THE OBSERVATORY

Vol. 136 2016 AUGUST No. 1253

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2016 February 12 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

M. A. Barstow, *President*
in the Chair

The President. Good afternoon. We are fast approaching the time when we have to produce our annual report and accounts and prepare for the AGM that is held in May. You will recall that we always appoint two Fellows, not being members of Council, to be the honorary auditors of the Society. They are directed by the bye-laws to develop a personal report on resources, goals, structures, activities, conduct, and general health of the Society, but not matters relating to finance, law, or personnel, to the AGM. So this year, Professor Paul Crowther and Dr. Euan Monaghan will undertake this role and would welcome any input from members of the Fellowship.

It's obviously a very exciting day for astronomy. There are a number of talks this afternoon and the first one is from the holder of the RAS 2015 Group Award for Astronomy, a group led by Dr. Simon Garrington from the University of Manchester, who is going to talk about 'High-resolution radio imaging with *e-MERLIN*'.

Dr. S. Garrington. It is an honour to accept the RAS Group Award to *e-MERLIN* on behalf of the whole *e-MERLIN* team.

The theme of this talk is the continuing importance of high-resolution observation and mapping especially in the field of cosmology. Much of the effort to achieve high resolution goes back to the early days at Jodrell Bank. It is now just over 70 years since Lovell and others came back from the war and began to carry out experiments in radio astronomy. In ten years a whole new science had grown up, yet the nature of discrete compact radio sources remained a mystery throughout the building of the *Lovell Telescope*. Another team, building an interferometer with a 100-km baseline, found that those sources still remained unresolved. The key was more resolution, and the answer when it came was that they were not stars but quasars.

Early experiments into long-baseline radio astronomy with a network of telescopes were folded into *MERLIN*. We were able to achieve the same resolution as the *HST* but at radio wavelengths, but this involved a whole new set of technical challenges. The main problem was to make *e-MERLIN* phase stable as it is phase information which helps to make images. In order to make images from a sparsely-spaced radio-telescope array, reconstruction techniques were needed. To detect more-typical radio-quiet AGNs or star-forming galaxies, 18 days of integration were required, but with a 200-km baseline we could resolve many of the objects that appeared in the Hubble Deep Field.

Having stretched *e-MERLIN* to get improved resolution, the available bandwidth was then the limiting factor in sensitivity. To increase bandwidth to the required 150 GBits per second meant fibre-optic links and, although ten years ago there were some commercially available optical cables which we could use, we still had to excavate 100 km of channels in which to install our own cables. We also had to replace virtually everything else — new receivers and telescope-drive-control systems, a brand new correlator, and data-acquisition software. Only the basic structure of the telescopes remained unchanged.

We continue to develop new amplifiers and receivers to increase sensitivity still further, especially in the C-band (at 428 GHz) and the K-band at higher frequencies. The first image using all the telescopes and the new system came in 2011 when we imaged a gravitational-lens system which had been discovered at Jodrell Bank about 40 years ago, and full production was achieved a year later.

We are certainly achieving expected sensitivity and image quality by completely filling the aperture plane. Because we have a range of apertures from 76 metres to 25 metres then the primary-beam field of view depends on the baseline, so we need to take care in making wide-field mosaic images. We currently have 12 legacy programmes in place covering the whole range of astrophysics. On nearby galaxies such as M 82, for example, we can see right into individual components which produce radio emission. Other programmes include PEBBLeS (Planet Earth Building Blocks Legacy *e-MERLIN* Survey) led by Jane Greaves in which we are looking at cm-emission from particles in planet-forming regions around young stars where we need a combination of sensitivity and wavelength. e-MERGE (*e-MERLIN* Galaxy Evolution survey) is a programme whose aim is to investigate star-forming regions in galaxies and in AGN at high redshift. We are concentrating on a field about 30-arc-mins square on which we have spent 20 days of integration. This reaches a sensitivity below 1 microJansky and we expect to detect 5000 objects.

e-MERLIN was also used to produce accurate radio astrometry which helped to align the radio and optical reference frames to better than 1 mas.

Looking forward we are continuing to develop the network of telescopes. This involves a massive programme by Manchester University to improve the *Lovell Telescope* by replacing the original surface and the telescope foundations, and working on the telescope structure. There is also a scientific upgrade to expand the field of view for the telescope and *e-MERLIN*.

The *e-MERLIN* network can be used in conjunction with other European telescopes to give us available baselines from 10 km to 10 000 km. We are considering increasing the bandwidth to 6 GHz (100 GBit per telescope), and by replacing the broadband receivers we hope to increase the sensitivity in the S-band by a factor of 3.

The success of *e-MERLIN* is predicated on achieving 0·1-arc-second resolution and imaging at 1-cm wavelength to resolve planets in formation, H II regions and supernovae in nearby galaxies, star-forming regions in distant galaxies, and do weak-lensing experiments. It will continue to play a role as we enter the epoch of *SKA* over the next decade. *e-MERLIN* is a unique UK facility with similar physical scale and frequency range to *SKA*.

The President. We've a few minutes for questions.

Professor S. Miller. There have been some proposals to look at exoplanets — radio emissions from exoplanets. Do you know if they have got anywhere at all? Particularly, I think people were hoping to be able to look for exoplanet magnetospheres in some of the giant, hot, super Jupiters.

Dr. Garrington. I think people have looked and thought about doing that at lower frequencies. Perhaps with *LOFAR*; we know that some of the emission we see from Jupiter is very low frequency so I know that people have thought about doing low-frequency experiments. The other part of it, the other exoplanet connection, is the work that I showed you from Jane Greaves and her group looking for planets and at the process of formation. I think those are the two key exoplanet experiments. There are also radio-astrometry exoplanet observations and they complement radial-velocity measurements in other exoplanet searches. So with sub-milliarcsecond astrometry, it's certainly possible to do experiments like that.

The President. Any other questions?

Dr. D. McNally. How do you now cope with the radio noise that is generated by the rest of the community?

Dr. Garrington. It's a good question. Parts of the spectrum are allocated exclusively to radio astronomy. There are bands which are purely passive bands that are used by radio astronomers and also the Earth-observation community. They are very strongly protected. I was in meetings last week with *Iridium* scientists about how one can challenge any infringement of the allocation to passive bands. Of course, we also want to observe outside some of those bands and we are able to do that with differing degrees of success in different parts of the spectrum. It is a challenge to continue to do radio astronomy in an era where there is greater and greater pressure on the use of the spectrum and demand for the spectrum. One advantage in long-baseline astronomy is that there is more immunity from local sources of interference since we're always measuring the correlation between pairs of telescopes and local sources of interference can be less of an issue. Sources like satellites, of course, can cause strongly correlated interference even on very long baselines.

The President. Are there any more questions? Let us thank our speaker again. [Applause.] I don't know which bit of the planet you'd have to be hiding on to have failed to notice yesterday's announcement. Given that we now usefully have a talk on gravitational waves you might have also thought that, for once, we might have known what we were doing in planning the programme. [Laughter.] So I'm really pleased to invite Professor Alberto Vecchio of Birmingham University to give a talk: 'Searching for gravitational waves'. Of course we now know the outcome of that search, at least in one example. So, over to you.

Professor A. Vecchio. [No summary of this talk had been received at the time of going to press; readers may be able to listen to the talk *via* www.ras.org.uk/events-and-meetings/ and following the links.]

The President. We have some time for questions, but as we've got to fit another talk in we can't spend too much time on this. I'm going to exercise President's privilege by asking the first one. If *VIRGO* had been operating at the same time, how much better positioning would you have been able to achieve?

Professor Vecchio. The question is exactly at which sensitivity, of course. The area that I'm showing [on the screen] is affected by our calibration errors that we have right now. So if there is 10% in amplitude and 10% in phase, we should be able to recalibrate the data and shrink this area by about a factor of 3. With *VIRGO*, probably this area would go down by another factor of 10 or so.

The President. Anybody else?

Professor O. Lahav. First, congratulations on this result. I know there have been electromagnetic follow-ups, so could you comment first on expectations of how black-hole–black-hole systems would compare with, say, neutron-star–neutron-star binaries electromagnetically, and also give a summary of what's been done?

Professor Vecchio. First of all, it's a black-hole–black-hole merger, so in the standard scenario we expect nothing, although I know there are non-standard scenarios where we may be able to extract electromagnetic radiation from such a collision; but in the standard scenario this is completely silent. This signal has been observed in real time, and within — I don't remember exactly the time — I think two days. It was sent to 62 observatories that had an arrangement with the *LIGO* collaboration to do follow-up observations. They range from X-ray, gamma ray, optical, to radio, thus covering the entire spectrum. There is a paper which is about to be made public but there were political issues in sharing the results with different groups. I think I can say — well I don't know what I can say [laughter] — that there is no strong candidate because the area is very big. But the paper should appear very soon.

Professor D. W. Kurtz. The energy release you put up of $2M_{\odot}c^2$ is phenomenal, but the pulse is incredibly short. Let's imagine we had a circumbinary planet at 1000 AU — what happens to it? The flux is phenomenal.

Professor Vecchio. The flux is phenomenal and almost isotropic and the only thing that happens is a fluctuation in the orbital separation of this planet with respect to the star it is orbiting. So the orbit of the Earth around the Sun changed at peak simply by the size of one nucleus.

Professor Kurtz. Yes, but what if you are close?

Professor Vecchio. Oh, you mean around the black holes! A thousand AU from the event. I actually don't know. But I would assume...

Professor Kurtz. I'd be scared to be there. [Laughter.]

Professor Vecchio. If you are at 1000 AU, I don't know whether you would actually plunge into the black hole within a Hubble time if you were a planet. I'd have to do a calculation.

The President. A bit of stretching going on I would have thought. [Laughter.] We're all stunned by this, I think. Do you think going back through the data, from what you've learned now, you may find there were gravitational waves that you missed previously? Because we've seen this happen where we've had a discovery and gone back to find it had been missed before.

Professor Vecchio. You mean going back to the data collected previously? Well, gravitational waves were there for sure. This signal which is really, really large, would have been missed by the first-generation instrument. The signal-to-noise ratio would be about three or four. So this is the first generation of instruments here [on the screen] and this is where we are now; all the signal is in this area here, so you have a factor of between 10 and 20 in amplitude. Although there is a chance that we've missed something, I don't think anyone would go back right now. But the data are now public, so I think everybody can go back if they want to.

The President. I think we should thank Alberto again. I think it's a privilege to have been here at this particular talk. [Applause.] Our next talk is very important: the Harold Jeffreys Lecture for 2015. Slightly unfortunately, it's got a rather hard act to follow but I'm sure it's going to be equally impressive — not perhaps dealing with gravitational waves but more about gravity. Professor Tony Watts is going to talk about 'Plate flexure and its implications for geological processes'. Plate flexure is a phenomenon that is manifest in a wide range of geological processes including volcanism, sedimentation, erosion, and extensional and compressional tectonics. So there's a bit of strain going on there as well. By comparing the surfaces of flexure generated by the shifts in loads associated with these processes, we have learned a considerable amount about the rheology and behaviour of planetary lithospheres on both short and long time scales.

Professor A. Watts. [It is expected that a summary of this talk will appear in a future *A & G*.]

