 Aur (like other triple systems whose inner orbits (only) have been published in this series of papers*) was retained on the observing programme. There was no reason even to *hope* that the orbit adopted in the paper[9] would have any predictive power — it was nothing more than a dodge that enabled the drift of the γ-velocity to be mimicked computationally for just the intervals within which it had been observed, exactly as has been suggested (though not implemented) above in the present paper for HD 25788. Even so, it was a bit disconcerting that for several ensuing seasons there appeared to be little, if any, further drift. Eventually, however, the γ-velocity began definitely to rise again, slowly at first and then not so slowly, although still on a very leisurely time-scale in comparison with that of the inner orbit, and during the last two years there have recurred the phases in the outer orbit that were traversed in 2000 and 2001. It is now possible to *solve* the observed velocities for the elements of the outer (as well as the inner) orbit, instead of merely imposing a set of elements that represent the outer orbit well enough to reproduce the observed change of the γ-velocity over a limited interval of time.

The 123 radial-velocity observations are set out in Table IV. It gives the twin velocities from each observation†, and then the computed phase in the outer orbit and the contribution made by that orbit to the radial velocities of both the observed stars; next, there is the phase and the respective contributions of the inner orbit to the velocities of each star individually, and finally the two residuals. The first two columns, as is usual in such tables in this series of papers, give the civil and Modified Julian dates of the times of observation, but in this case there is an important distinction between them. The civil date (the short period and large amplitude of the inner orbit require it to be specified to three decimal places rather than the usual two) is not the actual time of observation as shown by the observer's clock but is corrected to the Sun; that is in fact routine in these papers, but when the dates are given only to two decimals the distinction between topocentric and heliocentric times (never greater than 6 thousandths of a day) is only academic. The MJD is, however, additionally corrected in Table IV to the centre of gravity of the whole triple system, as a necessary step in the calculation that leads to the orbital elements. The peak-to peak correction to the dates for the motion of the observed pair of stars in their long-period orbit amounts to 13 thousandths of a day or about 19 minutes. As explained at greater length in Paper 160[9], in the time that it takes light to travel between here and the Sun, and the longer time that it takes to cross the outer orbit in the star system, the velocities of the stars in their short-period inner orbit can change very significantly. Quoting from Paper 160, "Near conjunctions the rate of [change] reaches K (the velocity amplitude) per radian of phase in the orbit, so in the presently discussed orbit of period π days the rate is exactly $2K$ — about 200 km s^{-1} — per day or about 1 km s^{-1} in 7 minutes." Neglect of the light-time correction would needlessly introduce errors which would sometimes be about equal to the characteristic standard deviation of an observation. The calculation

*1 Gem (Paper 10); HD 7426 (Paper 80); HR 965 (Paper 88); HD 141690 (Paper 110); HR 2879 B (Paper 119); 24 Aqr (Paper 128); HD 14415 and HR 3112 (Paper 189); HD 152109 (Paper 216); HR 396 (Paper 223).

†Except for three blends, two of which are treated as measures of each component, and two cases where only one component was measured.

TABLE IV

Cambridge radial-velocity observations of HD 45191 (V455 Aur)

Heliocentric Date		*HMJD* (*light-time corrected*)	*Velocity* Prim. km s⁻¹	Sec. km s⁻¹	*Outer orbit* Phase	Velocity km s⁻¹	*Inner orbit* Phase	Vel (P) km s⁻¹	Vel (S) km s⁻¹	*(O–C)* Prim. km s⁻¹	Sec. km s⁻¹
2000 Feb.	22·003	51596·004	+16·3		0·979	+17·5					
	25·995	599·996	−78·1	+116·2	·980	17·5	0·158	−95·4	+99·8	−0·3	−1·1
	28·894	602·895	−74·2	+115·6	·980	17·5	1·080	−92·0	+96·3	+0·3	+1·8
	28·966	602·967	—	+115·6	·980	17·5	·103	—	+99·9	—	−1·8
Mar.	1·889	51604·890	+102·8	−69·7	0·981	17·6	1·714	+84·1	−88·0	+1·1	+0·7
	3·929	606·930	+5·1	+31·1	·981	17·6	2·363	−11·2	+11·7	−1·3	+1·8
	4·949	607·950	+108·7	−75·4	·981	17·6	·687	+90·6	−94·8	+0·5	+1·8
Sept.	25·183	812·180	+105·3	−88·7	1·030	10·4	67·609	+94·4	−98·8	+0·5	−0·3
	27·201	814·198	−59·8	+83·2	·031	10·3	68·251	−70·3	+73·5	+0·2	−0·6
Oct.	6·203	823·200	−85·6	+113·0	·033	10·1	71·112	−96·3	+100·7	+0·6	+2·2
	9·175	826·172	−75·8	+99·0	·033	10·0	72·057	−86·6	+90·6	+0·8	−1·6
	17·197	834·194	+103·1	−89·1	·035	9·8	74·607	+94·2	−98·6	−0·9	−0·3
Nov.	3·055	851·052	−38·2	+57·2	·039	9·4	79·966	−48·5	+50·8	+0·9	−3·0
	4·178	852·175	−23·3	+43·3	·040	9·4	80·323	−34·2	+35·8	+1·5	−1·9
	13·215	861·212	−78·3	+100·2	·042	9·3	83·196	−88·7	+92·8	+1·1	−1·9
	15·142	863·139	+52·6	−36·7	·042	9·2	·808	+44·2	−46·2	−0·8	+0·3
	17·127	865·124	+42·6	−24·3	·043	9·2	84·439	+33·7	−35·2	−0·3	+1·7
	20·241	868·238	+36·9	−19·3	·043	9·1	85·429	+28·0	−29·3	−0·3	+0·9
	30·183	878·180	+99·4	−88·3	·045	9·0	88·590	+91·9	−96·1	−1·5	−1·2
Dec.	2·212	880·209	−66·8	+88·9	·046	9·0	89·235	−76·6	80·1	+0·8	−0·2
	3·108	881·105	+81·5	−64·5	·046	9·0	·519	+72·2	−75·6	+0·3	+2·1
	9·173	887·170	+46·5	−29·8	·048	8·9	91·447	+38·2	−39·9	−0·5	+1·3
	14·177	892·174	−70·0	+93·3	·049	8·8	93·038	−80·8	+84·5	+2·0	0·0
	22·088	900·085	+94·1	−76·7	·051	8·7	95·553	+83·6	−87·4	+1·8	+2·0
	28·027	906·024	+41·8	−26·0	·052	8·7	97·441	+34·6	−36·2	−1·4	+1·5
	28·994	906·991	+80·7	−66·4	·053	8·6	·748	+72·5	−75·8	−0·4	+0·8
	29·909	907·906	−71·6	+93·3	·053	8·6	98·039	−81·1	+84·9	+0·9	−0·2
2001 Jan.	8·992	51917·989	−63·7	+83·6	1·055	8·5	101·244	−72·8	+76·2	+0·6	−1·1
	9·970	918·967	+93·5	−81·3	·055	8·5	·555	+84·3	−88·1	+0·7	−1·7
	11·128	920·125	−13·1	+34·9	·056	8·5	·923	−24·3	+25·4	+2·7	+0·9
	13·966	922·963	+40·5	−27·3	·056	8·5	102·826	+34·6	−36·2	−2·6	+0·4
	15·999	924·996	+58·9	−44·1	·057	8·5	103·472	+50·9	−53·3	−0·5	+0·7
	16·118	925·115	+76·2	−64·0	·057	8·5	·510	+68·3	−71·5	−0·6	−1·0
	16·940	925·937	+70·3	−57·7	·057	8·5	·771	+62·8	−65·7	−1·0	−0·4
	31·950	940·947	+91·3	−77·7	·061	8·3	108·543	+80·4	−84·1	+2·6	−1·9
Feb.	12·930	952·928	−9·0	+26·3	·064	8·3	112·351	−18·1	+19·0	+0·9	−0·9
	14·978	954·976	−59·5	+76·1	·064	8·2	113·002	−66·3	+69·4	−1·4	−1·5
	17·012	957·010	+103·9	−92·1	·064	8·2	·648	+95·3	−99·7	+0·4	−0·6
	22·028	962·026	−65·9	+84·9	·066	8·2	115·243	−73·4	+76·8	−0·7	−0·1
Apr.	28·866	52027·864	−86·7	+106·0	·081	7·9	136·172	−93·5	+97·8	−1·1	+0·3
May	4·860	033·858	−83·5	+104·0	·083	7·9	138·078	−91·6	+95·8	+0·2	+0·3
	7·856	036·854	−69·4	+91·1	·083	7·9	139·030	−77·9	+81·5	+0·6	+1·8
Aug.	21·149	142·148	+75·1	−60·5	·109	7·7	172·501	+64·8	−67·8	+2·6	−0·4
Nov.	1·118	214·117	+7·8		·126	7·6	195·380	−1·0	+1·1	+1·2	−0·9
	14·069	227·068	+70·3	−59·9	·129	7·6	199·497	+62·7	−65·6	0·0	−1·9
	27·092	240·092	+101·7	−93·3	·132	7·6	203·636	+95·6	−100·0	−1·5	−0·9
Dec.	22·140	265·140	+100·6	−91·4	·138	7·6	211·599	+93·3	−97·6	−0·3	−1·4
2002 Jan.	1·006	52275·006	+84·6	−73·6	1·140	7·6	214·735	+77·4	−80·9	−0·4	−0·3
Feb.	14·936	319·936	−67·4	+83·9	·151	7·6	229·018	−73·2	+76·6	−1·8	−0·3
Mar.	26·899	359·899	+90·1	−77·9	·160	7·6	241·722	+81·8	−85·6	+0·6	+0·1
Oct.	18·198	565·200	−52·7	+68·7	·209	7·7	306·984	−57·9	+60·6	−2·5	+0·4
Dec.	5·068	613·070	−79·0	+99·3	·221	+7·8	322·202	−87·2	+91·2	+0·4	+0·3

TABLE IV (*continued*)