The President. Unfortunately, we're a bit short of time but I can take one question.

Rev. G. Barber. Would an ice cap add significant load, and at the end of an ice age on what sort of timescale would the crust rebound?

Professor Watts. Yes, they do indeed. They're quite short-term loads so relatively young; for example, since the Pleistocene, the last ice age was about 20 000 years ago. We are still seeing the recovery from the melting of that ice today in Canada. Canada is rising, planking where the ice load was, but where the bulge was is now sinking. It is spread over the planet, and we are still seeing the response from the melting which began around 18 000 years ago. The ice has now receded in North America to a few islands in the north and we're seeing the response to that unloading today. That gives you the kind of idea of the time-scales involved in that.

The President. Can we thank all of our speakers again? [Applause.] You probably don't need any reminding that there is a drinks reception in the RAS library immediately. I'd like to give notice that the next monthly A&G Open Meeting of the Society will be on Friday, 11th March 2016.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2016 March 12 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

M. A. BARSTOW, *President*
in the Chair

The President. I'm delighted to welcome two RAS Fellows (in the funded sense) to address us today. Most of us, if not all of us, in this room are Fellows of the Royal Astronomical Society but we also fund competitive fellowships and I'd just like to say a word or two about those. They were originally designed to fill the hole left when the STFC, during its financial crisis, withdrew its postdoctoral fellowship scheme. The crisis hasn't ended so we're still funding them. The quality of applications is absolutely, fantastically high. You're probably going to hear from two of the best scientists in the country this afternoon, so don't modulate your expectations. [Laughter.] The first of these is Dr. Sarah Badman from Lancaster University, who was awarded her RAS Fellowship for 2012 to 2015. She's going to talk about 'The aurorae of Jupiter and Saturn'.

Dr. Sarah Badman. My fellowship work has focussed on the aurorae of the giant planets Jupiter and Saturn. The aurorae are emissions of light from a planet's upper atmosphere, caused by charged particles crashing into the atmosphere and exciting atmospheric particles. Because the charged particles are guided into the atmosphere along magnetic-field lines, the aurorae provide a snapshot of what's happening in the local magnetic environment. Two studies of variability in Jupiter's and Saturn's aurorae will be described.

The motivation for this work is to understand how the Sun affects the planets in the Solar System. The Sun's atmosphere is constantly streaming away into space at a speed of about 400 km s^{-1}, and it carries the solar magnetic field with it. This flow of particles through the Solar System is known as the solar wind. Magnetized planets form obstacles to the solar-wind flow. Their magnetic fields carve out a region in space known as the magnetosphere. However, some of the solar-wind particles can enter the magnetosphere and, at the Earth at least, these particles are injected towards the planet to create bright aurorae. Different auroral features represent different regions of the magnetosphere and we can use auroral images to see how they vary spatially and temporally.

Jupiter's magnetosphere is a very different environment from that of the Earth. Not only is it the largest planet in the Solar System, it is the most rapid rotator, with a rotation period of less than 10 hours. It has a strong magnetic field and important sources of mass inside its magnetosphere: the moons.

The moon Io is the source of most of the plasma in Jupiter's magnetosphere. It is the most volcanically active body in the Solar System, releasing about a tonne every second to local space. Some of this matter is ionized to form plasma and becomes bound to the planet's magnetic field. As the plasma is spun up to rotate with the magnetic field around the planet it experiences a centrifugal force driving it radially outward and stretching the magnetic-field lines. To conserve angular momentum the plasma slows as it moves out and the field lines become bent out of the meridian planes. Angular momentum is transferred from the rotating planet to the magnetospheric plasma *via* a system of field-aligned currents to spin the field and plasma back up to rotation with the planet. The portion of the current directed upward from the planet is carried by down-going electrons which excite the atmosphere to cause Jupiter's main auroral oval.

This system of currents, and the resulting auroral intensity, can be modified by effects either internal or external to the magnetosphere. A change in the amount of plasma-loading from Io could affect the strength of the current needed to spin-up the plasma to rotation with the planet. However, if the magnetosphere were compressed by a high-pressure region in the solar wind, the stretching of the magnetic-field lines and the strength of the current would be reduced.

Using observations of Jupiter's aurora taken by the *Hubble Space Telescope* these variations can be investigated. Fourteen auroral images were taken over an interval of sixteen days in 2014 January. The intensity of the main auroral oval decreased by 70% over the course of those observations. Consideration of what drives the auroral currents shows that they can be caused by a 70% decrease in the auroral electron density or an order of magnitude increase in the electron thermal energy. The former could be caused by an expansion of the magnetosphere under low solar-wind-pressure conditions. The latter could be related to an increase in hot plasma injected through the corresponding region of the magnetosphere, and the auroral signatures of such hot plasma injections were indeed identified during the interval. In either case, there was no evidence for a change in the loading of Iogenic plasma, or a compression of the magnetosphere — the two scenarios previously anticipated.

Saturn is another rapidly-rotating gas giant, with a significant source of plasma inside its magnetosphere: the cryovolcanic moon Enceladus. A wealth of observations of Saturn's aurora has been provided in recent years by the *Hubble Space Telescope* and the *Cassini* mission. One of the intriguing features of Saturn's magnetosphere is that virtually all parameters (*e.g.*, magnetic-field direction, plasma density) oscillate with a period close to that of the planet's rotation, although the periods are slightly different in the northern and southern hemispheres and vary slowly over time. One visible effect of those oscillations is that the location of Saturn's main auroral oval rocks back and forward with a period close to 11 hours.

In spring 2013 *Cassini* was in an inclined orbit, flying over Saturn's northern and southern auroral regions. A sequence of auroral images were taken on April 21 and 22 by both the *Hubble Space Telescope* and instruments onboard *Cassini*. At the start of the interval the auroral oval was relatively narrow and displayed the expected rocking behaviour just described. Several hours later, *Cassini* detected an intense burst of radio emission extending to lower frequencies. This is known to be a signature of the high-pressure region in the solar wind impacting on the magnetosphere. A bright bulge appeared along the nightside aurora, and the auroral oval broadened towards the pole over the following few hours. This motion disrupted the regular rocking of the auroral oval expected over this time. *Cassini* flew through the magnetic-field lines connected to the nightside auroral oval and measured the field-aligned current connected to the aurora to be nearly twice as strong as it was on the previous day. This study shows that compressions by the solar wind, *i.e.*, an external factor, influence the location and intensity of Saturn's aurora.

In summary, auroral images provide a valuable snapshot of global magnetospheric dynamics. By studying sequences of images, the relative influence of the solar wind on the auroral morphology can be investigated to address the question of how the Sun affects planetary environments.

The President. We have a few minutes for questions, if anybody has any?

Dr. S. Haaland. On the Earth you have aurorae in the north and in the south. I presume you have similar features on the planets, and if yes, are they symmetric? Are they correlated?

Dr. Badman. That's a good question — yes, there are auroral ovals both at the north and at the south. In Jupiter, they're definitely not symmetric. Remember Jupiter had that kind of kidney-bean-shaped auroral oval in the north whereas in the south it's much more circular, and that suggests there's some magnetic-field anomaly that is distorting the auroral oval in the north. At Saturn they're more symmetric, but actually one of the studies for which we're trying to do some observations this year is have *Hubble* look at the north whilst *Cassini* is looking at the south and actually study how symmetric they are, hence what's causing the precipitation in both hemispheres.

Mr. E. Carpenter. Are there aurorae at Uranus and Neptune?

Dr. Badman. Yes. They've been seen by *Voyager* and also very faintly by Earth-based telescopes, but they are pretty difficult to see from the Earth.

Dr. Lyndsay Fletcher. Do you have simultaneous imaging of the aurora in different wavelengths and can you tell if there's offset between the locations at different wavelengths, in particular, X-rays *versus* optical?

Dr. Badman. It has actually proven incredibly difficult to get simultaneous observations at different wavelengths but we do have them thanks to *Cassini*, particularly at Saturn. There are some different features which suggest, for example, that the infrared emission responds to the temperature of the atmosphere so therefore it can have a slightly different profile to the ultraviolet precipitation. Will Dunn, who is sat in front of you there, is the expert on the X-rays which we have seen from Jupiter. They do line up quite well with the ultraviolet. It's not exactly the same emission mechanism.

The President. Any more questions? Otherwise, let's thank Sarah again. [Applause.] Our second RAS Fellow this afternoon, who was also funded from 2012 to 2015, is Nick Wright from Keele University, and he's going to talk to us about 'The dynamics of star clusters'.

Dr. N. Wright. Star formation is one of the fundamental processes in the Universe, impacting cosmic re-ionization, the structure and evolution of galaxies, the synthesis of elements, and the formation of planets. Since most young stars are distributed in groups or clusters of some sort, understanding the origin of this clustering and the formation of bound open and globular clusters is vital for a complete theory of star formation. The clustered environment of young stars also impacts the formation of planets, through UV photoevaporation from nearby massive stars and close encounters between stars, proto-planetary discs, and young planetary systems. Star clusters are also a fundamental astrophysical tool in their own right, acting as a simple stellar population to study stellar evolution and star formation in distant galaxies.

A transformational improvement in kinematic data quality is imminent, allowing us for the first time to resolve cluster kinematics for large numbers of stars in 3D, providing estimates of energies, angular momenta and dynamics, and avoiding the need to make crude isotropy assumptions. This change is driven by data from current radial-velocity (RV) surveys (*e.g.*, the 300-night *VLT Gaia* ESO Survey (GES)), and new-generation astrometric facilities (*Gaia*).

Recent RV studies from surveys such as GES have already revealed much about the dynamical state of young stellar groups and clusters. For example, while many young clusters appear to be in virial equilibrium, observations show that the pre-stellar cores in such regions actually have a sub-virial velocity dispersion. This has led to suggestions that stellar groups may be born with subvirial kinematics, from which they then collapse under gravity and form dense clusters. Kinematic observations have also revealed that many groups have hints of kinematic substructure in the form of spatially-varying kinematics or evidence for multiple structures with distinct kinematic signatures but superimposed along the line of sight.

While RV studies are useful, proper motions (PMs) provide a much more valuable view of the kinematics of a group of stars. This is partly because they provide two dimensions of kinematic information, but most importantly because they probe a dimension that can be studied spatially. With spatial and kinematic information in the same dimension, the relative motions and accelerations of stars can be studied, and thus the physical processes acting on them can be exposed.

Gaia will provide PMs for the billion brightest stars in the sky, which will revolutionize kinematic studies of star-forming regions and star clusters. While intermediate data releases are expected every year, the full release of *Gaia* data isn't due until 2022. However, it is possible to calculate high-precision PMs using existing data, specifically by exploiting the wealth of wide-field-imaging data available from telescope archives around the world.

We have been using these data to carry out a three-dimensional kinematic study of the massive OB association Cygnus OB2. OB associations are interesting because they are thought to be the expanded remnants of dense star clusters that have been disrupted by processes such as residual-gas expulsion. Cyg OB2 is an excellent target to test this theory because it is both relatively nearby (at a distance of only 1·4 kpc), but also sufficiently massive (total stellar mass has been estimated to be about 20 000 solar masses) that there are enough members of the association to study it in sufficient detail.