Heliocentric Date	*HMJD* (*light-time corrected*)	*Velocity* *Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$	*Outer orbit* *Phase*	*Velocity* *km s*$^{-1}$	*Inner orbit* *Phase*	*Vel (P)* *km s*$^{-1}$	*Vel (S)* *km s*$^{-1}$	*(O – C)* *Prim.* *km s*$^{-1}$	*Sec.* *km s*$^{-1}$
2003 Jan. 5·113	52644·116	−82·8	+101·5	1·228	+7·8	332·070	−90·0	+94·2	−0·6	−0·5
Feb. 18·014	688·017	−69·3	+88·9	·238	7·8	346·026	−76·4	+79·9	−0·7	+1·2
Mar. 15·939	713·942	−55·3	+72·8	·245	7·8	354·267	−62·9	+65·9	−0·2	−0·9
27·879	725·882	−82·1	+101·0	·247	7·9	358·063	−88·2	+92·3	−1·8	+0·9
Apr. 7·884	736·887	+94·5	−82·3	·250	7·9	361·561	+85·9	−89·8	+0·8	−0·3
Oct. 18·192	930·197	−62·0	+80·1	·296	8·0	423·012	−70·6	+73·9	+0·6	−1·8
Nov. 4·157	947·162	+24·4	−6·0	·300	8·0	428·405	+13·8	−14·5	+2·5	+0·4
Dec. 8·068	981·073	−84·2	+104·5	·308	8·1	439·185	−91·2	+95·4	−1·1	+1·0
16·129	989·134	+82·2	−68·2	·310	8·1	441·747	+72·9	−76·3	+1·2	0·0
29·152	53002·157	+9·6		·313	8·1	445·887	−2·1	+2·2	+3·7	−0·7
2004 Jan. 17·114	53021·119	−10·2	+28·4	1·318	8·1	451·915	−19·1	+20·0	+0·8	+0·3
30·065	034·070	−68·6	+91·8	·321	8·1	456·032	−78·5	+82·2	+1·8	+1·5
Feb. 25·910	060·915	+95·9	−83·0	·327	8·1	464·566	+86·9	−91·0	+0·8	−0·2
Apr. 13·890	108·896	+48·5	−31·1	·338	8·2	479·818	+39·0	−40·8	+1·3	+1·5
Sept. 21·196	269·203	+67·8	−56·7	·377	8·3	530·777	+59·9	−62·7	−0·5	−2·3
Oct. 27·175	305·182	−75·6	+95·1	·385	8·4	542·215	−83·4	+87·3	−0·5	−0·6
Dec. 26·099	365·106	−56·4	+76·2	·399	8·4	561·264	−64·6	+67·6	−0·3	+0·2
2005 Jan. 31·001	53401·008	+102·4	−88·1	1·408	8·5	572·677	+92·4	−96·7	+1·5	+0·1
Feb. 12·976	413·983	+56·9	−41·4	·411	8·5	576·801	+48·0	−50·2	+0·4	+0·3
Mar. 24·942	453·949	+74·7	−60·9	·420	8·5	589·506	+66·7	−69·8	−0·5	+0·4
Nov. 5·151	679·159	−86·1	+107·8	·474	8·8	661·097	−94·8	+99·2	−0·1	−0·2
Dec. 28·064	732·073	−9·6	+31·3	·487	8·8	677·918	−20·8	+21·7	+2·3	+0·7
2006 Feb. 17·973	53783·982	+31·6	−13·0	1·499	8·9	694·419	+22·0	−23·1	+1·2	+0·7
May 11·884	866·893	+69·9	−57·2	·519	9·0	720·775	+60·9	−63·7	0·0	−2·5
Oct. 5·137	013·146	−52·8	+76·8	·553	9·2	767·267	−63·0	+65·9	+1·0	+1·7
Nov. 2·196	041·206	−82·7	+102·8	·560	9·2	776·187	−90·7	+94·9	−1·2	−1·4
Dec. 17·059	086·069	+46·8	−30·4	·571	9·3	790·448	+38·6	−40·3	−1·0	+0·7
2007 Feb. 2·942	54133·952	+103·4	−90·5	1·582	9·3	805·670	+93·4	−97·7	+0·7	−2·1
Apr. 4·903	194·913	−73·9	+100·4	·597	9·4	825·048	−84·1	+88·0	+0·8	+3·0
Nov. 16·163	420·173	+103·4	−91·0	·650	9·8	896·656	+94·8	−99·2	−1·2	−1·5
2008 Jan. 28·005	54493·015	+52·6	−35·0	1·668	9·9	919·811	+42·7	−44·6	0·0	−0·2
Mar. 31·900	556·910	−87·6	+109·6	·683	10·0	940·122	−96·8	+101·3	−0·8	−1·7
May 6·889	592·899	+96·5	−82·5	·691	10·1	951·563	+86·3	−90·3	+0·2	−2·3
Oct. 19·183	758·193	−85·3	+109·7	·731	10·4	1004·108	−95·9	+100·4	+0·2	−1·1
Dec. 27·045	827·055	−54·0	+78·7	·747	10·5	1025·998	−64·5	+67·5	0·0	+0·7
2009 Mar. 23·957	54913·967	+105·3	—	1·768	10·8	1053·626	+95·5	—	−0·9	—
May 8·869	54959·879	−69·6	+96·2	1·779	10·9	1068·221	−81·4	+85·2	+1·0	+0·1
Oct. 25·214	55129·223	−73·4	+99·7	·819	11·4	1122·053	−85·5	+89·5	+0·7	−1·2
Dec. 21·048	186·057	−85·0	+114·1	·832	11·6	1140·120	−96·7	+101·2	+0·1	+1·3
2010 Jan. 30·038	55226·046	+43·7	−18·5	1·842	11·8	1152·832	+30·9	−32·4	+1·0	+2·1
Mar. 22·926	277·934	−18·6	+42·8	·854	12·0	1169·326	−32·2	+33·7	+1·6	−2·9
Apr. 16·858	302·866	−57·4	+83·9	·860	12·1	1177·252	−69·7	+72·9	+0·2	−1·2
Oct. 20·221	489·227	+75·0	−53·1	·904	13·3	1236·494	+61·4	−64·2	+0·3	−2·2
Nov. 28·152	528·158	+24·5	+3·2	·914	13·6	1248·869	+8·7	−9·1	+2·1	−1·3
Dec. 9·180	539·186	+7·4	+18·2	·916	13·7	1252·375	−3·9	+4·1	−2·4	+0·4
19·127	549·132	+92·7	−67·6	·919	+13·8	1255·537	+78·5	−82·2	+0·4	+0·8

TABLE IV (*concluded*)

Heliocentric Date	*HMJD (light-time corrected)*	*Velocity* Prim. km s⁻¹	*Velocity* Sec. km s⁻¹	*Outer orbit* Phase	*Outer orbit* Velocity km s⁻¹	*Inner orbit* Phase	*Inner orbit* Vel (P) km s⁻¹	*Inner orbit* Vel (S) km s⁻¹	*(O–C)* Prim. km s⁻¹	*(O–C)* Sec. km s⁻¹
2011 Jan. 10·011	55571·016	+76·3	−51·9	1·924	+14·0	1262·493	+61·2	−64·0	+1·1	−1·9
Feb. 8·094	600·099	+90·2	−64·8	·931	14·3	1271·738	+76·3	−79·8	−0·4	+0·6
Mar. 8·814	628·818	+27·8	+3·9	·938	14·7	1280·868	+9·5	−10·0	+3·6	−0·8
Apr. 6·836	657·840	−78·0	+113·3	·945	15·1	1290·093	−94·3	+98·7	+1·2	−0·5
May 2·868	683·871	+4·6	+22·6	·951	15·5	1298·369	−7·6	+8·0	−3·2	−0·9
10·877	691·880	−3·6	+35·5	·953	15·6	1300·914	−18·9	+19·8	−0·3	+0·1
Oct. 1·155	835·155	+61·2	−27·8	·987	17·7	1346·460	+44·7	−46·7	−1·2	1·2
Nov. 18·133	883·132	+102·0	−73·4	1·998	16·8	1361·711	+85·1	−89·0	+0·2	−1·2
Dec. 5·204	900·202	−81·5	+114·4	2·002	16·0	1367·137	−96·8	+101·3	−0·6	−2·9
10·017	905·015	+109·6	−81·8	·003	15·7	1368·667	+93·7	−98·0	+0·2	+0·5
18·030	913·028	−67·3	+101·6	·005	15·3	1371·214	−83·5	+87·3	+0·9	−1·0
2012 Jan. 4·050	55930·048	+109·1	−85·6	2·009	14·3	1376·625	+95·4	−99·9	−0·6	0·0
12·991	938·989	+63·7	−36·5	·011	13·7	1379·467	+48·5	−50·7	+1·5	+0·5
27·011	953·009	−11·1	+40·4	·015	13·0	1383·924	−24·5	+25·6	+0·4	+1·8
Feb. 4·001	960·998	+58·9	−36·6	·017	12·6	1386·464	+46·7	−48·9	−0·4	−0·3
18·875	975·872	−79·1	+106·4	·020	11·9	1391·192	−89·6	+93·8	−1·3	+0·8
Mar. 1·875	987·872	−55·1	+82·1	·023	11·4	1395·006	−68·3	+71·5	+1·9	−0·8
11·956	997·953	−72·6	+99·3	·025	11·0	1398·211	−84·5	+88·4	+0·9	−0·1
Apr. 5·895	56022·892	−87·0	+112·5	·031	10·2	1406·139	−96·8	+101·3	−0·4	+1·1
15·865	032·862	−32·2	+52·6	·034	9·9	1409·308	−42·4	+44·4	+0·3	−1·7
May 6·891	053·888	−52·0	+71·6	·039	9·5	1415·992	−61·7	+64·6	+0·2	−2·5
Aug. 31·173	170·171	−35·4	+52·9	·066	8·2	1452·957	−43·5	+45·5	−0·1	−0·8
Nov. 3·189	234·187	−34·8	+53·0	·082	7·9	1473·307	−43·2	+45·2	+0·5	−0·1
Dec. 2·079	263·077	+68·7	−55·5	·089	7·8	1482·490	+59·9	−62·6	+1·0	−0·7
2013 Jan. 2·009	56294·007	−26·0	+41·4	2·096	+7·8	1492·323	−34·3	+35·9	+0·6	−2·3

of the orbits of the triple system, therefore, iterates itself so as to correct the time assigned to each observation to the time that it *would* have had *if* the short-period binary had been then at the same distance from us as the overall centroid of the triple system. The introduction of the timing corrections for the motion of the short-period system in the outer orbit, which are determinable only after the solution has run initially with the 'raw' timings (corrected only to the Sun), reduces the sum of squares of the residuals of the 242 available velocities from 441 to 383 (km s^{-1})^2 — a very significant improvement.

Although there is a distinct difference in the strengths of the two dips in the observed traces (as illustrated in Fig. 8), yet the r.m.s. radial-velocity residuals of the two components differ by only 6% when all 242 observations are given equal weights, so we have been content to leave them so, although in Paper 160 the secondary's velocities were weighted ¾. The inner orbit has the very small eccentricity of 0·01 — less than that of the Earth's orbit around the Sun — but it is very definite, being 8σ, and no doubt is maintained by the unseen remote component in the system, by the mechanism discussed by Mazeh[29] (first proposed by Mazeh & Shaham[30]). The computed orbital elements are set out in the informal table below, while the outer and inner orbits, respectively, are shown in Figs. 9 and 10. It will be appreciated that each point in each of those diagrams shows the relevant observed velocity *less* the velocity attributed to motion in the *other* orbit according to the tabulated orbital elements. The residual of any given observation is thus the same in both diagrams (but it *looks* far worse in Fig. 9, whose vertical scale is 14 times that of Fig. 10!).

Element		*Outer Orbit*	*Inner Orbit*
P	(days)	4205 ± 17	3·1457741 ± 0·0000012
T	(MJD)	51686 ± 20	53524·71* ± 0·06
Γ	(km s^{-1})	+9·93 ± 0·11	
K_1	(km s^{-1})	5·05 ± 0·20	96·27 ± 0·16
K_2	(km s^{-1})		100·73 ± 0·23
q	(= m_1/m_2)		1·0463 ± 0·0029
e		0·726 ± 0·017	0·0099 ± 0·0012
ω	(degrees)	41·8 ± 2·7	132 ± 7
$a_1 \sin i$	(Gm)	201 ± 10	4·164 ± 0·007
$a_2 \sin i$	(Gm)		4·357 ± 0·010
$f(m_1)$	($M_\odot$)	0·0182 ± 0·0026	
$m_1 \sin^3 i$	($M_\odot$)		1·277 ± 0·007
$m_2 \sin^3 i$	($M_\odot$)		1·220 ± 0·005
P_{true}	(days)		3·1456700 ± 0·0000016

Difference between observed and true periods for inner orbit = 64 s.d.s
R.m.s. residual (unit weight) = 1·26 km s^{-1}

*See the paragraph immediately following the previous informal table (for HD 35967): in the case of HD 45191, T_0 = MJD 53523·5545 ± 0·0007

The real outer orbit, whose elements have just now been determined, is very different from the 1350-day circular one that was adopted in Paper 160 to represent, as nearly as could be judged at that time, the apparent trend of the γ-velocity of the inner binary. Even so, the velocities given by the circular one at the actual times of the 41 observations set out in the earlier paper differ little from the corresponding entries, which show the computed contribution of

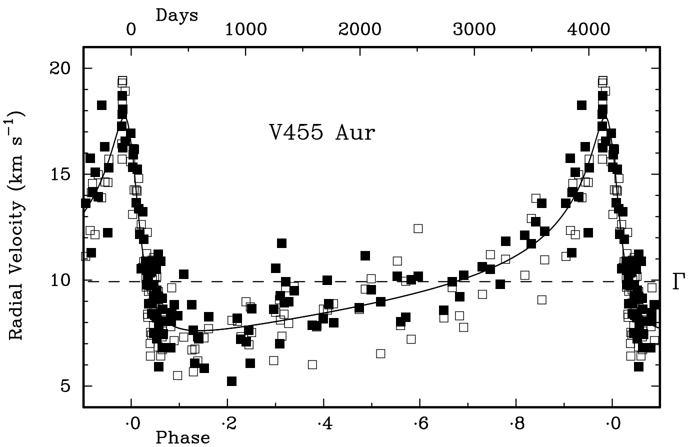

FIG. 9

Radial velocities of the components of V455 Aurigae in the outer orbit, opposite an unseen companion. The line represents the motion of the γ-velocity of the short-period system according to the adopted orbital elements. This diagram is analogous to Fig. 3, except inasmuch as it covers the complete cycle of the long-period orbit, which is accordingly quite well determined.