Our kinematic survey is targeting approximately 4 000 X-ray and spectroscopically selected members of Cyg OB2. RVs are taken from a 10-night *MMT/Hectospec* survey and PMs calculated from wide-field images spanning a 15-year baseline. The PMs were calculated from astrometric measurements of stars in approximately 2 500 images of Cyg OB2, with each star typically detected about 50–100 times. This results in a PM precision of as high as 0·3 mas yr^{-1} for the brightest sources, and a median uncertainty of 0·6 mas yr^{-1}. This equates to a kinematic precision of 4 km sec^{-1} at the distance of Cyg OB2.

Our initial study was limited to the central part of the association that has been studied in the most detail, and which includes ~ 750 members. The 3-dimensional velocity dispersion of the association is measured to be 18 km s^{-1}. This implies that the association is gravitationally unbound, since the virial mass is an order of magnitude larger than the known stellar and gas mass. The PMs, however, do not reveal any preferential expanding motions, with only 50% of the radial component of the kinetic energy in the form of expansion. This suggests that the association was not unbound by processes such as residual-gas expulsion (that predict a radial dispersal of stars) and was not a dense, gravitationally bound star cluster in the past. This challenges the classical view of OB associations that seeks to explain them as dispersed star clusters.

This picture is supported by the presence of considerable kinematic substructure in the PMs that suggests the association is dynamically unevolved. The substructure is visible both from PM vector maps and from statistical spatial correlation tests that show to a very high significance (>5 sigma) that the PMs are spatially correlated, *i.e.*, that stars closer together on the sky have more similar motions than those further apart. This leads to a number of interesting conclusions regarding the current and past evolution of the association, and clearly shows that the association was not a single dense star cluster in the past, but was most likely a collection of smaller groups or clusters that are now visible as these kinematic substructures.

To conclude, we are entering a golden era in dynamical studies of star clusters. A wealth of kinematic data is on the horizon, thanks to both large-scale spectroscopic surveys and next-generation astrometric missions. As a preview to what these data will soon be capable of, we have used existing wide-field-survey imaging data to conduct a kinematic study of the massive OB association Cygnus OB2, showing it to be dynamically unevolved and lacking the kinematic signature expected if it were a dispersed star cluster.

The President. *Gaia* does have radial-velocity capabilities, so would that not have any use for you because the stars are too bright?

Dr. Wright. Actually it's because our stars are mostly too faint. *Gaia*'s radial velocities will go out only to 16th magnitude in the *B* band, and even then they'll only reach about 5 or 10 km s^{-1} so it's going to be useful, I think, for bulk galactic archaeology kinematics but not star-cluster studies.

Professor M. W. Feast. I may have missed this but have you any radial-velocity information on these different groups you have the proper motions for?

Dr. Wright. We do. We should end up with radial velocities for all those stars for which we have proper motions, but because we want to make sure we haven't contaminated our sample with binaries, we are taking multiple epochs of radial velocities so that we can identify those objects and remove them; that's building up quite a lot of data that we've got to reduce. It should be hopefully done in the next year or two.

Mr. M. F. Osmaston. Do you think our Sun was made in company with much more massive stars and has been ejected from the group? Where would you put our Sun in terms of it's origin?

Dr. Wright. As to the Sun's birth cluster, that's an area where I probably can't give you an educated answer other than to say there has been a lot of work to look for tell-tale signs — for example, assessing what sort of environment our Sun could have been born in based upon the fact that all of our planets still orbit within a plane, and that some of them, Pluto for example, have an eccentric orbit. There's also some evidence in isotopes in meteorites that suggest that the Sun, at some point during the formation of the Solar System, was not too far from a supernova. How you put all that together to identify the birth site of the Sun is for people smarter than I.

The President. Any other questions? If not, let's thank Nick again. [Applause.] The final talk for this afternoon is our 2016 Eddington Lecture which is going to be given by Professor Eric Ford from Pennsylvania State University. It's entitled '*Kepler*, the architectures of exoplanet systems and implications for planet formation'.

Professor E. Ford. [It is expected that a summary of this talk will appear in a future issue of *A & G*.]

The President. Lots of questions as you might expect.

A Fellow. Why do you think, with all these fine observations, we can't find any exomoons?

Professor Ford. Exomoons are actually quite hard to find. If you look at where exomoons would be stable in the sense of things like Hill radii and tidal effects, there's a certain range of orbital separations and mass ratios where they could be stable in the long-term. Only recently, through dedicated searches of the full time-span of *Kepler* data, have we begun to make a significant dent in excluding exomoons that would be likely to survive for long time spans. I forget the exact numbers but my memory says roughly dozens of systems have been searched. While no exomoons have been found, there's over a hundred systems for which the sensitivity based on *Kepler* data is able either to detect or rule out plausible moons with relatively high moon–planet mass ratios. Of course, a Moon-to-Earth mass ratio would be much, much harder to detect. Unfortunately, we don't have the sensitivity to find those with the present techniques for more than a few planets. There are people who are looking and who knows? Maybe next week, we'll hear of an exomoon. I have no inside information about that, but such a discovery would be plausible with the present data.

Professor S. Miller. A very, very interesting talk; thank you. I was interested in your idea about getting over the problem of bringing planetesimals together to get them about a metre-sized and then beyond that by having droplets. So what would these be droplets of? [Laughter.]

Professor Ford. A very good question! In principle, you can get silicates which sublimate at about 1400 Kelvin. Perhaps iron or magnesium silicate — of course that's pure speculation.

The President. I was slightly surprised when you talked about future missions that you've stopped at the *James Webb Telescope*, in the sense that there's a lot of activity going on in the US right now over larger telescopes; there's potentially a bit of a collision coming up between people who want a dedicated exoplanet mission and those who, including me I have to say, want a more general mission that can do exoplanets and other stuff. I'd be interested in your thoughts.

Professor Ford. I mostly listed missions that are really going to happen, where they are cutting metal right now. *PLATO* may not be there quite yet. Of course, there are many astronomers both in the US and abroad who are interested in the possibility of a large-scale, optical, near-infrared-type mission to be able to do exoplanet searches and presumably cosmology and the faint fuzzy stuff that other astronomers like. One challenge is that, at the moment, we still don't know how large a telescope we would need to be able to do something like identify water in an Earth-sized planet in the habitable zone of a nearby star. If it's four metres, then we could do it on the next mission, we'd be very happy and then a lot of possibilities open up. If the answer is sixteen metres, then I suspect many astronomers would like a major mission before we can build a sixteen-metre space telescope. Then we would have to make some trades to figure out the right balance. Unfortunately, despite the great success of *Kepler* being able to find so many exciting planetary systems, due to a variety of factors we don't really know the rate of Earth-like planets in the habitable zone. So, we still are trying to figure out the right telescope to build. That's both a scientific question and a political question. To what extent does it make sense to have a dedicated mission that might be able to reduce costs in some way, but then lose your base of support in science? And to what extent might it be wise to have a more capable mission with more instruments but which inevitably will cost significantly more?

The President. There's one more question there.

Dr. Joanna Barstow. I'm sorry, but I can't remember which *Kepler* system it was now because you showed many and they all have different numbers. The system with the two very similar masses but the outer planet was somewhat larger than the inner planet — is it possible that that's simply because the outer planet still has a hydrogen–helium envelope over something rocky and the inner planet did have one but has lost it because it's closer in? Is that feasible?

Professor Ford. You'd be a great exoplanet theorist, indeed.

The President. She is.

Dr. Barstow. I am.

Professor Ford. OK! [Laughter.] Indeed that was one of the first suggestions to explain this system. I think when this was the sole example we had of this phenomenon, that was a pretty attractive model. The tricky part is that, now we have several systems where we see disparate densities at similar orbital distances. If you tune your knob for 'where photoevaporation kicks in' to match this system, then it doesn't neatly fall between the boundary in all the systems. So it needs to be more complex than just a one-parameter model if that's the explanation.

Dr. G. Q. G. Stanley. With the growing number of systems we're finding, are you able to comment at this stage on which systems are stable, or are they all stable, or which ones are just going to fly apart and this is a lucky time to see them?

Professor Ford. In our analyses, we measure the orbital phase, the orbital period, the duration of the transits, and that gives us constraints on their orbit but it doesn't give us a complete orbit determination. We don't have seven parameters per planet. Typically, what we do is assume long-term orbital stability and use that to help us pin down things like what are the maximum eccentricities allowed and any constraints on the longitudes of pericentre in the systems. Sometimes, the transit-timing constraints are so strong that we actually can pin them down quite precisely. In every case where we've been able to do that, they have permitted solutions that were long-term stable. It's a relatively small fraction, it's just where the error bars are so small that we're able to tell you, "we've figured out a solution and yes it's stable". Typically, we say we assume it's stable and therefore we can gain an extra tool for whittling down parameter space.

The President. Please thank Eric and all our speakers this afternoon again. [Applause.] I have to do the usual business of reminding you that we have drinks in the RAS Library immediately following this meeting and I give you notice that the next monthly A&G Open Meeting of the Society will be on Friday 8th of April 2016. I hope to see you all there.

EARLY-20TH-CENTURY VISUAL OBSERVATIONS OF M 13 VARIABLE STARS

*By Wayne Osborn and E. E. Barnard**
Yerkes Observatory

In 1900 E. E. Barnard published 37 visual observations of Variable 2 (V2) in the globular cluster M13 made in 1899 and 1900. A review of Barnard's notebooks revealed he made many additional brightness estimates up to 1911, and he had also recorded the variations of V1 starting in 1904. These data provide the earliest-epoch light-curves for these stars and thus are useful for studying their period changes. This paper presents Barnard's observations of the M13 variables along with their derived heliocentric Julian Dates and approximate *V* magnitudes. These include 231 unpublished observations of V2 and 94 of V1. How these data will be of value for determining period changes by these stars is described.

Introduction

Observations of pulsating variable stars provide one of the better ways for testing theoretical stellar models and evolution. Evolutionary effects should produce changes in the pulsational parameters of the variables that can be observed. The approach has, until recently, concentrated on seeking changes in pulsational periods. Because the changes are predicted to be small, reliable early observational records are particularly valuable.

*E. E. Barnard passed away in 1923 and, obviously, did not contribute to writing this paper. Nevertheless, he is listed as an author to draw attention to the increasing tendency to list as authors of a paper all who made any contribution to the research, no matter what that contribution may have been. This problem has been discussed by Wyatt[1] and the subsequent comments on his article. The paper of Abdo *et al.*[2] with 454 authors is an example.

The short-period Population II Cepheids, or BL Her stars, have been of particular interest in period-change studies. Such stars are believed to be low-mass stars evolving from the horizontal branch toward the asymptotic branch as their core helium is exhausted, or asymptotic-branch stars undergoing 'blue-ward' loops due to helium-shell flashes as they evolve up the asymptotic branch[3,4]. In either case, the stage of evolution is predicted to produce relatively rapid, and therefore more-observable, period changes.