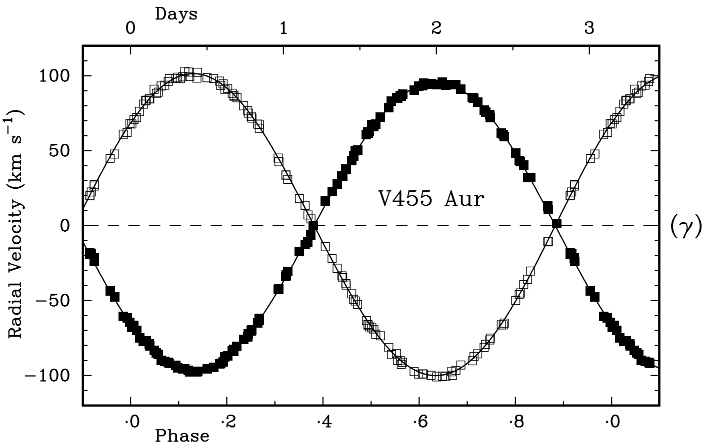

FIG. 10
As Fig. 2, but showing the inner orbit of HD 45191 (V455 Aurigae).

the outer orbit to the observed radial velocity, in the sixth column of Table IV here. More than half of all the entries in that column are either identical with the corresponding ones in the analogous table in Paper 160 or differ by only 0·1 km s^{-1}, and about half of the remainder differ by just 0·2. It is only at the ends of the earlier table that larger discrepancies occur — the worst one, of 0·8 km s^{-1}, occurs for the very first observation, which being of a blend does not enter into the orbital solutions in any case. That discrepancy right at the start of the table in Paper 160 arose because the gradient of change during the very short interval (12 days) of the first season's observations was indeterminate, and was supposed in the adopted circular orbit to be much the same as at the start of the second season — in the diagram corresponding to Fig. 9 here, the first season is shown as being nearly halfway down the descending side of the sine-wave velocity curve, whereas we now know that it happened to be right on the peak of the true curve, so in fact there was virtually no variation in the contribution from the outer orbit during that short season.

A comparison of the elements of the *inner* orbit between the ones given here and those in Paper 160 is very satisfactory — they are all within their standard errors apart from K_2, which is marginally outside 1σ. There is an infelicity (less-kindly commentators might call it a mistake!) in the number given in Paper 160 for T, notwithstanding that there is a paragraph (analogous to, but more detailed than, the one immediately following the informal table of elements for HD 35967, above, in *this* paper) explaining the difference in the significances of T and T_0. Here, we rehearse the matter afresh. T is the epoch of periastron, which obviously is poorly determined in an orbit of low eccentricity, whereas T_0, the epoch of maximum velocity of recession, is much more accurate and may be thought of as defining the positioning of the velocity curve left-and-right along the time axis of the graph. Unfortunately the T value given in Paper 160 was of a hybrid character: it was indeed T, but was assigned the standard deviation that belonged to T_0, whose value of 0·0011 days was actually explained in the paper*.

The areas of the dips seen in radial-velocity traces are 1·21 km s^{-1} for the primary and 1·00 for the secondary; the standard deviations of the means are about 0·015 km s^{-1}. The ratio of 1·21 is equivalent to a difference of 0^{m}·21 if expressed in stellar magnitudes. According to the rule of thumb[1] mentioned in the section on HD 25788 above, the difference in V magnitudes is larger by a factor of 1·15 than the number given by the ratio of dip areas, so in this case it would be 0^{m}·24. It corresponds to just over one spectral sub-type; judged purely from their absolute magnitudes, the stars could be expected to have types of about F5 and F6, or F6 and F7. The agreement with the colour index of the total system is satisfactory. The formal mean $v \sin i$ values are 17·98 ± 0·17 and 18·77 ± 0·29 km s^{-1} for the primary and secondary, respectively. Of course the absolute values do not merit the decimal parts that they have been accorded there, but the decimals might be expected to be relevant to the relative values, which differ by 0·79 ± 0·33 km s^{-1} in the unexpected sense. Thus the star that is certainly the more massive (the q value differs from unity by 16σ) and is almost certainly the more luminous one, having a dip signature that is nearly 10σ larger than its companion's, has distinctly (by 'only' about 2·4σ, but nevertheless P > 99%) the narrower dip, implying the smaller rotational velocity. It is, moreover, an annoying fact that has long been noticed in many of the individual radial-velocity traces that the weaker dip tends to be slightly wider than the stronger one. The rotational velocity of a star with a radius of 1·15 $R_\odot$, such as the components of V455 Aur may be expected to have, with a rotation period equal to the inner orbital period of about π days, is 18·5 km s^{-1}, which appears to make it practically certain that the rotations of the two stars must be (pseudo-) synchronized to the orbital revolution. So the brighter and more massive one, it seems, is slightly the smaller of the two. As an observer, however, the writer feels that his principal responsibility is to tell how things are, as matters of fact, and not necessarily to explain them!

At V455 Aur's distance of about 76 pc, the components' separation of about 8·5 Gm subtends considerably less than one millisecond of arc. The orbit of the close pair around the centre of gravity of the system has a mean radius of about 1·34/sin i AU; if it were presented perpendicularly to the line of sight it would subtend an angle of about 0″·017/sin i. The unseen tertiary must be a good deal fainter and less massive than either of the observed components, otherwise it would feature in the radial-velocity traces, but the mass function of the outer orbit shows that it cannot have a mass smaller than 0·5 $M_\odot$ — so it cannot be an M dwarf but is in all probability of type K, as a G star would be bright enough to contribute a perceptible signature in the radial-velocity traces. Having, therefore, about one-third to one-quarter the mass of the close pair, it must have a separation from that binary that is four or five times the distance of the latter from the centre of gravity of the system, say about 6/sin i AU. Owing to the high eccentricity of the outer orbit, for much of the time the system languishes

*One can appreciate exactly how it comes to have the value that it does — it is, obviously, a timing uncertainty, and is approximately the time taken for the stellar velocity to change by one typical standard deviation, divided by the square root of the number of observations. Now, the number of observations has increased by a factor of about 3, and the standard error of T_0 has therefore obediently diminished by about $\sqrt{3}$, from 0·0011 to 0·0007 days — about 95 to 60 seconds.

near apastron, where the distance is nearly doubled again, so it could be up to as much as 10/sin i AU or so. The maximum observable angular separation is subject to uncertainties related to the inclination of the outer orbit. 10 AU would subtend 0″·13 if seen directly athwart the line of sight, but that maximum separation is subject to modification (though not by a large factor) by presently unquantified projection effects.

HD 213896 (*LL Aquarii*)

This object is to be found just one-fifth of the way from κ to γ Aqr. It is another system whose photometric variability was established by *Hipparcos*; it even features in a table of 'new' eclipsing variables in the *Hipparcos* chief's (Perryman's) 'popular' description[31] of the project in *Sky & Telescope* some time after the principal *Catalogue* was itself unveiled. The satellite's photometry of the object is plotted in Vol. **12** of that *Catalogue*[28], p. C156; as in the case of V455 Aur, it shows just two eclipses, this time about two years apart. A clearly erroneous minimum magnitude is noted beside the plot (and listed elsewhere in the *Catalogue*): two of the points in one of the eclipses are far below the alleged minimum. Reference to the rubric to be found in vol. **1**, §1.3.5 (p. 45) reveals that what is quoted as the minimum magnitude is actually the 95th percentile of all the photometric data. While that may well be a conservative method of assessing smoothly-varying light-curves, where it could be regarded as allowing something for the random errors of the photometry and even guarding against excessive reliance on what might be a rogue point, it seems nonsensical when applied blindly* to an eclipsing variable that naturally has a few outlying points that are perfectly plausible and in the case of LL Aqr include a sequence of four that clearly document the progress of an eclipse over an interval of about 2½ hours. The faintest point is more than $0^{m}\cdot 2$ below the tabled minimum, and shows the range of variation to be more than 50% greater than is admitted.

Although *Simbad* does not report any ground-based photometry of LL Aqr but gives *V* and *B* credited to *Tycho 2*, and shows the spectral type merely as G0, as given in the *HD*, actually there is a good lot of photometry; and (like almost all *HD* stars with declinations below +5°) LL Aqr also has a two-dimensional type, G1 V in its case, in the Michigan re-classification[15] of the *HD* stars by Houck and her collaborators. Although no period could be found for LL Aqr from the *Hipparcos* photometry alone, in 2004 Otero & Dubovsky[32] established the period as 20·1784 days by the use of numerous additional measurements from *ASAS–3* and *NSVS*. The former, the *All Sky Automated Survey*[33], used a 135-mm *f*/1·8 telephoto camera lens set up at Las Campanas, and surveyed the complete southern sky almost nightly. *NSVS*, the *Northern Sky Variability Survey*[34], was a rather analogous system located near Los Alamos and utilizing four 200-mm *f*/1·8 lenses on a single mount — the '*ROTSE–1*' (*Robotic Optical Transient Search Experiment*) telescope. The outcome of the investigation was a very well-observed light-curve, with an out-of-eclipse *V* magnitude of $9^{m}\cdot 21$ and eclipse depths of $0^{m}\cdot 65$ and $0^{m}\cdot 38$.

Moreover, there subsequently appeared another paper, by İbanoğlu *et al.*[35], that gives a comprehensive and altogether independent photometric coverage of LL Aqr and two other eclipsing systems throughout their orbital cycles, with of course particular emphasis on the vicinities of the eclipses. The observations were made 'by hand' with the 48-inch Cassegrain reflector at the Ege University Observatory in Turkey. The mean out-of-eclipse magnitudes were given as

*It isn't always! For example, for EP Ori (HIP 22535) the listed minimum brightness coincides with that of the faintest point.

$V = 9^{m}{\cdot}206$, $(B-V) = 0^{m}{\cdot}559$, $(U-B) = 0^{m}{\cdot}0{\cdot}085$. The primary and secondary eclipses were found to be respectively $0^{m}{\cdot}60$ and $0^{m}{\cdot}39$ deep in V; the system appears redder during the primary eclipse and bluer during the secondary one, showing that it is the primary (hotter) star that is being eclipsed during the primary eclipse.

The paper[35] also presents a small number of radial-velocity observations that permitted the derivation of the orbit. LL Aqr is the only one of the stars investigated in this paper that has had an orbit published for it previously by other authors. Ten measurements were made with the 91-cm reflector of the Catania Astrophysical Observatory in Italy; three of them were of blends and measured as single-lined, so they could not contribute to the solution of the orbit. There were just two others, obtained with the 1·5-m reflector at the TÜBİTAK National Observatory at Antalya, Turkey. *They* were measured as *triple*-lined, showing not only the two usual components but also a (probably illusory) third one in the vicinity of the γ-velocity. A very reasonable orbit was obtained on the basis of the nine double-lined observations. When the present writer tried to confirm the published elements from the same observations, they did not come out quite the same, but that may be because the authors[35] weighted their measurements individually. In their table of velocities, each is assigned its own "error"; if they were weighted inversely as the squares of those quantities their weights would span a range of almost 100 to 1, which seems scarcely reasonable since it is effectively rejecting some of the data. Although this writer's re-calculation from the same observations, giving all 18 of them identical weights, yielded slightly different elements, the r.m.s. residuals from the computed orbit came out *smaller* than the uncertainties claimed for all but one of the data points, so it looks as if the assignment of the errors to the individual points must itself have involved some error that is manifested as pessimism.