Many of the known Galactic BL Her variables are found in globular clusters. The cluster M 13 (NGC 6205 = CL 1639+365) has three such stars: V1, V2, and V6. Period-change investigations of these stars[5,6] found little evidence for changes for V1 and V6, but apparently a large period change for V2, the brightest and reddest of the three stars and the one with the longest period. The evidence for the V2 period change largely depended on a set of visual magnitude estimates from 1900 published by E. E. Barnard[7,8]. More recent work[9] suggests small period changes for V1 and V6 as well as the larger one for V2. However, in all cases it is the earliest observations that effectively fix the rates of change. Additional early-epoch observations are obviously of interest.

The Barnard observations

Barnard[7,8] published 37 visual observations of the brightness of V2 relative to a nearby star he identified as Scheiner's[10] number 200. This star is L281 in the more utilized catalogue of Ludendorff[11]. The observations consist of one of the star at maximum made in 1899 and thirty-six magnitude estimates made in 1900. An examination of Barnard's papers, archived at the University of Chicago, revealed a list he had prepared of his observations of M 13 V2. That compilation lists 75 observations of this star in addition to those published — six more near the end of 1900, 40 in 1901, and 29 in 1902. Reviewing his observing notebooks not only confirmed the entries in his list but also revealed many additional observations of V2 made after his summary list was compiled. It was also found that he had made visual estimates of V1 starting in 1904. Barnard alludes to observing V1 in a 1909 paper[12], where he suggests a period for it, but he never published any observations.

V2 observations

Barnard's compilation of his V2 observations lists the date of observation, his estimated magnitude difference between the variable and L281, and the time of observation. The earlier entries agree with the data he published. These are reproduced in Table I along with the corresponding heliocentric Julian Dates and approximate V magnitudes. The Julian Dates have been derived as discussed below. The magnitudes have been determined by adopting $V = 13{\cdot}37$ for the comparison star[13,14,15]. Note that blank lines in all the tables denote the ends of observing seasons.

Table II gives the same information as in Table I for Barnard's observations of V2 that were listed in his compilation but were not published. One sees that the later entries became more detailed. Table III continues the listing with now the data being after Barnard's compilation and taken directly from his observing books. Barnard's handwriting is at times illegible and it is possible a few of the entries have errors from the copying. Julian Dates given to two or three

TABLE I

Barnard's published observations of M13 Variable 2

Date	*Time*	*ΔMag.*	*HJD*	*V*	*Notes*
1899 Aug 14		"Max"	2414880·7	12·3	1
1900 July 9	11:35	0·2	2415210·735	13·2	
1900 July 10	11:10	0·1	2415211·717	13·3	
1900 July 10	14:00	0·4	2415211·835	13·0	
1900 July 11	9:40	1·0	2415212·655	12·4	
1900 July 12	10:10	0·7	2415213·675	12·7	
1900 July 24	10:00	0·1	2415225·668	13·3	
1900 July 24	12:25	0·1	2415225·769	13·3	
1900 July 25	9:15	0·1	2415226·637	13·3	
1900 July 25	10:20	0·0	2415226·682	13·4	
1900 July 26	12:00	0·7	2415227·751	12·7	
1900 July 28	9:15	0·3	2415229·637	13·1	
1900 July 29	9:10	0·1	2415230·633	13·3	
1900 July 29	10:55	0·1	2415230·706	13·3	
1900 July 30	9:45	0·0	2415231·657	13·4	
1900 July 30	12:00	0·1	2415231·751	13·3	
1900 July 31	8:50	0·7	2415232·619	12·7	
1900 July 31	10:10	0·7	2415232·675	12·7	
1900 Aug 1	10:10	1·0	2415233·675	12·4	
1900 Aug 4	8:15	0·1	2415236·595	13·3	
1900 Aug 5	12:00	0·7	2415237·751	12·7	
1900 Aug 6	8:35	1·0	2415238·608	12·4	
1900 Aug 7	8:15	0·5	2415239·594	12·9	
1900 Aug 12	8:35	0·5	2415244·608	12·9	
1900 Aug 13	8:50	0·1	2415245·619	13·3	
1900 Aug 14	8:05	0·0	2415246·587	13·4	
1900 Aug 18	8:25	0·1	2415250·601	13·3	2
1900 Aug 20	8:00	0·1	2415252·583	13·3	
1900 Aug 21	8:40	1·0	2415253·611	12·4	
1900 Aug 27	7:35	0·6	2415259·566	12·8	
1900 Aug 28	9:00	0·3	2415260·625	13·1	
1900 Sept 3	7:20	0·1	2415266·555	13·3	
1900 Sept 3	7:50	0·2	2415266·576	13·2	
1900 Sept 4	7:15	0·1	2415267·551	13·3	
1900 Sept 4	8:00	0·2	2415267·583	13·2	
1900 Sept 19	10:10	0·2	2415282·672	13·2	
1900 Oct 2	6:40	1·0	2415295·526	12·4	3

Notes:
1. Time and *V* estimated. Barnard only noted that V2 was at maximum on this date.
2. This published observation does not appear in Barnard's compilation list.
3. Compilation list has time as 7:00 but published time was 6:40.

decimals have an estimated uncertainty of three units in the last digit; those given to only one decimal are cases where Barnard recorded only the date and the time has been estimated and may be in error by several hours, *i.e.*, 0·2 d.

Table IV continues the listing of the V2 observations but with only the date of the observation and the derived HJD and magnitude given. The light-curve for all the V2 observations is shown in Fig. 1 with the phases computed from the ephemeris of Kopacki *et al.*[16]. Also shown are a few observations derived from yellow photographic plates taken in the same time period, as discussed in the section below on epochs.

TABLE II

Barnard's unpublished observations of Variable 2 from his compilation

Date	*Time*	*ΔMag.*	*HJD*	*V*
1900 Oct 8	6:15	0·6	2415301·508	12·8
1900 Oct 9	6:00	0·1	2415302·498	13·3
1900 Oct 15	6:00	0·1	2415308·497	13·3
1900 Dec 18	18:00	0·1	2415372·997	13·3
1900 Dec 28	17:10	0·5±	2415382·963	12·9
1900 Dec 31	17:05	0·7±	2415385·960	12·7
1901 Jan 14	17:00	0·0	2415399·957	13·4
1901 Jan 15	16:30	0·5±	2415400·936	12·9
1901 Jan 20	16:00	0·6	2415405·915	12·8
1901 July 26	11:00	0·2	2415592·710	13·2
1901 July 26	12:00	0·5	2415592·751	12·9
1901 July 29	8:30	0·6	2415595·605	12·8
1901 July 29	10:50	0·7	2415595·702	12·7
1901 July 30	9:35	1·0	2415596·650	12·4
1901 Aug 5	9:43	0·7	2415602·656	12·7
1901 Aug 5	10:50	0·5	2415602·702	12·9
1901 Aug 7	8:35	0·2	2415604·608	13·2
1901 Aug 12	10:20	0·1	2415609·681	13·3
1901 Aug 13	9:30	0·7	2415610·646	12·7
1901 Aug 19	7:45	1·0	2415616·573	12·4
1901 Aug 20	7:50	0·7	2415617·576	12·7
1901 Aug 24	8:25	0·7	2415621·600	12·7
1901 Aug 25	8:00	1·0	2415622·583	12·4
1901 Aug 26	7:35	0·7	2415623·566	12·7
1901 Aug 26	11:15	0·5	2415623·718	12·9
1901 Aug 27	8:00	0·2	2415624·583	13·2
1901 Aug 27	10:35	0·1	2415624·691	13·3
1901 Aug 31	7:34	0·5	2415628·565	12·9
1901 Sep 1	7:55	0·1	2415629·579	13·3
1901 Sep 10	9:00	0·3	2415638·624	13·1
1901 Sep 22	7:11	0·6	2415650·548	12·8
1901 Sep 30	6:37	0·8	2415658·524	12·6
1901 Oct 1	6:33	0·5	2415659·521	12·9
1901 Oct 13	6:10	0·2	2415671·504	13·2
1901 Oct 21	6:00	0·4	2415679·497	13·0
1901 Oct 22	5:50	0·2	2415680·490	13·2
1901 Oct 29	5:40	0·7	2415687·483	12·7
1901 Nov 4	5:35	1·0	2415693·480	12·4
1901 Nov 4	6:35	0·9	2415693·521	12·5
1901 Nov 5	5:30	0·7	2415694·476	12·7
1901 Nov 5	6:00	0·8	2415694·497	12·6
1901 Nov 12	6:20	0·3	2415701·511	13·1
1901 Nov 12	6:30	0·1	2415701·518	13·3
1901 Nov 16	5:53	0·2	2415705·493	13·2
1901 Dec 16	18:30	0·5	2415736·018	12·9
1901 Dec 17	17:45	0·1	2415736·987	13·3
1902 Jan 2	17:00	0·1	2415752·956	13·3
1902 Jan 12	16:30	0·2	2415762·936	13·2
1902 Feb 7	16:20	0·2	2415788·930	13·2
1902 Feb 8	15:30	0·8	2415789·895	12·6
1902 Feb 15	15:45	0·1 br than 200	2415796·906	13·3
1902 Feb 24	13:35	1·0	2415805·816	12·4
1902 Feb 24	17:40	1·1	2415805·987	12·3
1902 Mar 17	13:00	0·3	2415826·793	13·1
1902 Mar 17	15:10	0·7? or 0·3	2415826·883	13·1
1902 Mar 18	13:10	0·2 or 0·3	2415827·800	13·1
1902 Mar 24	13:09	exactly	2415833·800	13·4

TABLE II (*concluded*)

Date	*Time*	*ΔMag.*	*HJD*	*V*
1902 Mar 24	15:15	0·1	2415833·887	13·3
1902 Mar 24	15:45	0·1	2415833·908	13·3
1902 Mar 25	13:05	0·1	2415834·797	13·3
1902 Mar 25	15:20	0·1 unchanged	2415834·891	13·3
1902 Apr 8	11:35	0·1	2415848·735	13·3
1902 Apr 8	15:10	0·2 or 0·3 seems brighter	2415848·884	13·1
1902 Apr 8	15:55	0·1 or 0·2 can't say it is brighter	2415848·916	13·2
1902 Apr 13	11:50	0·1 or 0·2	2415853·746	13·2
1902 Apr 14	11:10	0·1 faint.	2415854·718	13·3
1902 Apr 14	15:30	0·05 almost exactly	2415854·898	13·3
1902 Apr 15	12:00	0·3 to 0·4	2415855·753	13·0
1902 Apr 15	13:20	0·3 not same	2415855·808	13·1
1902 Apr 15	15:40	0·2 at most	2415855·905	13·2
1902 Apr 21	13:39	0·75	2415861·821	12·6
1902 Apr 21	15:30	0·75	2415861·899	12·6
1902 Apr 27	11:50	0·50	2415867·746	12·9
1902 Apr 29	10:35	0·10	2415869·694	13·3
1902 May 6	14:11	0·50	2415876·844	12·9