The Cambridge observations of LL Aqr began only in 2011 September, and now number 25; they are listed in Table V, after those of İbanoğlu *et al.*, which have received a zero-point adjustment of +1·4 km s^{-1}. An example is shown in Fig. 11, well illustrating the inequality of the two dips. In the solution of the orbit, it has proved appropriate to weight the Cambridge observations of the secondary ⅓ in comparison with those of the primary. The İbanoğlu *et al.* velocities have been weighted ⅕, for both components. So far from needing to have the secondary further down-weighted, they actually give smaller residuals for that star than for the primary; but that surely must be a statistical fluke — it would seem quixotic to give extra weight to the secondary's velocities. The orbital elements are given in the table here, and the orbit is portrayed in Fig. 12.

P	=	20·17814 ± 0·00028 days	$(T)_{75}$	=	MJD 55826·457 ± 0·020
γ	=	−8·37 ± 0·08 km s^{-1}	$a_1 \sin i$	=	13·10 ± 0·03 Gm
K_1	=	49·77 ± 0·13 km s^{-1}	$a_2 \sin i$	=	15·08 ± 0·06 Gm
K_2	=	57·27 ± 0·21 km s^{-1}	$f(m_1)$	=	0·2207 ± 0·0017 $M_\odot$
q	=	1·151 ± 0·005 ($= m_1/m_2$)	$f(m_2)$	=	0·336 ± 0·004 $M_\odot$
e	=	0·3158 ± 0·0019	$m_1 \sin^3 i$	=	1·175 ± 0·010 $M_\odot$
ω	=	154·8 ± 0·4 degrees	$m_2 \sin^3 i$	=	1·021 ± 0·007 $M_\odot$

R.m.s. residual (unit weight) = 0·43 km s^{-1}

The mean separation of the stars is at least 28 Gm (from $a_1 \sin i + a_2 \sin i$ in the table above), which is of the order of 20 times the sum of their radii, so just the qualitative fact that there are eclipses demonstrates that the orbital

TABLE V

Radial-velocity observations of LL Aqr

The sources of the observations are as follows:
2007 — İbanoglŭ et al.[35] *(weighted ⅕ in orbital solution);*
2011/2 — Cambridge Coravel (primary weight 1, secondary ⅓)

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
2007 Aug. 15·137	54327·137	+25·7	−48·4	0·696	+0·6	−1·5
16·189	328·189	+21·7	−41·7	·748	+1·3	−0·2
17·061	329·061	+12·3	−33·9	·791	−1·3	−0·3
18·060	330·060	−6·2		·841	—	—
19·045	331·045	−15·9		·889	—	—
23·052	335·052	−63·5	+56·3	1·088	+0·5	+0·7
24·058	336·058	−49·1	+37·4	·138	−0·9	0·0
25·054	337·054	−32·8	+18·0	·187	−0·6	−1·0
26·043	338·043	−14·3		·236	—	—
Sept. 21·097	364·097	+26·7	−44·5	2·528	+2·4	+1·4
23·922	366·922	+25·5	−48·9	·668	−1·0	−0·4
24·982	367·982	+22·7	−44·5	·720	−0·7	+0·4
2011 Sept. 30·975	55834·975	+14·7	−35·0	75·422	−0·4	+0·4
Oct. 7·986	841·986	+17·4	−37·8	·770	0·0	+0·2
15·937	849·937	−40·3	+26·7	76·164	−0·6	−0·9
18·928	852·928	−0·8	−15·2	·312	+0·7	+1·1
19·913	853·913	+6·7	−26·0	·361	−0·2	−0·1
22·919	856·919	+23·2	−44·2	·510	+0·1	+0·4
23·906	857·906	+25·4	−46·3	·559	−0·4	+1·4
Nov. 1·896	866·896	−69·1	+61·2	77·004	−0·4	+0·2
9·871	874·871	+12·6	−32·1	·399	+0·3	+0·1
19·847	884·847	−20·0	+3·9	·894	−0·9	0·0
Dec. 3·788	898·788	+26·7	−47·7	78·585	0·0	+1·0
28·734	923·734	+7·3	−26·1	79·821	+0·5	−0·3
2012 Jan. 2·737	55928·737	−68·0	+62·2	80·069	+0·6	+1·2
5·725	931·725	−23·6	—	·217	−0·1	—
Aug. 9·078	56148·078	−41·3	+29·4	90·939	+0·2	−0·4
10·046	149·046	−63·1	+55·5	·987	+0·5	+0·3
15·080	154·080	−18·5	+3·2	91·237	−0·2	+0·2
21·050	160·050	+24·6	−45·9	·532	+0·1	+0·3
31·020	170·020	−71·7	+65·4	92·027	+0·4	+0·4
Sept. 4·027	174·027	−21·8	+4·6	·225	−0·5	−1·9
7·000	177·000	+8·8	−29·3	·372	+0·2	−1·3
18·974	188·974	−55·2	+45·0	·966	−0·4	0·0
Nov. 21·855	252·855	−50·5	+40·4	96·132	−0·2	+0·6
Dec. 1·819	262·819	+27·0	−48·8	·625	−0·2	+0·5
9·766	270·766	−71·3	+64·4	97·019	+0·1	+0·2

inclination must differ by less than ¹⁄₂₀ of a radian, or 3°, from 90°. For practical purposes, therefore, we can set $\sin i = 1$ where it occurs in the elements above. The logarithm of the mass ratio q is 0·06, which, interpreted in the light of Andersen's graph[10] referred to in the section on HD 25788 above, indicates a difference of four or five sub-types between the spectral types of the components. The actual values of the masses suggest types close to F8 V and G2 V.

The mean equivalent widths of the dips of the primary and secondary components are 2·12 and 1·07 km s^{-1}, respectively — a ratio of almost exactly 2:1, corresponding arithmetically to a magnitude difference of $0^{m}{\cdot}75$. Using the rule of thumb, noted in the HD 25688 section as stemming from the 'Suchkov

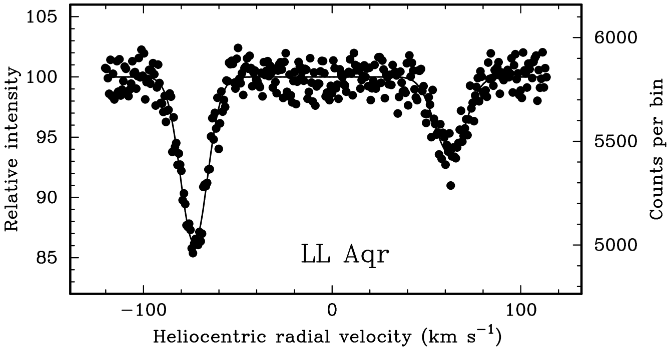

FIG. 11

Radial-velocity trace of HD 213896 (LL Aquarii), obtained with the Cambridge *Coravel* on 2012 December 9.

paper'[1], that the difference of V magnitudes can be taken as about 1·15 times the ratio of dip areas expressed in magnitudes, we find that for LL Aqr $\Delta m \sim 0^{m}\cdot 85$. Just like the difference in masses, that corresponds to a difference of four or five sub-types in spectral type, so the two criteria back one another up very satisfactorily.

All that can be said about the rotational velocities of the stars is that they are small, the values given by individual traces ranging from 0 to 10 km s^{-1}. For each component some individual values are 0, and since negative values are not permitted that implies that the means are liable to be over-estimates. It would be entirely consonant with the observations to suppose that both stars are rotating in pseudo-synchronism[36] with the orbital revolution, which would require their rotational velocities to be near 5 and 4 km s^{-1} for the primary and secondary components, respectively.

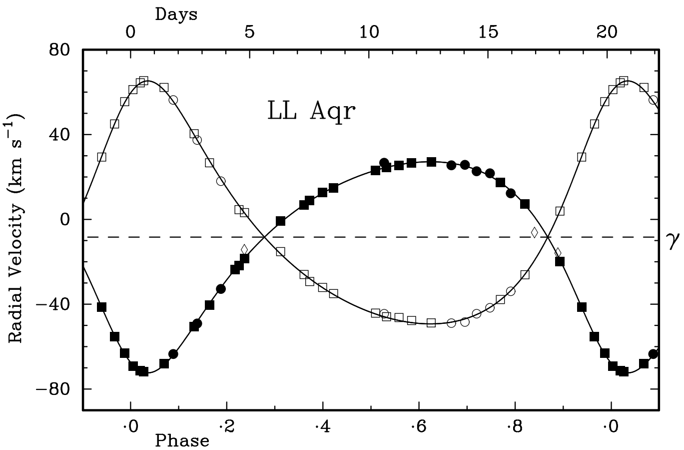

FIG. 12

As Fig. 2, but for LL Aquarii.

References

(1) R. F. Griffin & A. A. Suchkov, *ApJS*, **147**, 103, 2003.
(2) A. A. Suchkov & M. McMaster, *ApJ*, **524**, L99, 1999.
(3) H. G. van Bueren, *Bull. Astr. Inst. Netherlands*, **11**, 385, 1952.
(4) [Announced by] E. Høg *et al.*, *A&A*, **355**, L27, 2000.
(5) [Announced by] B. Nordström *et al.*, *A&A*, **418**, 989, 2004.
(6) F. van Leeuwen, *Hipparcos, the new reduction of the raw data* (Springer, Dordrecht), 2007.
(7) O. C. Wilson, *ApJ*, **93**, 29, 1941.
(8) A. H. Joy, *ApJ*, **71**, 336, 1930.
(9) R. F. Griffin, *The Observatory*, **121**, 315, 2001 (Paper 160).
(10) J. Andersen, *A&A Review*, **3**, 91, 1991 (see Fig. 2).
(11) A. Bečvář, *Atlas Skalnaté Pleso II* (Přírodovědecké Vydavatelství, Praha), 1951, p. 280.
(12) G. V. Zajtseva, *Peremennye Zvezdy* ('*Russian Variable Stars Bulletin*'), **19**, 63, 1973.
(13) L.-F. & Q.-F. Xing, *A&A*, **537**, A91, 2012.
(14) M. S. Kazanasmas, *Abastumani Astr. Obs. Bull.*, **44**, 175, 1973.
(15) N. Houk & C. Swift, *Michigan Catalogue of Two-Dimensional Spectral Types for the HD Stars, Vol. 5, Declinations –12°·0 to +5°·0* (Univ. of Michigan, Ann Arbor), 1999, p. 51 (HD 32704); p. 327 (LL Aqr).
(16) T. Pribulla *et al.*, *MNRAS*, **392**, 847, 2009.
(17) K. Biazzo *et al.*, *A&A*, **542**, A115, 2012.
(18) M. Lampton *et al.*, *ApJS*, **108**, 545, 1997.
(19) D. J. Christian *et al.*, *AJ*, **117**, 2466, 1999.
(20) G. Walker *et al.*, *PASP*, **115**, 1023, 2003.
(21) S. M. Rucinski *et al.*, *AJ*, **124**, 1746, 2002.
(22) M. A. C. Perryman *et al.*, *A&A*, **331**, 81, 1998.
(23) R. F. Griffin, *The Observatory*, **133**, 144, 2013.
(24) S. E. Robinson *et al.*, *ApJS*, **169**, 430, 2007.
(25) C. W. Allen, *Astrophysical Quantities* (Athlone, London), 1973, p. 206.
(26) E. V. Kazarovets* *et al.*, *IBVS*, no. 4659, 1999.
(27) P. N. Kholopov (editor-in-chief), *General Catalogue of Variable Stars*, 4th Edn. (Nauka, Moscow), 1985.
(28) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997.
(29) T. Mazeh, *AJ*, **99**, 675, 1990, and references therein.
(30) T. Mazeh & J. Shaham, *AJ*, **77**, 145, 1979.
(31) M. Perryman, *Sky & Tel.*, **97**, part 6, p. 40, 1999.
(32) S. A. Otero & P. A Dubovsky, *IBVS*, no. 5557, 2004.
(33) G. Pojmański, *Acta Astr.*, **52**, 397, 2002.
(34) P. A. Woźniak *et al.*, *AJ*, **127**, 2456, 2004.
(35) C. İbanoğlu *et al.*, *MNRAS*, **390**, 958, 2008.
(36) P. Hut, *A&A*, **99**, 126, 1981.

*initials misprinted as A. V. in the actual publication

REVIEWS

Europe to the Stars, by G. Schilling & L. Lindberg Christensen (Wiley-VCH, Weinheim), 2012. Pp. 264 + DVD, 26·5 × 25·5 cm. Price €34·90 (about £30) (hardbound; ISBN 978 3 527 41192 4).

Last year (2012) marked the 10th anniversary of the UK's agreement to join the European Southern Observatory (ESO) as the 10th member state, using the *VISTA* survey telescope as part of the joining price. More significantly, however, it marked 50 years since the signing of the ESO Convention by the five founder members on 1962 October 5. This volume celebrates, in the words of the book's subtitle, 'ESO's first 50 years of exploring the southern sky'. However, this year, 2013, marks 60 years since the first serious discussion of the idea of a joint European effort in astronomy, a discussion that took place on a boat trip in the Netherlands between Kourganoff, Oort, and Spencer Jones, following a conversation between Oort and Baade earlier in the year. The idea Oort outlined to Baade may have been sparked off by Oort's first visit to the southern hemisphere the previous year. Gart Westerhout later remembered losing track of where Oort was on that site-testing trip to Hartebeespoort, and recalls finding him "flat on [his] back in the wet grass … with the centre of the Milky Way in the zenith", clearly spell-bound by the sight.

Despite Oort being recognized as the founding father of ESO, he was never Director General, although Dutch influence has remained strong, with four of the seven DGs to date having been Dutch. There are photographs and short interviews with all seven scattered through the book. But the main emphasis in the book is not on a complete history of ESO, for which the reader is referred (on p. 263) to another book of more than twice the length [see following review], but on the extraordinary progress that ESO has made from a small observatory at La Silla to running some of the largest and most ambitious telescopes in the world, up to its current involvement in *ALMA* and the *E-ELT*. This volume traces that progress, with a well-written, succinct text profusely supplemented by beautiful and/or historic pictures. There are several magnificent fold-out panoramas of observatory sites. The text, generally at the layman level, tells a continuous story, with different themes in each of the ten chapters, but is also accompanied by one- or two-page summaries of particular topics, such as comets or adaptive optics or unconventional visitors (did you know that the closing scenes of *Quantum of Solace* were filmed at Paranal? Director General Tim de Zeeuw recalls thinking to start with that the letter requesting permission was a hoax!). The pleasure and pride of everyone who works at ESO shines out from every page.

There are of course a few quibbles — for example, on p. 62 there is the classic error of stating that it is *radiation* pressure [my emphasis] that resists gravity in stars; it would have been so easy (and much more accurate) just to say 'pressure' — and the odd typo (*e.g.*, in the figure caption on p. 95 the page reference should be to p. 146, not 116), but in general the standard of production is very high and a great deal of information is conveyed in an attractively informal and beautifully illustrated manner. Two appendices provide details of ESO's telescopes, past and present, and a time-line for the development of the Observatory. On top of all this, there is a superb DVD attached to the back cover of the book, with an hour-long main film that complements the text of the book, and some 'bonus material': three time-lapse movies and a slideshow; narration is in English, but subtitling is provided in 13 languages (including English itself).

The achievements of ESO are well celebrated by this splendid book, and the price makes it a possible birthday present for your favourite astronomer. — ROBERT CONNON SMITH.

The Jewel on the Mountaintop: The European Southern Observatory through Fifty Years, by C. Madsen (Wiley-VCH, Weinheim), 2012. Pp. 560, 24·5 × 17·5 cm. Price €49·90 (about £42) (hardbound; ISBN 978 3 527 41203 7).

'ESO at 50' is more than a mere milestone report, and more than a history. It is a celebration, because — without doubt — ESO has much to celebrate, whatever its age. But rather than a eulogy, it is a factual description of events, which it traces in detail through the many ideas and innovations that were conceived, pursued, developed, and refined during the growth and establishment of ESO as a world-class observatory. Here is the emergence of 'Big Science' across half a century, and Madsen does it ample justice.

In researching the material for this book, Madsen conducted numerous interviews and perused heaps of archival reports and documents, yet the finished article reads like an end-to-end eye-witness account. Writing in almost flawless English (with only a slight over-abundance of commas), enriching it with an assortment of photographs, and sprinkling it with touches of wry humour, he tells the whole story honestly and fairly. He employs no filter to remove the negative bits, and does not attempt to play down individual foibles, or disasters that never quite happened. With the talent of the true biographer, Madsen is able to visualize each small event as part of the greater picture, where setbacks are teachers and complacency has no place.

The achievements of ESO are the more remarkable, given that only a handful of years earlier almost every nation within Europe was at loggerheads with all of its neighbours, and their research programmes were in tatters. From what remained grew a brave new idea — to build a pan-European observatory that would research the relatively unknown regions of the southern sky and would also become challenging, absorbing, and productive enough to halt the 'brain-drain' that was decimating Europe's astronomy population. Engaging top expertise and skills to work and interact regardless of nationality, attracting the best industrial contracts regardless of nationalistic preferences, and employing staff from local or international origins regardless of language or culture, helped establish a multi-talented entity whose prime devotion was to Europe rather than to the home country. Yet in a way the timing of the conception of ESO was part of its making. The evil that was in the European air for many preceding decades had first to be exorcised, and though the costs of so doing were huge and left Europe weak and impoverished, the challenge of creating an ESO facility was the right fillip to breathe new life and creativity into souls who (as after a comprehensive but successful surgery) emerged enfeebled but potentially healthy. Despite early political challenges from Chile and fiscal ones in France, what has come out of ESO by way of the *NTT* and *VLT* alone, as well as subsequent partnerships in global instruments, amply vindicates the experiments of its founders to pool European expertise. Progress took time; every new concept required the construction of its own road (literally as well as metaphorically), and every new development demanded delicacy as well as courage, but if ever the total is greater than the sum of its parts, it is here.

It is not quite apparent for whom this book is intended. Madsen deliberately does not assume familiarity with astronomy or with its jargon, so as not to exclude the layman. All the same, the amount of detail, and the following-through of so many intriguing threads of personal involvement, suggest a readership that has at least some familiarity with the *dramatis personae*. Indeed, anyone who has had any involvement with, or interest in, the incredible astronomical phoenix that sprang out of the ashes of WW II, should get a copy. It's an absorbing read. — ELIZABETH GRIFFIN.

Nicolas-Louis De La Caille, Astronomer and Geodesist, by I. S. Glass (Oxford University Press), 2012. Pp. 194, 24 × 16·5 cm. Price £35 (hardbound; ISBN 978 0 19 966840 3).

The 18th Century was the great age of long-haul global scientific expeditions:

to measure the shape of the Earth, observe Venus transits in pursuit of the solar parallax, or map the stars of the southern hemisphere. It was an age of extraordinary adventure and courage, and without doubt Frenchmen and Englishmen were the big players, and often co-operators, in spite of the frequent wars between their countries.

Abbé Nicolas-Louis De La Caille visited South Africa, and then Mauritius and Réunion in the Indian Ocean, during one spell of peace, between 1751 and 1754, performing work of the highest order. And this expedition, along with much more of his sadly short life, is admirably covered in Glass's biography. De La Caille was born into a comfortable French family, was ordained a Deacon, or Abbé, in the Roman Catholic Church, and, like so many men in that age, both Catholic and Protestant, spent his life as a clerical scientist, in particular as an astronomy professor at Paris's distinguished Collège Mazarin.

Eighteenth-Century astronomy was dominated by astrometry, and the need to measure increasingly small angles for the purposes of celestial cartography, geophysics, Newtonian celestial mechanics, and cosmology. It was this quest that took De La Caille to the southern hemisphere. And from his early days, as he recollected towards the end of his life (p. 131), it had been his ambition to re-observe the heavens to a new standard of accuracy.

Glass gives a detailed account of De La Caille's instruments and observing practice. He observed at the Cape with a quadrant, a sextant, and a zenith sector, all equipped with what were by 1751 standard telescopic sights. Between 1751 August and 1752 July, sometimes working continuously for eight hours on a clear night, he successfully took the positions of 1942 stars in right ascension and declination. There is also discussion of De La Caille's geodetic work at the Cape, as part of the 18th-Century (predominantly French) enterprise to establish the exact parameters of the Earth's oblate shape.

This is an excellent and detailed book, not only dealing with a great astronomer, but exploring the less-well-known world of 18th-Century French research astronomy. It is lucidly written and thoroughly documented. De La Caille died at the age of forty-nine, apparently from a cold, but was most likely bled to death by his Parisian physicians who, in accordance with prevailing clinical theory, believed that bleeding reduced inflammation. — ALLAN CHAPMAN.

Ordinary Geniuses: Max Delbrück, George Gamow, and the Origins of Genomics and Big Bang Cosmology, by Gino Segrè (Viking, New York), 2011. Pp. 330, 22·5 × 14·5 cm. Price $27·95 (about £17) (hardbound; ISBN 978 0 670 02276 0).

What author Segrè (nephew of Emilio Segrè and himself a physicist) means is that Gamow (1904–1968) and Delbrück (1906–1981) were not Pauli, Heisenberg, Dirac, or Einstein (extraordinary geniuses). Segrè knew them both, and calls them Max and Geo throughout. I knew Delbrück not at all and Gamow only slightly, but enough to be aware that the author's statement that he had stopped consumption of alcohol in his later years is not entirely true, at least not at the time of the 1967 'Texas' Symposium on Relativistic Astrophysics. This was the first 'Texas' after the discovery of the cosmic microwave background and so perhaps an emotionally charged event for him. All I know first-hand is that he bought me a drink and sketched me as "the redshift girl", an allusion both to my dress and to my work with Jesse Greenstein, measuring the relativistic redshift in white dwarfs.

But Segrè's Gamow is the creator of the liquid-drop model of the nucleus, and the Urca process (back when Carmen Miranda was the highest-paid woman in the United States), the thesis advisor of Vera Rubin, the alanine of the RNA Tie Club, and the propagator of the "Einstein's biggest blunder" anecdote. The book is full of "aha!" moments, not all closely coupled to the "ordinary heroes". Particularly striking is a remarkably (but perhaps not unduly) harsh letter from Lise Meitner to Otto Hahn condemning German scientists who remained in Germany throughout World War II. The letter is dated 1945 and said to have been written from Sweden (where she first fled in 1938). A couple of other "Aha!s" — Delbrück had originally been an astronomy student at Göttingen; Gamow and his first wife, Rho (Lyubov Vokhminzeva), tried to escape from the Soviet Union by rowing from the Crimea to Turkey; and the scientist in Solzhenitsyn's *The First Circle* was modelled on Nikolai Timofeev-Ressovsky (not Kozyrev, apparently).

Not surprisingly, a good many missing pieces and people, mistakes, and so forth also stalk through the text. Some are quite minor (Baade and Zwicky first wrote super-nova with a hyphen and took a few years to pry it out). Some are rather more serious (the statement that, as the Universe cooled from 3000 K to 3 K, each photon's wavelength decreased by a factor of three thousand). And some in between (the curtailments of the stories of the discovery of dark matter — no, Zwicky did not invent the words; and of the CMB — multiple near-misses and one unsuccessful deliberate search before Penzias, Wilson, Dicke and all). And if you are going to read only one footnote, let it be the capsule biography of Fritz Houtermans on page 294: imprisoned first by the Russians and then by the Germans he has to have been the unluckiest physicist of his generation.