TABLE III

Additional unpublished observations of Variable 2 from Barnard's notebooks

Date	*Time*	*ΔMag.*	*HJD*	*V*	*Notes*
1902 May 11	12:10	0·4	2415881·760	13·0	
1902 May 18	10:40	0·3	2415888·698	13·1	
1902 May 19	10:27	0·2	2415889·688	13·2	
1902 May 19	1:07	vy fnt	2415889·800	13·5	17:00 sid.
1902 May 20		0·0	2415890·675	13·4	14:05 sid.
1902 May 25	9:46	Faint	2415895·660	13·4	14:00 sid.
1902 May 26	9:42	0·1	2415896·657	13·3	
1902 May 26	1:30	0·5	2415896·816	12·9	
1902 May 27	9:26	0·6	2415897·646	12·8	
1902 June 1	12:20	0·8	2415902·767	12·6	
1902 June 3	9:45	0·2	2415904·659	13·2	
1902 June 9	9:45	0·1	2415910·659	13·3	
1902 June 17	11:14	1·25	2415918·721	12·1	
1902 June 30	10:35	0·0	2415931·693	13·4	
1902 July 1	11:15	0·3	2415932·721	13·1	
1902 July 9	2:30	0·1	2415940·856	13·3	
1902 July 17	2:00	1·1	2415948·835	12·3	
1902 July 21	10:36	0·2	2415952·693	13·2	
1902 July 28		0·8	2415959·73	12·6	19:50 sid.
1902 July 29	11:12	0·75? or 0·3	2415960·719	13·1	19:50 sid.
1902 Aug 4		0·3	2415966·697	13·1	19:40 sid.
1902 Aug 5	10:20	0·1	2415967·681	13·3	
1902 Aug 18	9:40	0·75 – 1·0	2415980·654	12·5	19:35 sid.
1902 Aug 21	8:50	0·7	2415983·618	12·7	
1902 Aug 25	9:26	0·0	2415987·643	13·4	19:47 sid.
1902 Aug 26	9:00	0·1 – 0·2	2415988·625	13·2	
1902 Sept 1	8:37	1·25?	2415994·608	12·1	
1902 Sept 2	8:30	0·5	2415995·604	12·9	
1902 Sept 9	8:15	0·1 – 0·2	2416002·593	13·2	
1902 Sept 11	9:37	1·0	2416004·650	12·4	
1902 Sept 15	8:20	0·0	2416008·596	13·4	
1902 Sept 16	7:45	0·8	2416009·572	12·6	
1902 Sept 18	7:50	0·7	2416011·575	12·7	
1902 Sept 30	7:00	0·0	2416023·540	13·4	Sept 29 ?
1902 Oct 14	7:00	0·4	2416037·539	13·0	
1902 Oct 27	5:40	0·8	2416050·483	12·6	

TABLE III (*concluded*)

Date	*Time*	*ΔMag.*	*HJD*	*V*	*Notes*
1903 Jan 19	6:20	0·0 – 0·1	2416135·012	13·3	
1903 Feb 9	6:05	0·1 – 0·2	2416156·003	13·2	
1903 Feb 16	3:04	1·0	2416162·875	12·4	12:44 sid.
1903 May 19	12:40	0·75	2416254·781	12·6	
1903 June 30	9:55	1·0	2416296·666	12·4	
1903 July 14	9:40	0·5	2416310·655	12·9	
1903 July 27	9:22	0·3	2416323·641	13·1	
1903 Aug 11		0·8	2416338·64	12·6	18:45? sid.
1903 Aug 18	8:20	0·2	2416345·597	13·2	
1903 Aug 31	7:15	1·0	2416358·552	12·4	
1903 Sept 1	7:50	0·5 – 0·6	2416359·576	12·8	
1903 Sept 7	7:34	0·1	2416365·564	13·3	
1903 Sept 8	7:15	0·4	2416366·551	13·0	
1903 Sept 21	6:50	0·5	2416379·533	12·9	
1903 Oct 19	6:10	0·5 – 0·6	2416407·504	12·8	
1903 Oct 20	6:05	0·8 – 0·9	2416408·501	12·5	20:05 sid.
1903 Oct 27	5:50	0·8	2416415·490	12·6	
1903 Nov 2	5:40	0·4 – 0·5	2416421·483	12·9	
1903 Nov 10		1·0	2416429·482	12·4	21:00 sid.
1903 Nov 24	5:35	0·4	2416443·480	13·0	

Note: Recorded sidereal time was considered in deriving HJD.

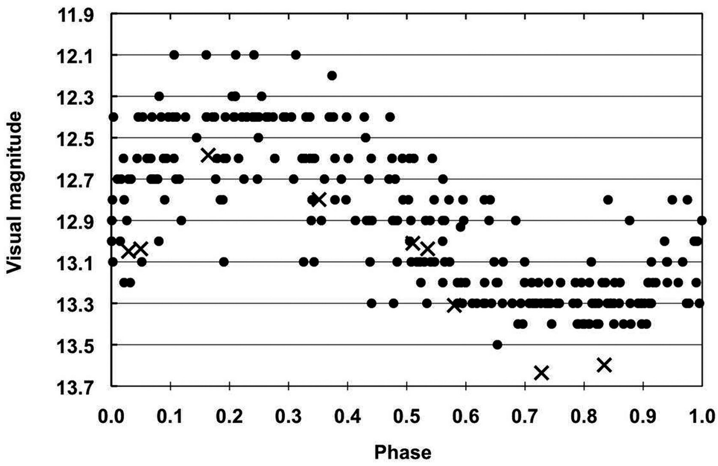

FIG. 1

Phased light-curve of Variable 2. Approximate *V* magnitudes from Barnard's visual estimates are plotted as dots and those from estimates on contemporaneous photographic plates are shown as crosses.

V1 observations

Starting in 1904 Barnard began making brightness estimates of Variable 1 as well as of V2. He probably began observing V1 once he learned its identification from Bailey[17]. Barnard generally estimated the magnitude difference of V1 relative to Scheiner 645 = L841, but occasionally used the "small star n. p."

TABLE IV

Barnard's later observations of V2 and his observations of V1

Date	*HJD*	*V2*	*V1–841*	*V1–np*	*Notes*
1904 Jan 3	2416484·024	13·2			
1904 Feb 15	2416526·87	12·4			1
1904 Feb 16	2416527·882	12·8			
1904 Feb 22	2416533·89	13·3			
1904 Feb 29	2416540·808	13·2			1
1904 Apr 18	2416589·708	13·2			
1904 Apr 19	2416590·673	13·4			
1904 Apr 26	2416597·807	12·8			1
1904 May 2	2416603·624	12·6	14·4		
1904 May 2	2416603·732	12·6	14·4		
1904 May 3	2416604·624		13·7		1
1904 May 3	2416604·859		14·2		1
1904 May 16	2416617·7	12·7	13·6		
1904 May 17	2416618·721	12·6			1
1904 May 23	2416624·645	12·9	14·0		
1904 May 30	2416631·819	13·3	14·4		
1904 June 4	2416636·774	13·4			
1904 June 7	2416639·780	12·8	14·0		1
1904 June 14	2416646·669	13·4	13·9		
1904 June 20	2416652·643	13·1	13·9		1
1904 June 25	2416657·732	13·2	14·8		1
1904 June 27	2416659·77	12·7	14·7		1
1904 July 4	2416666·636	13·2	14·7		1
1904 July 4	2416666·78		14·7		2
1904 July 9	2416671·683	12·9	13·8		
1904 July 9	2416671·752		14·2		
1904 July 11	2416673·674	12·7	14·7		1
1904 July 16	2416678·713	13·0	13·9		
1904 July 18	2416680·672	12·4	14·2		
1904 July 23	2416685·682	12·8		14·4	
1904 July 25	2416687·622	13·3	13·5		1
1904 Aug 1	2416694·793	12·4	14·0		
1904 Aug 6	2416699·643	12·4	14·5	14·0	1
1904 Aug 8	2416701·598	12·6	14·5		
1904 Aug 13	2416706·625	12·9	13·7		
1904 Aug 15	2416708·584	13·3	14·7		
1904 Aug 22	2416715·584	12·4	14·0		1
1904 Aug 29	2416722·565	12·8	13·9		1
1904 Sept 3	2416727·560	13·1		14·3	1
1904 Sept 5	2416729·576	13·1	14·7	14·6	
1904 Sept 12	2416736·540	12·4	15·2	14·4	1
1904 Sept 16	2416740·5	12·4		14·3	
1904 Sept 26	2416750·540	12·4	13·7		
1904 Oct 1	2416755·522	12·6	14·9	14·4	
1904 Oct 3	2416757·553	12·9	13·7		
1904 Oct 11	2416765·530	12·8	14·9	14·1	1
1904 Oct 17	2416771·508	12·4	15·2	14·4	
1904 Dec 12	2416827·997	12·7	13·9		
1906 Jan 27	2417238·992	12·8		13·5	
1906 Feb 10	2417252·957	12·2	14·7		1
1906 Mar 17	2417287·9	12·1	14·5	14·3	
1906 Mar 24	2417294·929	13·2	14·7		
1906 Apr 17	2417318·753	12·7	15·2	14·5	
1906 Apr 21	2417322·909	12·6	14·9	14·4	
1906 May 1	2417333·795	13·1	14·4	14·2	
1906 May 5	2417336·888	13·2	13·7		
1906 May 8	2417339·649	12·4	14·1		
1906 May 27	2417358·774	12·7	13·6		

TABLE IV (*concluded*)

Date	*HJD*	*V2*	*V1–841*	*V1–np*	*Notes*
1906 June 16	2417378·75	13·0	14·7		
1906 June 26	2417388·739	13·1	14·9		
1906 June 28	2417390·717	12·6	14·0		
1906 Jul 17	2417409·72	12·4	14·2		
1906 Jul 21	2417413·668	13·3	15·2	14·4	
1906 Jul 24	2417416·783	12·4	15·4	14·4	
1906 Jul 29	2417421·64	12·6	13·9		2
1906 Aug 14	2417437·616	12·6	13·7		
1906 Aug 28	2417451·604	12·1	15·4	14·4	
1906 Sept 18	2417472·56	12·6	13·7		1
1906 Dec 22	2417568·00	12·7			
1907 Feb 5	2417612·94	13·2	14·1		
1907 Mar 5	2417640·96	12·4	14·2		1
1907 Mar 10	2417645·939	12·4	14·2		1
1907 Mar 12	2417647·970	13·1	14·3		
1907 Mar 15	2417650·939	12·4	14·3		
1907 Mar 17	2417652·911	13·3	15·4		
1907 Mar 19	2417654·908	12·6	13·5		
1907 Apr 14	2417680·808		14·9		
1907 Apr 23	2417689·815	13·3	13·9		
1907 May 1	2417697·940	13·3	15·2	14·5	
1907 May 19	2417715·826	13·1	15·2		
1907 Jul 2	2417759·610	12·9	14·9	14·5	
1907 Jul 7	2417764·627		14·2		
1907 Jul 30	2417787·702	12·4	13·7		
1908 Mar 3	2418004·862	12·8	13·9		
1908 Mar 8	2418009·86	12·4	14·2		2
1908 Mar 10	2418011·88	13·4	14·7		2
1908 Mar 22	2418023·882	12·6	14·0		2
1908 Mar 29	2418030·776	13·1	14·7		2
1908 Apr 3	2418035·780	12·6	13·4		
1908 Apr 19	2418051·725		13·5		
1908 Apr 21	2418053·860	12·3	14·2		
1908 May 19	2418081·644	13·1	14·2		
1908 Jul 10	2418133·627	13·4	13·7		
1908 Aug 4	2418158·7	13·3	14·7		
1908 Aug 9	2418163·639	12·9	14·7		
1908 Aug 22	2418176·6	12·4	14·9	14·4	
1908 Aug 30	2418184·6	13·3	13·7		
1908 Sept 1	2418186·6	12·4	14·2		
1908 Sept 4	2418189·555	12·8	14·7	14·4	
1908 Sept 6	2418191·55	12·8	14·7		
1909 Mar 28	2418394·8	13·4	14·2		
1909 Jun 15	2418473·704	12·4			
1909 Jun 20	2418478·71	12·4	14·7		2
1909 Jun 22	2418480·67	13·2	13·8		2
1911 Jan 8	2419046·005	12·4			
1911 Jul 21	2419239·7	12·1	14·0		
1911 Jul 25	2419243·640	12·9	14·1		

Notes

1. Barnard gave both local time and sidereal time. Both used in deriving HJD.
2. Barnard gave only sidereal time.

[north preceding]. We adopted $V = 13{\cdot}69$ for L841[18] and $V = 14{\cdot}5$[19] for the 'n. p.' star to derive approximate V magnitudes from Barnard's differential estimates. The results are given in Table IV, where V1–841 and V1–np indicate the values based on L841 and the n. p. star, respectively.