Delbrück and Gamow, in contrast, were considerably luckier, and their work is now much better known. This joint biography is definitely worth reading, easiest, perhaps, if you already know a smidge about DNA and cosmology. My copy came from Edward R. Hamilton, Bookseller, and so is completely conflict-free as well as having been a major bargain, as have been all (well, almost all) of the books, CDs, *etc.*, I've acquired from that source. — VIRGINIA TRIMBLE.

Organizations, People and Strategies in Astronomy, Volume 1, edited by A. Heck (Venngeist, Duttlenheim, France), 2012. Pp. 324, 24 × 16·5 cm. Price €63 (about £54) (paperback; ISBN 978 2 9542677 0 8).

I first encountered André Heck as a colleague among the first wave of Resident Astronomers in Spain at the launch of the *IUE* mission in 1978, and I think it's fair to say that he was the most organized of us, with a strong sense of the procedures needed to bring order to the excited chaos that surrounded those early days of what was to be an outstandingly successful project. He appears as editor of this present series of books (a successor to the first series, *Organizations and Strategies in Astronomy*, which ran to seven volumes), but he is much more than that, for he has driven the whole programme in a brave effort to record just how we actually *do* astronomy. Furthermore, dissatisfied with the approach of the usual commercial publishers, he has created his own company, Venngeist, to produce the present volume and, more generally, "devoted to the non-commercial production of scientific publications, serving the professional astronomy community".

As the title suggests, the present series has a greater emphasis on 'people' in astronomy, and readers in the UK will find Paul Murdin's summary of the recent demographics of British astronomy of particular interest. While we seem to be making some progress here in engaging women and minorities, it would seem to require more effort in the USA, as outlined by Jarita Holbrook. On the professional front, there are several articles of interest to aspiring astronomers, but one that contains some particular words of wisdom on PhD theses is that by Jeff Linsky who offers 'Unexpected advice for beginning graduate students in astrophysics'. In the arena of outreach, there are again several chapters of interest, including one by Carol Christian *et al.* which describes public involvement in projects such as the 'Zooniverse' (see these pages **132**, 227).

One chapter in this volume that caused me some concern is by Mike Bode on 'The ASTRONET infrastructure roadmap: a strategic plan for astronomy in Europe'. While it is certainly true that national financial resources are becoming ever tighter, I fear that EU-style governance will put unacceptable constraints on the blue-sky approach that has traditionally been available to astronomers in Britain. I have this awful nightmare where UK astronomers will have been forced to abandon all of their diverse observing facilities for a 1% share of one 50-m telescope. Set against that, however, is the story that Jack Meadows tells of the survival of the large and diverse (if rather impersonal) Rutherford Appleton Laboratory, while smaller and more specialist institutions (like the 300-year-old and much-more-friendly Royal Greenwich Observatory) were flushed down the drain of history.

For editors of this *Magazine* and other journals in astronomy, the chapter by Uta Grothkopf on 'Two decades in an astronomical library' makes for interesting reading ("interesting" in the sense of the "interesting times" from the old oriental curse). *The Observatory*, as I outlined in *OSA*, **4**, 205, has no plans to go electronic; if that leads to our demise, so be it.

Here, then, in *OPSA* **1**, is a fine collection of material for thought; and I gather that *OPSA* **2** is not far behind. Well done, André, for charting our 'progress' in the doing of astronomy. — DAVID STICKLAND.

Working on Mars, by W. J. Clancey (MIT Press, London), 2012. Pp. 310, 23·5 × 18·5 cm. Price £20·95/$29·95 (hardbound; ISBN 978 0 262 01775 6).

William Clancey has taken on the difficult task of writing an analytical and narrative background to the scientific missions of the Mars Exploration Rovers (MER). It's not so much a scientific account as a description of the how and why of the mission, told from the viewpoint of its leading participants. From 2002 to 2005 Clancey led a NASA Ames team of computer and social scientists as 'participant observers' of the Science Team. They documented conversations, meetings, and plans by observing that team's scientific conferences from the landing of *Spirit* in 2004 January and finishing with selected interviews two years later.

Clancey asks how working with a mobile robotic laboratory changes the nature of field science. MER is compared with other missions, and Clancey moves on to discuss telerobotics and the nature of scientific practices, including the scientific team's 'virtual presence' on the Martian surface. He writes a lot about scientific cooperation and teamwork, including the idea of 'planning by consensus'. How the lessons from MER can inform and guide future missions forms the subject of the closing chapter. He concludes that chapter with this perceptive comment: "If it turns out that remotely reprogrammed systems can be made adequate for scientists' needs …. then the articulated motivations for human spaceflight will need to shift from talk about scientific exploration to something more deliberately poetic, commercially practical, or frankly political and nationalistic." The author's Epilogue, however, shows him to be an optimist about human exploration and, as he nicely quotes Chris Mackay: "Even if computers progress so much they can go to Paris, taste the wine, eat the food and come back and tell me all about it, I'd still want to go myself."

In summary, Clancey has provided an interesting behind-the-scenes perspective about how scientists worked together to operate the MER. He's done a thorough job, and future participants in the field will have much to learn from it, but I suspect that the nature of the subject matter will limit its more general readership. — RICHARD MCKIM.

Annual Review of Earth and Planetary Sciences, Volume 40, 2012, edited by R. Jeanloz & K. H. Freeman (Annual Reviews Inc., Palo Alto), 2012. Pp. 737, 24 × 19·5 cm. Price $248 (institutions; about £160), $94 (individual; about £60) (hardbound; ISBN 978 0 8243 2040 9).

Weighing in at 1·7 kg and 737 pages, this year's volume doubles up as exercise for both mind and body. Amongst its 27 chapters, there are, however, guaranteed to be juicy plums for readers with interests in almost any aspect of the subject.

The first chapter, reminiscences of Henry L. Ehrlich on his career in geomicrobiology, is the kind of window into a person's life that is always uplifting to read. In my opinion everyone should be forced to write such a piece before they are allowed to retire. Let's have many more.

A chapter compares the end-Permian mass extinction with the present day. This neat, digestible summary concludes that it was the Siberian Traps that were ultimately responsible, though we cannot, of course, appeal to flood-basalt eruptions to explain the current on-going mass extinction. Magma oceans are dealt with elegantly by Linda T. Elkins-Tanton in a well-written, beautifully illustrated, and extensively referenced chapter. This is nicely complemented by a helpful review of current opinion on whether or not Archean subduction occurred. The authors have little doubt that it did, but that the style was rather different from Phanerozoic subduction. The ancient past is also the subject of a chapter by Norman Sleep and colleagues on the effect of the biosphere on mantle composition. In particular, the biosphere can fractionate isotopes, a process not thought to be significant in geological mantle processes. Other esoteric aspects of the biosphere are dealt with by chapters on intra-terrestrial life and the habitats of very early life.

Climate change is well covered by several chapters on such subjects as the coupling of the solid Earth, oceans, and atmosphere, tropical climate change, the future of Arctic sea ice (bleak), and impacts on the organic carbon cycle. To add a novel flavour, there is also a chapter on methane weather on Titan, Saturn's largest satellite. (Take heart. It's even worse than Earth's.) We have not yet got around to investigating whether Titan's climate is also changing, because a single seasonal cycle is 30 years long.

For geological traditionalists there is a chapter on New Guinea tectonics, and for those in the thick of the mantle-plumes controversy, yet another paper highlighting the chemical heterogeneity of the mantle. The residence time of those heterogeneities is long, and the authors hold out the tantalizing possibility that such heterogeneities may be detectable using seismology.

So, as usual, something for everyone in this excellent book. In addition to enlightening those of us who ought to know more than we do, the potential of *Annual Review of Earth and Planetary Sciences* for providing helpful summaries for undergraduates is, I think, insufficiently widely appreciated. I shall certainly recommend it to my final-year classes in future. — GILLIAN FOULGER.

An Introduction to Celestial Mechanics, by R. Fitzpatrick (Cambridge University Press), 2012. Pp. 266, 26 × 18 cm. Price £40/$65 (hardbound; ISBN 978 1 107 02381 9).

If I were teaching a senior-undergraduate or postgraduate course on celestial mechanics, this is undoubtedly the text I would choose. Throughout the book, and within each chapter, the author starts with a careful exposition of the fundamental ideas. These could readily be understood by a first-year undergraduate, and the more advanced student may be tempted to skip over what may be regarded as familiar material such as Newton's laws of motion and of gravitation. I would advise against this. I found the discussion of even such 'familiar' topics not only fascinating, but essential for the understanding of later sections. Following the fundamental concepts, the bulk of the book (and of each chapter) moves to material that is appropriate for final-year honours undergraduate level. And there is also material that is beyond what is needed in a typical undergraduate course and would be more of interest to postgraduates specializing in the field. That's all right, though. Keep the book after you have graduated, and you'll be glad of the more advanced material then. Regardless of the level, I must commend the author for the unusual clarity of his writing. There are some tough parts, but I didn't find any sections where I got bogged down because of inadequate explanation by the author.

As an example of the gradual approach from the elementary to the more advanced, I would cite the chapter on gravitational potential, where the concept is explained, and some simple examples are given. Then we move on to some more difficult (and important) examples than are treated in elementary texts, such as the potential of axially-symmetric mass distributions, of a uniform spheroid, and of a uniform ring. It is in these examples that we become familiar with the use of Legendre polynomials.

After potential theory we move to the theory of Keplerian orbits. Many undergraduate physics texts limit this topic to showing that a particle under the influence of an inverse-square force will move in an elliptic orbit. Here we go beyond that and on to calculating the position in an orbit (the several 'anomalies') as a function of time, and the heliocentric ecliptic coordinates in terms of the orbital elements — all of which an astronomy student will need to know, but which is generally not treated in a standard physics text. We don't go quite as far as being able to construct an ephemeris from the elements, but we are close, and a reader wanting to do this doesn't have much further to go.

Although the sections on parabolic and hyperbolic orbits are brief, this is made up for in the examples at the end of the chapter, where the student can work out for himself what to do in these cases. I can think of many methods of solving Kepler's equation better than the one offered by the author. On the other hand, when it came to the corresponding equation for a parabolic orbit, the author gave me a better method than the one I have usually used, so we are quits.

The chapter on orbits does not tell us how to calculate the orbital elements from a set of observations. This will disappoint some readers, but that problem, however much you may want to know how to do it, is sufficiently difficult that to do it properly needs almost a book in itself devoted to it. However, for those wanting a more advanced topic, there is one showing how to calculate the precession of the perihelion of a planetary orbit from planetary perturbations, and even how to do the General Relativity correction for the precession of Mercury (though in the latter case we are freely given the formula for the modification of the field).

Much of the material in Chapters 5 to 7 (rotating reference frames, Lagrangian mechanics, rigid-body rotation), while having many astronomical applications, would be quite suitable for a course on classical mechanics for non-astronomy students. Indeed, if one has a mixed population of students, some taking astronomy, some not, you may well want to consider using this text for both, rather than requiring students to buy separate books for courses in celestial and classical mechanics. If you were to use it as a text for a classical-mechanics course, you may be able to tempt some of the brightest students into astronomy! Hamiltonian mechanics and Hamilton–Jacobi theory are not covered, but you could supplement the text with your own lectures on these topics. The chapter on rigid-body rotation includes a lengthy and fascinating section on spin–orbit coupling. This is an example of a topic that is taken well beyond what would be needed in an undergraduate course — but it is good to have a few examples like that.

The restricted circular three-body problem is done — that's always great fun, and the treatment here is good. The stability of the Lagrangian points L4 and L5 is discussed. These points are points of *unstable* equilibrium (maximum potential energy). It is the small orbits *around* them that are stable (as long as the particle goes the right way round in its orbit).

By the time we get to Chapter 9, on general secular-perturbation theory, we are moving into more advanced material, but which certainly can be taught in an advanced undergraduate course. The material includes, of course, the Lagrange planetary equations. (There are lots more equations of like ilk in an appendix. These will not be needed for most undergraduate teaching, but they will be a most valuable reference for those pursuing the subject further.) There is a lot of material on the secular evolution of asteroid and satellite orbits, which many readers (not only undergraduates) will want.