The light-curve for Variable 1 is shown in Fig. 2. The V1–841 data are plotted as filled circles and the V1–np ones as open circles. Again, the phases have been computed using the period given by Kopacki *et al.*[16] and some observations from contemporaneous photographic plates are shown as crosses. The derived magnitudes for those dates when Barnard made estimates using both comparison stars suggest his largest magnitude differences relative to L841, *i.e.*, when V1 was near its faintest, were over-estimated by up to $0^{m}{\cdot}6$. In contrast, as seen in Fig. 1, both Barnard's V1–np data and the photographic observations are fairly consistent with the star's known range in V from 13·5 to 14·55[20].

Epochs of Barnard's observations

Barnard's publications and his compilation list state his times are Central Time, six hours behind UT. However, being certain of the epochs of observation is essential for period-change studies. A careful examination of Barnard's listed times, which sometimes also include sidereal time, demonstrates that he initially used astronomical time, which reckons the hours from noon rather than midnight and was commonly used in astronomy before 1925. That this is so can be seen by looking at his observations around the start and end of the 1900, 1901, and 1902 observing seasons. M13 is in conjunction with the Sun around December 3 of each year. Those observations taken in late December — that is, in the early morning when M13 rose just before the Sun — have listed times around 18:00 while those made in November — in the evening just before conjunction occurred — have times around 6:00. Correcting for

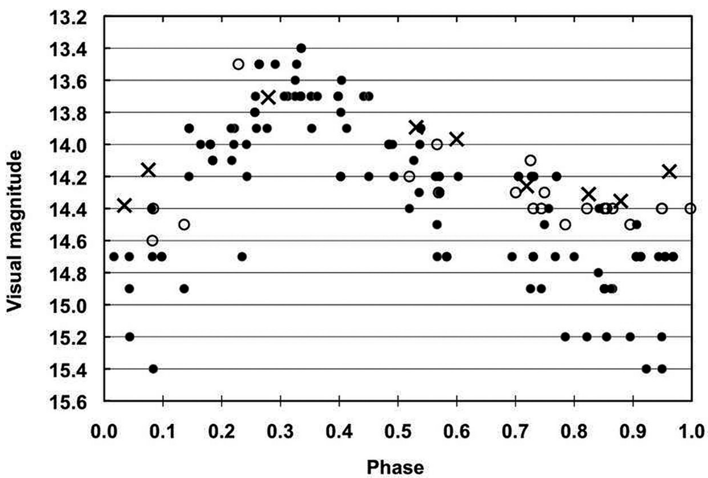

FIG. 2

Phased light-curve of Variable 1. Approximate V magnitudes from Barnard's visual estimates relative to L841 are plotted as dots and those relative to the n.p. star as circles. A few magnitude estimates from contemporaneous photographic plates are shown as crosses.

astronomical time and adding the six-hour time-zone difference indicates UT should be eighteen hours later than the listed time. This usually places the date of observation one day later than the one given by Barnard. Beginning with the 1904 season, Barnard switched to using a twelve-hour clock, so times after midnight require just a six-hour correction to UT.

We have looked for other evidence that we indeed have the correct date of observation. A few of the observations in Barnard's list have Julian Dates entered beside them. Surprisingly, each entry is one day later than our derived JD for that observation. Is it possible that Barnard was using some 'astronomical date' convention that is one day later than the calendar date? There are several pieces of evidence against that. First, the entries for several nights in Barnard's observing books give both the date and the day of the week, and these agree with the traditional calendar. Second, increasing the Julian Dates by one would significantly increase the phase shift for Barnard's observations compared to the later ones, leading to unreasonably large period changes. Finally, G. W. Ritchey obtained photographic plates of M 13 with the then-new *Yerkes* 40-inch refractor during the time when Barnard was using the same telescope for his visual observations[21,22]. Approximate *V* magnitudes of V1 and V2 have been estimated on those plates, whose dates are known and were taken with a yellow filter. These observations are given in Table V and also plotted in Fig. 1 and Fig. 2. The photographic data agree reasonably well with the visual-data light-curves based on the Julian Dates we derived. We conclude that our Julian Dates are correct.

While it is impossible to say for sure why Julian Dates given by Barnard are in error, a likely cause is a simple arithmetic error. For example, one of Barnard's notes regarding V2 contains the entry "Epoch 2,415,523·50 = May 17·50 1901." Examining *The American Ephemeris and Nautical Almanac for the Year 1901*[23] one finds that the Julian Date for 1901 January 1 is given as 2,415,386 (page vii) while May 17 is listed as day 137 of the year (page 76). Adding the two values gives Barnard's 2,415,523 rather than the correct 2,415,522, which is *136 days* — not 137 — after January 1.

Concluding remarks

The previously unpublished observations of V1 and V2 in M 13 described here, made by E. E. Barnard in the early years of the 20th Century, will be valuable

TABLE V

Approximate V magnitudes from Yerkes 40-inch refractor plates

Plate	*Listed date*	*Listed time*	*HJD*	*V2*	*V1*
FRy-2	1900 Aug 5	8:30 – 10:10 CST	2415237·640	13·04	13·89
FRy-2a	1900 Aug 9		2415241·65	13·60	13·71
FRy-3	1900 Aug 10	8:30 – 10:30 CST	2415242·646	13·05	14·17
F-8·5	1900 Aug 27	18:57 – 20:05 sid.	2415259·626	12·80	13·97
FRy-28	1901 Apr 18	12:40 – 15:40 sid.	2415493·765	12·58	14·16
FRy-29	1901 Apr 25	12:50 – 16:50 sid.	2415500·774	13·04	14·35
F-52	1908 Jul 5	16:50 – 19:36 sid.	2418128·719	13·64	14·38
F-104	1909 Jun 27	16:34 – 18:54 sid.	2418485·722	13·31	14·26
F-136	1911 Mar 12	12:35 – 16:35 sid.	2419108·883	13·01	14·31

Note: time for FRy-2a is estimated as only the date was recorded for this plate.

in improving knowledge of period changes of short-period Population II Cepheids. Stellar-evolution models predict observable changes in the pulsational periods of such stars[24,25], and those in globular clusters are ideal for testing the predictions given that the cluster ages and metallicities are known. The three Cepheids in M 13 — V1, V2, and V6 — comprise 12% of these stars known in Galactic globulars, and a comprehensive study of them by the first author and colleagues is under way. The results, including those from a period-change analysis which will incorporate all the early Barnard observations as well as new CCD photometry from 2001–2014, will be published in a future paper. Nevertheless, a few comments here seem in order.

The change in period of a variable star is determined from the well-known O–C diagram. In cases with a linearly changing period, the rate of change β is given by a parabolic fit of the O–C values as a function of epoch[26,27]. Osborn[5] was the first to seek period changes in the M 13 Cepheids. Converting to units more commonly used now, he found $\beta = 20$ d My^{-1} for V2 but suggested the possibility of a sudden period change rather than a linearly changing one. Expanding upon Osborn's data, Wehlau & Bohlender[6] derived β values for V1 and V2 of $0{\cdot}05 \pm 0{\cdot}19$ and $18{\cdot}0 \pm 2{\cdot}0$ d My^{-1} as part of their study of 12 globular-cluster Cepheids. Inspection of their published O–C diagrams, however, reveals that their M 13 results were essentially determined by just two early data sets: Barnard's[8] 36 published visual observations of V2 in 1900 and a very small set of just seven photographic observations* of V1 and V2 by Shapley[28] made in 1914–1915. An O–C value determined from the phase shift of the light-curve based on only seven points is obviously uncertain.

The present observations for V1 will provide at least five O–C values in the period 1904–1909, extending the epoch coverage by 12% from the previous earliest data set — the poor one of 1914–1915; the next-earliest observations are from 1925. The additional Barnard observations combined with the recent CCD ones will provide a sufficient time base to confirm or refute the parabolic behaviour of the V1 O–C values tentatively found by Smith *et al.*[9] and, we hope, will permit a statistically significant period-change rate to be determined. For V2, the newly discovered Barnard observations will add at least ten additional pre-1925 O–C data points in the period 1899–1911 to the one 1900 and the poor 1914–1915 values previously known. These together with the new CCD observations will permit a much more robust determination of β. Not only is the rate of change for V2 of interest because it is one the largest known for Population II Cepheids, a better O–C diagram may help answer the question whether the periods of these stars change at a constant rate or in abrupt steps[25].

Acknowledgments

R. Huttas and the staff of the University of Chicago's Special Collections Research Center are thanked for their assistance in locating and working with Barnard's papers. This work made use of the NASA Astrophysics Data System.

References

(1) P. J. Wyatt, *Physics Today*, **65**, no. 4, 9, 2012. See also *Physics Today*, **65**, no. 8, 8, 2012.
(2) A. A. Abdo *et al.*, *ApJ*, **727**, 129, 2012.
(3) H. A. Smith *et al.*, *PASP*, **90**, 422, 1978.
(4) G. Wallerstein, *PASP*, **114**, 689, 2002.
(5) W. Osborn, *AJ*, **74**, 108, 1969.
(6) A. Wehlau & D. Bohlender, *AJ*, **87**, 780, 1982.*

*Wehlau & Bohlender erroneously list 21 plates for this data set.

(7) E. E. Barnard, *Popular Astronomy*, **8**, 486, 1900.
(8) E. E. Barnard, *ApJ*, **12**, 182, 1900.
(9) H. A. Smith, *JAAVSO*, **43**, 255, 2015.
(10) J. Scheiner, *Abh. K. Akad. d. W. Berlin*, 1892.
(11) H. Ludendorff, *Pub. Astrophys. Obser. Potsdam*, **50**, 1, 1905.
(12) E. E. Barnard, *ApJ*, **29**, 72, 1909.
(13) A. Brown, *ApJ*, **122**, 146, 1955.
(14) K. M. Cudworth & D. G. Monet, *AJ*, **84**, 774, 1979.
(15) D. E. Welty, private communication of measures of this star made as part of the work reported in *AJ*, **90**, 2555, 1985.
(16) G. Kopacki, Z. Kołaczkowski & A. Pigulski, *A&A*, **398**, 541, 2003.
(17) S. I. Bailey, *Ann. Astr. Obs. Harvard Col.*, **38**, 1902.
(18) P. B. Stetson, *PASP*, **112**, 925, 2000. Data available at http://www.cadc-ccda.hia-iha.nrc-cnrc.gc.ca/en/community/STETSON/standards.
(19) Brightness estimated by author W. O. on several photographic *V* plates.
(20) C. D. Pike & C. J. Meston, *MN*, **180**, 613, 1977.
(21) G. W. Ritchey, *ApJ*, **12**, 352, 1900.
(22) G. W. Ritchey, *Pub. Yerkes Obs.*, **2**, 389, 1904.
(23) *The American Ephemeris and Nautical Almanac for the Year 1901* ([U.S.Navy] Department of Equipment, Washington), 1899.
(24) R. Gingold, *ApJ*, **204**, 116, 1976.
(25) M. Catelan & H. A. Smith, *Pulsating Stars* (Wiley-VCH), Chapter 7, 2015 and references therein.
(26) C. Sterken, in C. Sterken (ed.), *Light time Effects in Astrophysics, ASP Conference Series*, **335**, 3, 2005.
(27) J. F. Le Borgne *et al.*, *A&A*, **476**, 307, 2007.
(28) H. Shapley, *Mt. Wilson Contrib.*, no. 116, 1915.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 249: HD 32662, HD 76462, HD 78141, AND HD 111285

By R. F. Griffin
Cambridge Observatories

Habitual readers of this *Magazine* will not be surprised to find a paper such as this one in this issue, giving orbits for a number of stars that have come to attention in different ways. HD 32662, a $7^{m}\cdot 7$ F8 star, was proposed by Perryman *et al.*, on the basis of its parallax and proper motion, to be an outlying member of the Hyades star cluster, but its radial velocity is shown below to disqualify it. Its orbit has a period of very nearly 1000 days and the high eccentricity of 0·77. HD 76462 is an 8^{m} early-F star whose projected rotational velocity of nearly 20 km s^{-1} has necessitated unusually generous integration times in the effort to obtain reasonably accurate radial velocities. It has an orbit with an eccentricity just over 0·5 and a period of five years that is determined to little more than two days. HD 78141, a star that was brought into the 'Clube Selected Areas' programme when the northern Areas were deemed to be increased in size in order to provide numbers of stars more closely matching those of the southern Areas, proves to have an orbit with an eccentricity of 0·29 and a period of 160 days, determined to within an hour. HD 111285 was first observed more than 40 years ago, but its binary nature was discovered only comparatively recently. Only a small part of its 50–70-year orbit has been properly observed, but that part includes both nodes, so the orbit is tolerably well determined apart from the considerable uncertainty in its period. None of the stars treated here has a mass function large enough to raise expectations that any of the companion stars ought to be conspicuous in the radial-velocity traces.

HD 32662

HD 32662*, an object somewhat brighter than the eighth magnitude and of approximately solar type, is to be found in the north-preceding part of Orion, about 5° preceding Bellatrix (γ Ori; for northern observers the top right-hand bright star in the familiar constellation figure).

The star is one of 39 that were proposed by Perryman *et al.*[1] (a consortium led by the presiding genius† behind the *Hipparcos* astrometric satellite and the resulting catalogue[2]) to be possible (but previously unrecognized) members of the Hyades star cluster. Those objects are mostly outliers as far as their positions in the sky are concerned, so they had not featured in previous investigations directed towards the identification of cluster members. Perryman *et al.*'s assessments were naturally based largely upon the distances and proper motions of the candidates, whose radial velocities were in most cases (21 out of the 39) unknown. In the 18 instances in which the radial velocities were more or less known, they tended to support the idea of Hyades membership: that was of course only to be expected, because otherwise the objects concerned would not have featured in the Perryman paper at all. There was no way for the reader of the paper to tell whether ever so many other objects may have fulfilled the astrometric criteria for Hyades membership but been rejected on the basis of their already-known radial velocities. It seemed like more than just a 'straw in the wind', however, when the present writer[3] was able to measure 15 of the 21 Perryman objects whose radial velocities were previously unknown‡, and found that they definitely disqualified all but one of them.

In the absence of ground-based *UBV* measurements of HD 32662, we are fortunate in having photometry from *Tycho 2*[4], which provided the values $V = 7^{m}\cdot69$, $(B - V) = 0^{m}\cdot51$; there does not seem to be any recent spectral

*There is a curious similarity between the star's *Henry Draper Catalogue* and *Hipparcos* designations, which are respectively 32662 and 23662.

†Subsequently Project Scientist for *Gaia*, too.

‡The other six were either too faint (4) or of inappropriate spectral types (2) to be measured with the Cambridge radial-velocity instrument.

classification, but the *Henry Draper Catalogue* type of G0 is not far from the F8 that might be implied for a main-sequence star by the colour index. The (revised[5]) *Hipparcos* parallax of 17·54 ± 1·03 milliseconds of arc corresponds to a radial distance of about 57 ± 4 pc — which can at least be claimed to be no further from the Hyades centre at[6] 45·4 ± 2·1 pc than the tangential offset that is already implicit in the star's angular distance of about 14° from the apparent centre of the cluster near θ^1 and θ^2 Tauri. The distance modulus is 3·78 ± 0·13 magnitudes, leading to an absolute *V* magnitude of $3^{m}\cdot 9$, in full agreement with the F8 V type suggested by the colour index.

It was the proposal[1] that HD 32662 might be a member of the Hyades that led to its being entered into the Cambridge radial-velocity observing programme in 2006. In fact the *Hipparcos* catalogue not only indicated that, as far as position and proper motion are concerned, the star might be a member of the Hyades, but *Hipparcos* also detected orbital motion. The satellite data did not suffice for a complete determination of the orbital elements, and the *Hipparcos* authors felt obliged to adopt a circular orbit (which has proved to be a poor approximation to the actual fact) as the basis for their solution. Because the orbital period is of the same order as the operational lifetime of the satellite, it is in any case not to be expected that the astrometric orbit would be very reliable. Its period is listed in the *Hipparcos Catalogue*[2] (vol. **10**, p. DO1), with ludicrously unwarranted precision, as 667·7063 ± 42·0115 days; the orbital inclination is given as 82·69 ± 9·87 degrees, whose approximation to 90° implies that the $\sin^3 i$ factor in the expression for the mass function is not likely to be far from unity.

HD 32662 was evidently recognized as a spectroscopic binary by Nordström *et al.*[7], whose measurements are not available individually but who listed its mean radial velocity as +35·6 km s^{-1}, with the extraordinary standard error of 18·2 km s^{-1}. That seems to show, in the light of what is known now, that they had made just two measurements of the velocity and that they had disagreed by $\sqrt{2}$ times the latter quantity (almost the maximum possible, as it turns out)*. In view of its large uncertainty, the mean velocity could be regarded as being possibly consonant with Hyades membership of the star. Nordström *et al.* also noted that HD 32662 exhibits a projected rotational velocity of 12 km s^{-1}; if the axial inclination were supposed equal to the orbital one, not far from 90°, the rotational period would be about five days, though there is no implication that the orbital period ought to be the same.

The initial Cambridge radial-velocity observations confirmed the 12 km s^{-1} rotational velocity and showed a definite, though only gradual, variation of velocity, amounting to 3 km s^{-1} over an interval of five months in the first season. A surprise awaited the observer, however, when a change of 12 km s^{-1} occurred between observations made only 20 days apart in the following season. The implied high eccentricity has proved to be about 0·77. Table I presents the 56 observations, all made at Cambridge, that are now available; the orbital period is very close to 1000 days (998·51 ± 0·11 days), very nearly 50% longer than the *Hipparcos* value and differing from it by nearly eight times its asserted standard error of 42·0115 days, which is evidently misleading. The γ-velocity of +30 km s^{-1} is far removed (well over 200 standard deviations) from the value of about +45 km s^{-1} appropriate to a Hyades member at HD 32662's position in the sky. A preliminary statement of the writer's conclusions concerning the star was published in 2013 (in a paper[3] not retrieved by *Simbad*); the present

*The whole range of velocity for HD 32662 is about 27 km s^{-1}, so if there were more than two measurements the standard error of the mean could not be so large.

TABLE I

Radial-velocity observations of HD 32662

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2006 Sept. 21·18	53999·18	+28·1	0·554	+0·5
Oct. 3·22	54011·22	27·2	·566	−0·3
Nov. 1·15	040·15	27·4	·595	+0·4
26·11	065·11	26·4	·620	−0·3
Dec. 9·09	078·09	25·9	·633	−0·6
2007 Jan. 6·02	54106·02	25·6	0·661	−0·5
22·87	122·87	26·3	·678	+0·5
Feb. 26·87	157·87	25·1	·713	−0·2
Oct. 19·16	392·16	21·8	·948	+0·1
Nov. 16·09	420·09	25·3	·976	+0·3
Dec. 6·02	440·02	37·5	·996	−0·3
8·10	442·10	40·1	·998	+0·2
11·05	445·05	42·8	1·001	+0·2
13·01	447·01	44·4	·003	+0·2
17·00	451·00	46·3	·007	−0·4
2008 Jan. 6·02	54471·02	48·5	1·027	+0·7
24·94	489·94	44·9	·046	0·0
Feb. 19·89	515·89	41·8	·072	+0·1
Mar. 7·88	532·88	39·8	·089	−0·3
30·81	555·81	39·0	·112	+0·5
Sept. 20·20	729·20	32·2	·285	−0·1
Oct. 17·22	756·22	31·8	·312	+0·1
Nov. 8·11	778·11	30·8	·334	−0·5
26·08	796·08	31·2	·352	+0·3
Dec. 27·02	827·02	30·8	·383	+0·4
2009 Feb. 10·94	54872·94	29·0	1·429	−0·6
Mar. 20·84	910·84	28·8	·467	−0·2
Dec. 21·02	55186·02	25·5	·743	+0·7
2010 Jan. 17·97	55213·97	24·5	1·771	+0·2
Feb. 17·85	244·85	23·8	·802	0·0
Sept. 15·20	454·20	47·7	2·012	−0·6
Oct. 7·22	476·22	46·7	·034	−0·1
Nov. 24·10	524·10	40·7	·082	0·0
2011 Jan. 18·97	55579·97	37·6	2·137	+0·5
Oct. 16·16	850·16	30·1	·408	+0·2
Nov. 28·09	893·09	29·5	·451	+0·3
2012 Jan. 2·07	55928·07	27·7	2·486	−1·0
14·89	940·89	28·4	·499	−0·1
26·95	952·95	28·9	·511	+0·6
Feb. 18·85	975·85	28·4	·534	+0·5
2013 Jan. 9·98	56301·98	22·6	2·861	0·0
Feb. 27·83	350·83	21·7	·909	0·0
Oct. 30·21	595·21	35·7	3·154	−0·6
Dec. 1·09	627·09	34·4	·186	−0·7
28·94	654·94	34·1	·214	0·0
2014 Feb. 2·90	56690·90	33·4	3·250	+0·3
25·83	713·83	32·3	·273	−0·3
Nov. 24·05	985·05	+27·9	·545	+0·1