I have never studied lunar theory — I have always assumed that it was far too difficult for me. But that is the subject of the final chapter. I have not yet read it, but, if it is written with the clarity and care of previous chapters, I have no doubt that I shall become an instant expert, and I am looking forward to tackling it.

Each chapter is followed by a selection of examples (no solutions given!). I greatly enjoyed doing those problems. I found that they were well chosen and instructive, and they much increased my understanding and appreciation of the text.

Is there anything disappointing that I found in the book? Well, yes, as a reviewer I like to search for mistakes, especially minor and trivial ones, so that I can make nasty remarks about them in a review. I tried hard. I searched and searched, but the proof-reading and the care of the production has been so thorough that I almost failed in my search. I couldn't find a spelling or grammatical mistake, let alone a scientific mistake or a wrong equation. The diagrams are clearly drawn and labelled. The (many!) equations are printed to the highest standards of mathematical typesetting. I almost despaired of finding anything to criticize until — just moments before I mailed my review — I think I spotted one. On page 93 I think the author has taken the cube root of two to be 1·41. But that's all that I could find.

A summary? This is a first-rate text to use as a senior-undergraduate text in celestial mechanics. I recommend it strongly without reservation. — JEREMY B. TATUM.

Cosmology and Gravitation, edited by M. Novello & S. E. Perez Bergliaffa (Cambridge Scientific Publishers, Cambridge), 2012. Pp. 198, 28 × 21·5 cm. Price £60 (hardbound; ISBN 978 1 908106 20 9).

This is a series of articles resulting from a school held in Rio in Brazil in 2010. It is an interesting volume in that it covers a rather heterogeneous set of topics, some of which are quite specialized. As a result, I suspect that all readers will find something new in here. The scope includes theory and observation, with some historical perspectives, but with an emphasis on theory. Singularities in cosmology, some aspects of black holes, $f(R)$ theories, and mass (Mach or Higgs?) are some of the areas covered, and if this gives the impression that the school proceedings is not a pedagogical volume, then that is correct. The level of assumed knowledge is rather high, and many students will need good preparation to understand the articles. On the other hand, the apparently rather random nature of the topics covered is curiously rather appealing, and many researchers in the field, including those with plenty of experience, may enjoy being taken into unusual corners of the subject, and may well be enthused by a few of the articles. — ALAN HEAVENS.

High Energy Astrophysics: An Introduction, by Thierry J.-L. Courvoisier (Springer, Heidelberg), 2013. Pp. 332, 23 × 14·5 cm. Price £67·99/$99/€80·20 (hardbound; ISBN 978 3 642 30969 4).

Author Courvoisier begins by pointing out that the subject matter of high-energy astrophysics is very poorly defined. Historically it was "lots of energy released in a hurry"; later, quite often almost identical with X-ray and γ-ray astronomy, though many of the important sources (pulsars, active galaxies) first turned up as strong, non-thermal radio sources or even (black holes, binary neutron stars) as theoretical entities.

What this volume gives you is equation-dense treatments of radiation processes (Bremsstrahlung, cyclotron, synchrotron, Compton and its inverse) plus pair creation and particle acceleration to start with, then accretion as a bridge to the classes of sources. These are well categorized (nine flavours of AGN, at least three colours of X-ray binaries, and possible evolutionary relationships), and for each the underlying physics is described in so far as it is understood. The radiation processes are each presented with one sort of source for which it is important. No, I haven't checked all the equations, and so am mildly annoyed by other sorts of items; here are just a couple of examples (because I think this will be a very good textbook at the level suggested by the author — advanced MSc students and beginning PhD ones). Credit for discovering GRBs is given exclusively to the US *VELA* satellites, with no mention of the simultaneous USSR *Kosmos* recognition. A few figures are tricky to make out: 13.9 with its 14(?) equations of state for dense nuclear matter, and 1.10 with effective cross-sections of various components of the interstellar medium, with long dash, long dash-dash-dahh, *etc.*, difficult to distinguish (a case where colour might have been useful). Gravitational radiation just barely makes the cut to be HEAp; neutrinos do not. And if you can stand for one more carp, it is that Fig. 5.10 shows radio, infrared, and optical emission from the Crab Nebula all at the same scale, but the X-rays blown up a factor of four or so, making the ring and jet look the same size as the whole nebula at the other wavelengths. And neither the caption nor the surrounding text says so.

The phenomenology is admirably up to date (19 neutron-star masses all consistent with 1·35 $M_{\odot}$, many with error bars less than $\pm$0·05 $M_{\odot}$) and the author has made good (and fully acknowledged) use of calculational methods from other books, including G. B. Rybicki & A. P. Lightman's *Radiative Processes in Astrophysics* and M. S. Longair's two-volume *High Energy Astrophysics*. The book is not, Courvoisier assures the reader, a text in General Relativity, and he presents just enough to be able to discuss accretion in a Kerr metric and such. This is also the topic where references are treated most casually: Shapiro and Teukolsky have no title, and MTW are alphabetized as Gravitation, rather than Misner. All in all, an admirable book and one I will gladly use as a text, the next time the opportunity arises. I hope Thierry has a secret stash of homework problems he is prepared to share! — VIRGINIA TRIMBLE.

Stellar Magnetism, 2nd Edition, by L. Mestel (Oxford University Press), 2012. Pp. 715, 25 × 17·5 cm. Price £85 (hardbound; ISBN 978 0 19 964174 1).

For this second edition of the book, Professor Mestel has set himself the task of bringing up to date the comprehensive and detailed survey of stellar magnetism that featured in the original version (OUP, 1999). It should be stressed that in such a fast-evolving field this task is almost bound to finish incomplete since such a book, on being published, is almost certainly missing some new and important recent advance. However, as pointed out by the author "It is not thy duty to complete the labour, but neither art thou free to desist therefrom". That Professor Mestel does such a spectacularly good job on this task is an amazing achievement.

The book itself is built carefully on the first edition, with the foundations set out in the early chapters. As in the first edition, these elementary considerations are comprehensively explained with many details included that are often omitted (or left to the diligent reader) in other such expositions. In this sense the book is still an ideal introduction for the starting researcher. It is in the later chapters that most of the differences from the first edition occur. There are significant additions to the chapters on 'Dynamo processes in stars', 'Stellar winds', 'Late-type stars', 'Early-type magnetic stars', and 'Pre-main-sequence stars' as well as that on 'Star formation'. The inclusion of this material has naturally meant the sacrifice of the chapters on 'Pulsar electrodynamics'. I do feel that this sacrifice is worth it, and the book remains a coherent survey of stellar magnetism even without the more esoteric considerations of the dynamics of pulsars.

Of course, in a book such as this with such a broad scope, one is always concerned with the accuracy of the information included. Having read most carefully the chapters on material with which I am most familiar (Chapters 5, 6, and 8), I can only say that the reader should have the utmost confidence in the exposition. Where there is disagreement in the literature, Professor Mestel gives a balanced view, but is not averse to giving his opinion as to which side of the argument he believes to be the correct one. Sometimes this opinion is included 'between the lines' or in a wry comment — but these are not difficult to pick up.

Perhaps the greatest strength of this book, though, is in the clarity of the exposition and the ease of reading. Professor Mestel has an engaging style, which makes the 700 pages seem much less. This means that, in addition to being an invaluable reference book that can be dipped into, the book can easily be read straight-forwardly from start to finish.

In short, the author has succeeded in his ambition. I am sure that this, now up-to-date, treatise will remain the book of choice in this field for a number of years to come. — STEVEN TOBIAS.

Galactic Archaeology: Near-Field Cosmology and the Formation of the Milky Way (ASP Conference Series, Vol. 458), edited by W. Aoki, M. Ishigaki, T. Suda, T. Tsujimoto & N. Arimoto (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 434, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 798 8).

"Galactic Archaeology aims to recover the evolutionary history of galaxies from their present stellar content, by measuring ages, chemical compositions, and kinematics for (very) large numbers of stars." So said conference summarizer Alvio Renzini in the last talk at this conference, originally scheduled for 2011 May, but postponed until 2011 November, because of the large earthquake and tsunami in host country Japan. It may well have been worth waiting for. There were, of course, long and short talks and posters, and some of the discussion following the talks has been preserved in the proceedings. Many of the observations presented come from the *Subaru* wide-field telescope, and the images are apparently in colour in the on-line version of the volume. Sadly, only the cover (NGC 2403 in very-nearly-honest low-contrast colours) and the conference photo (of which the less said the better) appear on paper in anything except glorious black and white.

Divergent viewpoints were expressed: galaxies can grow by mergers, but also by inflowing gas that makes stars later. Dark matter could be cold or warm. Globular clusters might be almost primordial objects, or just the surviving cores of systems once ten times more massive. But there were also some fairly firm answers: dwarf galaxies do have blue stragglers, and they are mostly late phases of mass-transfer close binaries; the X-ray ridge along the plane of the Milky Way consists largely of sources with the infrared spectra of late-type stars; and the early star-forming gas of the Milky Way was by no means chemically homogeneous, which shows up as normal incidence of binary stars among the extremely metal-poor stars with *r*-process enhancements.

Important results from other telescopes also appear, including the Sgr dwarf galaxy and stream observed with the *AAT*, the parent populations of core-collapse supernovae with spectra from the University of Hawaii 88-inch and *Gemini North*, and Milky Way satellite galaxies studied with *Keck*. Many of the participants were, of course, from Japanese institutions, but a pair of colleagues from Tehran, Iran, showed a poster containing data obtained with the *Isaac Newton Telescope* (now in the Canary Islands) processed with a German algorithm. The list of participants includes both postal and e-dresses, so you will be able to find them, at least for a while. Sadly missing from that list is Robert Rood of the University of Virginia, who died at the beginning of the conference week. — VIRGINIA TRIMBLE.

The Deaths of Massive Stars: Supernovae and Gamma-Ray Bursts (IAU Symposium No. 279), edited by P. W. A. Roming, N. Kawai & E. Pian (Cambridge University Press), 2012. Pp. 455, 25 × 18 cm. Price £76/$125 (hardbound; ISBN 978 1 107 01979 9).

According to the textbooks of my boyhood, the ultimate fate of stars more massive than $\sim 8 M_{\odot}$ was well understood, and straightforward: they became core-collapse supernovae (ccSNe), so let's briskly move on to the interesting stuff about neutron stars, pulsars, and stellar-mass black holes. Then along came SN 1987A, the first supernova for which a progenitor was identifiable, and, oops, contrary to all expectations it was a *blue* supergiant. There followed the unveiling of further progenitors (still not matching expectations in detail, although most detections are of red supergiants after all); the discovery of the association of SNe with gamma-ray bursts; and the cosmological applications of thermonuclear supernovae, which resulted in surveys which serendipitously yielded core-collapse systems in unprecedented numbers, and unforeseen variety. This flood of new results reinvigorated the study of ccSNe, turning it into a major industry, and attracting attention from a collection of observers and modellers with perhaps surprisingly diverse interests.

A substantial subset of this group of astrophysicists met in Nikko, Japan, in 2012 March, to discuss topics on these themes. It's perhaps worth recording that the meeting was postponed by almost a year, at short notice, as a consequence of the tragic events that struck Japan in 2011 March; more happily, it's also worth noting how quickly the proceedings have appeared in print, with a publication date only six months after the close of what was evidently a successful meeting.

In terms of format, this is a book of two halves, almost literally. Part 1 (311 pp.) gives the invited and review talks, reported together with their associated discussions, and covers the nature and evolution of progenitors, multi-wavelength (and particle!) observations of eruptions, models of core collapse and jets, and cosmological and GRB connections. This is followed, after the 'Concluding remarks' (in the middle of the volume), by a perhaps poorly-considered section of 'Conference photographs' — five full-page images, featuring six of the 158 participants, without identifications or captions. Parts 2 and 3 (130 pp.) then contain two-page reports of posters, and 'abstracts' (of what, I'm not sure) which vary in length from a brief sentence to a paragraph. Author, object, and subject indexes round off the volume. So, overall, a pretty standard conference proceedings: all good, sound stuff, and a collection of up-to-date, authoritative summaries that's handy to have for easy reference.

I do, though, want to air a particular gripe. The contributors to this volume have made very extensive use of colour in their diagrams and illustrations — and yet the book is published in unrelieved monochrome. The use of (supposed) colour is so pervasive that it's hard to believe that authors weren't encouraged to employ it, and the consequence is that a lot of presentations are significantly handicapped by its absence in print. As far as I can see, there's not a word about this anywhere in the book; no apology for the poor presentation, no pointers to on-line colour versions. Any help on the CUP web site? Nope — searching there for the book just led me in a circular round of links that never got beyond a basic sales pitch.

It turns out that it *is* possible to access on-line, full-colour versions of the contributed papers. The secret is to look up a paper on ADS; you'll then get through to the HTML version (and colour pdfs) on the CUP web site, discovering in the process that the trick is to know that IAU Symposium 279 is, in CUP-speak, also Volume 7 of the journal(!) 'Proceedings of the International Astronomical Union'. As far as I can see, this isn't mentioned anywhere in connection with the printed book — perhaps because spending a considerable amount of money on the bound volume doesn't confer any privileges to access this on-line material; for that, you'll additionally need a subscription to the journal (or be willing to fork out another $45 to view a single paper that you've already paid for).

I suppose that, in practice, this won't matter very much to the professional readership, who for the most part will have an institutional subscription to a relevant CUP bundle. But anyone who doesn't have such access might very well feel pretty disgruntled, with some justification, paying for a book that is significantly compromised in not providing the production values that the authors clearly expected. Come on CUP (and IAU), you really should be doing better than this. — Ian D. Howarth.

AGN Winds in Charleston (ASP Conference Series, Vol. 460), edited by G. Chartas, F. Hamann & K. M. Leighly (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 279, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 802 2).

Continuing the ASP series comes this volume providing the presentations from the Charleston meeting held in 2011 October. With the usual mix of review-type articles and short contributed papers, the volume summarizes what must have been a very interesting conference.

The subject of AGN winds has taken on a new life in recent years with the discovery of fast, highly-ionized, highly-energetic X-ray winds, some reaching velocities of a few tenths the speed of light. These winds carry a significant amount of energy, comparable to the radiation budget, and provide a direct view into the central powerhouse of the AGN, a view more traditionally associated with the study of AGN relativistic jets. X-ray winds also provide a possible, although not entirely clear, way to feedback energy and mass from the central object into its surroundings. A feedback process seems to be required to solve the astronomical equivalent of the chicken-and-egg question: how do you grow galaxies and supermassive black holes together? The topics of X-ray winds, feedback, accretion-disc physics, and outflows viewed on larger scales are discussed in the multiple papers presented in this slim but comprehensive volume. While astronomers have clearly made excellent use of the current facilities, the results shown here provide a powerful argument as to why we need to start building the next generation of X-ray observatories.

This volume provides an excellent summary of a growing field of research and I recommend it to anyone working in this area. — PAUL O'BRIEN.

Cosmic Masers — from OH to H_0 (IAU Symposium No. 287), edited by R. S. Booth, E. M. L. Humphreys & W. H. T. Vlemmings (Cambridge University Press), 2012. Pp. 536, 25 × 18 cm. Price £76/$125 (hardbound; ISBN 978 1 1073 0284 2).

The 4th IAU symposium since 1992 on cosmic masers was held in 2012 January 29 – February 3 in Stellenbosch, South Africa, and was opened by Dr. Bernie Fanaroff, the Director of the South African *Square Kilometre Array* Project. The subsequent announcement of South Africa as the main site for *SKA* Phase 2 sets the science results and challenges stated in these proceedings in their proper place; *SKA* Phase 2 will be able to image many of the maser transitions discussed, both at the present epoch and at very high redshift, with the huge ultraviolet coverage finally allowing maser emission both to be mapped completely by interferometers and detected at high redshift.

There were 110 participants at the conference with 68 oral papers and 45 poster papers presented. These contributions were in the areas of 'Advances in maser theory'; 'Polarization and magnetic fields'; 'Star-formation masers — their variability and other properties'; 'Stellar masers'; 'Maser surveys'; 'Cosmology and the Hubble Constant'; 'AGN and megamasers'; 'Maser astrometry'; 'New masers and further developments in maser physics'; and 'Masers and the impact of new facilities'.

The high brightness temperatures of masers, which allows very accurate and high-spatial-resolution imaging of the emission by interferometers, demonstrated their huge usefulness in all the observational talks by allowing molecular-line emission to be detected in the first place in surveys, proper-motion measurements of maser emission, position–velocity modelling of data such as rotation, infall, and outflow (resulting in a new word to this reviewer

— *excretion*), and high-signal-to-noise measurements of polarized emission, especially Zeeman pairs, which are allowing high-spatial-resolution maps of magnetic-field structures to be made.

The highlights are the advances made in (*i*) astrometry and the increased understanding of dynamics in star formation (Keplerian rotation in discs), AGN, and stellar outflows, (*ii*) the huge amount of methanol-survey data and its importance in understanding high-mass and low-mass star-formation physics, and (*iii*), and most exciting, the measurement of magnetic-field strengths in detail using polarized maser emission. The latter are crucial if we are to understand the role of magnetic fields in shaping inflows and outflows and how shocks affect the interstellar medium.

The challenges still remain in our inability to model accurately the effects of exponential gain on the spatial and spectral appearance of masing regions. Coupled to this is how to model the onset of saturating effects. This remains a numerical problem and how to solve this is excellently stated in cookbook-style in the proceedings. The highlight from this meeting is the advances in constructing pumping schemes from detailed simulations of maser regions. This approach will at least give greater insight into the physics of maser regions, while we continue to work on true 3-D simulations of masers.

These proceedings show a vital community that is looking to the future and present an outstanding set of scientific advances, which are based upon that great attribute of masers: if you want to image molecules, and what they are up to, below an arcsecond in angular resolution, and certainly below 10–100 mas, then masers are your only guide. They still remain one of astrophysics' great observational tools and play an increasing role in modelling and simulation work. With the *SKA* Phase 2 only ten years away, the future for masers is very, very bright indeed. — JEREMY YATES.

Advances in Computational Astrophysics: Methods, Tools, and Outcomes (ASP Conference Series, Vol. 453), edited by R. Capuzzo-Dolcetta, M. Limongi & A. Tornambè (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 413, 23·5 × 15·5 cm. Price \$77 (about £50) (hardbound; ISBN 978 1 58381 788 9).

This proceedings volume covered a conference on advances in computational astrophysics. It was the sixth in a series of astronomy and astrophysics workshops held in Cefalù, Sicily, devoted to hot topics in the whole field of modern astrophysics. Although this book focusses on computational astrophysics, the goal was to bring together those who use computational techniques to solve scientific problems and those who primarily develop computational methods and techniques. Reading this proceedings volume suggests that they achieved this, as there is a very nice mixture of contemporary astrophysics and papers devoted to the various computational methods.

I suspect that I should probably stop agreeing to review conference proceedings as it's taking longer and longer for me to get my reviews back and I have less and less time actually to read the papers. I did, however, quite enjoy reading some of the papers in this volume. There were good reviews on star-formation simulations (which are now becoming more and more realistic), supernovae, and on compact interacting objects and gravitational waves. I found some of the papers on Type Ia supernovae particularly interesting, especially given that there still seems to be a great deal of uncertainty about the origin of these supernovae, and yet a recent Nobel Prize in physics was awarded for work that relied extensively on them. Similarly, there were interesting papers about interacting compact objects. I don't think that I actually knew of the possibility of naked singularities, until I read this volume.

There were also a number of good papers discussing computational methods and techniques including some papers on new techniques, one of which — amusingly called BONSAI — is a Tree-Code method that uses Graphics Processing Units (GPU). It seems that GPUs are starting to be used quite extensively to solve problems in computational astrophysics. Also, whenever I review one of these volumes, there always seems to be a paper by Dan Price with an amusing title and with some fiery rhetoric in support of Smoothed Particle Hydrodynamics (SPH) — in this case, a paper titled "Smoothed Particle Hydrodynamics: Things I wish my mother taught me." It made a convincing argument that SPH does better than many think; but also had a suitable running title of "SPH: When you should, when you shouldn't".

All in all, I thought this was a very good volume — a number of fine papers across a wide range of areas, and a nice mix of technical papers together with papers using those techniques to address interesting scientific questions. — KEN RICE.

Observing the Solar System: The Modern Astronomer's Guide, by G. North (Cambridge University Press), 2012. Pp. 489, 25·5 × 19·5 cm. Price £30/$48 (hardbound; ISBN 978 0 521 89751 8).

Conceived as a companion to the same author's *Observing the Moon: The Modern Astronomer's Guide* (2nd edition, 2007), this substantial and nicely produced volume provides similarly practical advice for the amateur astronomer wishing to conduct systematic observation of other Solar System objects. Topics covered include observation of the Sun, the Moon, the major planets, asteroids and minor planets, and comets, as well as phenomena closer to home such as aurorae, noctilucent clouds, and meteors.

The author is a practical observational astronomer of long experience, and in this book he tries to show how the traditional skills of telescopic observation may be combined with new and more sophisticated techniques made available to the amateur as a result of rapid and exciting technical developments over the past decade or so. Thus North provides a hands-on guide to the practical skills of visual observation and sketching while simultaneously acknowledging the new possibilities opened up by high-resolution imaging. He also considers, especially in the chapter on the Moon, how telescopic work may be supplemented by the analysis of spacecraft imagery and datasets now freely available to the amateur *via* the internet.

The book is engagingly written, and the author's enthusiasm constantly shows through. His practical guidance on the techniques of observation is supported by brief overviews of essential information about the celestial bodies under consideration. In some respects, this struck me as an 'old-fashioned' book, but that is not necessarily a bad thing — the modern observer can learn much from North's appreciation of past observational tradition.

As well as a comprehensive treatment of the sorts of telescopes (and binoculars) best suited to Solar System observation, North also provides sound guidance on optical collimation and testing, as well as in the use of eyepiece types, filters, and other observational accessories. He is perhaps less secure in his treatment of imaging. This reviewer felt that perhaps too much space is devoted to techniques involving single-shot CCDs/DSLRs, which are appropriate for some types of Solar System object (*e.g.*, comets and asteroids), but less so for the majority. Conversely, there is perhaps not enough depth to the treatment of high-resolution imaging using high-speed planetary cameras. True, North provides walk-through tutorials of basic techniques in the capture, stacking, and processing of such images, but it is easy to reach a ceiling quite quickly in this field and I for one longed for more insight into the sorts of image-processing techniques that make the difference between a competent image and an outstanding one.

There are a few minor typos and errors, but on the whole this is an attractive book that will serve as a useful guide to those about to enter this complex field. — BILL LEATHERBARROW.

OTHER BOOKS RECEIVED

Hinode-3: The 3rd Hinode Science Meeting (ASP Conference Series, Vol. 454), edited by T. Sekii, T. Watanabe & T. Sakurai (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 469, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 790 2).

Appearing somewhat *after* the proceedings of the 4th and 5th *Hinode* Science Meetings (see **133**, pp. 49 & 50), this volume records the discussions of a conference held in Tokyo in 2009 December. A wide range of solar topics was discussed in addition to early results from the satellite, from the Sun's interior to the solar wind.

Here and There

SOMETHING OF AN OWN GOAL

The detail in the atmosphere of Uranus was revealed by using filters and stacking images to reduce the signal-to-noise ratio. — *A&G*, **53**, 6.7, 2012.

BACK TO THE FUTURE

Had Challis proceeded with vigor and vitality, he most assuredly would have found Neptune, for it was there on his photographic plates. — in G. R. Fowles & G.L. Cassiday, *Analytical Mechanics, 7th Edition*, 2005, p. 242.

PERHAPS IF ONE LEANS OVER

... Monoceros the Unicorn, now visible directly overhead. — *The Daily Telegraph*, The Night Sky for January.