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)* *km s⁻¹*
2015 Sept. 7·18	57272·18	+23·0	3·832	−0·2
2016 Jan. 15·93	57402·93	22·4	3·963	−0·2
25·95	412·95	23·9	·973	−0·4
Feb. 2·96	420·96	27·0	·981	0·0
11·90	429·90	32·7	·990	0·0
15·90	433·90	35·9	·994	−0·3
18·93	436·93	39·4	·997	+0·2
23·89	441·89	+43·9	4·002	+0·2

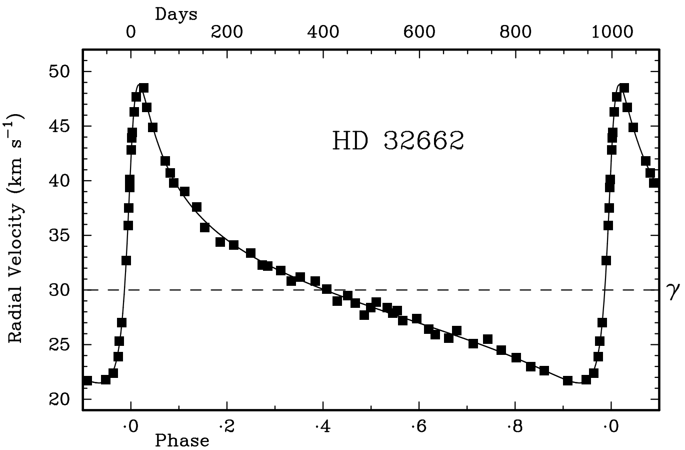

FIG. 1

The observed radial velocities of HD 32662 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*.

discussion is the "full account" that was promised then. The list of observed radial velocities appears here as Table I, while the orbital elements are listed in the first numerical column of Table VI on p. 193 below; the orbit is illustrated in Fig. 1. The mass function implies a minimum mass of about 0·6 $M_{\odot}$ for the companion object, which (unless it is itself a double star) must therefore be no later than about K7V and no more than about four magnitudes fainter than the primary; the high inclination indicated by the *Hipparcos* orbit indicates that the companion's actual mass and luminosity cannot be much greater than those minimum values.

HD 76462

HD 76462 is an $8^{\rm m}$ star in the middle of the constellation Cancer. It is very close to making an equilateral triangle, 3½° on a side, with the $4^{\rm m}$ stars γ and δ Cancri; those stars are nearly in a N–S line, and HD 76462 is on the following side of them. That area of the sky is enlivened by the presence of Praesepe, which is about 1° preceding the mid-point between the two bright stars and is accordingly about 4° preceding HD 76462.

In the absence of ground-based *UBV* photometry of HD 76462, we have to be grateful to *Tycho 2* once again for the *V* and $(B-V)$ values of $8^{\rm m}\cdot16$ and $0^{\rm m}\cdot42$, respectively. The colour index is unusually blue for a star that features in this series of papers, and is exactly the value listed in *Astrophysical Quantities*[8] for stars of type F5 V. F5 is in fact the type that is given for HD 76462 in the *HD*, and the (revised[5]) *Hipparcos* parallax of 7·62 ± 0·83 milliseconds of arc leads to a distance modulus of $5^{\rm m}\cdot59$, with an uncertainty of about $0^{\rm m}\cdot24$, and thus to an absolute magnitude of $+2^{\rm m}\cdot57$, with the same uncertainty. An apparent inconsistency arises from the fact that the absolute magnitude is that shown[8] for a main-sequence star of type F0, not F5 (for which types the listed[8] values are respectively $2^{\rm m}\cdot6$ and $3^{\rm m}\cdot4$), so HD 76462 appears to be the best part of a magnitude 'too bright' for its type. A probable answer to the problem is that the star is significantly evolved: the evolutionary tracks of F-type stars start more or less vertically upwards in the H–R Diagram — towards higher luminosity at nearly constant colour index. Disappointingly little else can be learnt about the star from the literature, apart from a mean radial velocity near −7 km s^{-1} with an uncertainty of 2·8 km s^{-1} (in some reports listed as 6·3 km s^{-1}, $\sqrt{5}$ times as great — evidently the r.m.s. scatter of five individual measurements that seem not to be accessible separately).

HD 76462 was placed on the Cambridge radial-velocity programme in 1994, for reasons now beyond recall. At that time there was no operational instrument at Cambridge, and the programme was initially dependent on observations made at Haute-Provence with the prototype *Coravel*, through the courtesy of Dr. M. Mayor. It is not easy to obtain accurate velocities of the star: not only does its relatively early type mean that the 'dips' in radial-velocity traces are quite small, but they are also very considerably smeared out by the star's projected rotational velocity of almost 20 km s^{-1}. Nine measurements were made with the Haute-Provence *Coravel*, and after a largely analogous instrument was brought into use (at first intermittently) at Cambridge, another 87 observations were made from the home site. They are all set out in Table II. During the 20-odd years of the observing campaign, progressively increasing recognition of the need for generous integrations (and of patience on the part of the observer!) led to substantial increases in exposure times to improve the reliability of the velocities obtained from the weak and wide 'dips' given by HD 76462. Integration levels of 10 000 counts per 'bin' — at least ten times what is usual for most stars on the programme (other than bright ones, where such a level is reached very quickly) — became the norm. In the calculation of the orbit, the weighting attributed to the observations made in successive intervals of time has been deemed to increase stepwise, to keep the r.m.s. residuals of the successive groups of observations approximately constant. The early Haute-Provence measures have been weighted 0·1; then the early Cambridge ones have been given 0·2, those made in the 2005/6 and succeeding seasons 0·6, and those in the final two seasons (starting at the end of 2014) 1·0. The numbers of measurements in the four successive intervals are 9, 38, 39, and 10, respectively, and are separately distinguished in the orbit plot that is presented here as Fig. 2. The orbital elements are included in Table VI.

TABLE II

Radial-velocity observations of HD 76462

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

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O–C) km s⁻¹
1994	May	3·92*	49475·92	+5·8	0·952	+1·4
	Dec.	12·17*	698·17	−3·9	1·066	0·0
1995	June	2·05*	49870·05	−9·5	1·155	−0·1
	Dec.	27·09*	50078·09	−12·6	·262	−1·6
1996	Mar.	31·97*	50173·97	−14·4	1·311	−3·3
	Nov.	24·13	411·13	−12·0	·433	−1·1
	Dec.	2·13	419·13	−11·4	·437	−0·6
		25·20*	442·20	−9·9	·449	+0·9
1997	Jan.	22·97*	50470·97	−8·6	1·464	+2·1
		26·09*	474·09	−10·4	·465	+0·3
	Feb.	7·99	486·99	−10·8	·472	−0·2
		10·88	489·88	−10·0	·473	+0·6
		18·99	497·99	−10·4	·478	+0·2
	Mar.	3·00	510·00	−9·3	·484	+1·3
		29·91	536·91	−10·8	·498	−0·3
	Apr.	17·90	555·90	−10·2	·507	+0·2
	May	3·91	571·91	−11·2	·516	−0·9
	Dec.	22·13*	804·13	−9·3	·635	−0·3
1999	Dec.	27·19	51539·19	+5·9	2·013	+2·1
2000	Feb.	14·03	51588·03	+0·1	2·039	+0·2
	Apr.	21·89	655·89	−5·6	·073	−0·9
	Nov.	20·21	868·21	−8·9	·183	+1·2
	Dec.	3·19	881·19	−11·1	·189	−0·9
2001	Jan.	14·10	51923·10	−8·1	2·211	+2·5
	Feb.	14·02	954·02	−9·8	·227	+0·9
	May	4·91	52033·91	−11·1	·268	−0·1
	Nov.	24·21	237·21	−12·3	·373	−1·2
2002	Jan.	2·11	52276·11	−9·4	2·393	+1·6
	Mar.	27·95	360·95	−11·6	·436	−0·8
	May	4·91	398·91	−11·6	·456	−0·9
2003	Jan.	6·13	52645·13	−9·1	2·582	+0·6
	Feb.	20·02	690·02	−8·6	·606	+0·8
	Mar.	23·94	721·94	−8·4	·622	+0·8
	Nov.	4·22	947·22	−6·3	·738	+0·8
	Dec.	8·16	981·16	−8·1	·755	−1·4
2004	Jan.	15·14	53019·14	−6·3	2·775	−0·2
	Feb.	26·04	061·04	−4·9	·796	+0·6
	Apr.	14·95	109·95	−5·0	·822	−0·5
	May	16·88	141·88	−5·5	·838	−1·7
	Nov.	14·23	323·23	+2·3	·931	−0·2
	Dec.	18·22	357·22	+3·9	·949	−0·2
2005	Jan.	13·10	53383·10	+6·0	2·962	+0·8
	Mar.	13·00	442·00	+4·0	·992	−1·9
		24·99	453·99	+3·4	·999	−2·1
	Apr.	3·93	463·93	+6·0	3·004	+1·0
		18·89	478·89	+3·3	·011	−0·8
	May	7·88	497·88	+2·5	·021	−0·2
	Oct.	10·22	653·22	−6·5	·101	+0·5

TABLE II (*concluded*)

Date (UT)			*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2005	Nov.	4·23	53678·23	−7·4	3·114	+0·4
	Dec.	18·15	722·15	−8·9	·137	−0·1
2006	Jan.	29·10	53764·10	−10·6	3·158	−1·1
	Mar.	1·96	795·96	−8·9	·175	+1·0
	Apr.	5·95	830·95	−10·0	·193	+0·3
	Nov.	26·21	54065·21	−11·1	·313	0·0
2007	Jan.	14·16	54114·16	−12·2	3·338	−1·1
	Feb.	15·07	146·07	−11·6	·355	−0·5
	Mar.	26·97	185·97	−10·1	·375	+1·0
2008	Jan.	25·09	54490·09	−9·5	3·532	+0·7
	Mar.	10·00	535·00	−9·4	·555	+0·6
	Apr.	1·92	557·92	−10·0	·567	−0·2
	Oct.	28·25	767·25	−8·5	·674	−0·1
	Dec.	7·23	807·23	−8·9	·695	−0·9
2009	Jan.	6·16	54837·16	−7·8	3·710	−0·1
	Mar.	27·92	917·92	−6·9	·752	−0·1
	Apr.	19·85	940·85	−7·2	·764	−0·7
	Dec.	1·18	55166·18	−2·4	·880	−0·8
		21·17	186·17	−0·5	·890	+0·4
2010	Jan.	31·08	55227·08	+2·5	3·911	+1·8
	Feb.	21·01	248·01	+2·2	·922	+0·6
	Apr.	4·94	290·94	+3·1	·944	−0·6
		17·86	303·86	+4·1	·951	−0·2
	May	11·87	327·87	+5·1	·963	−0·2
		21·89	337·89	+5·2	·968	−0·4
	Nov.	28·23	528·23	−4·6	4·066	−0·7
	Dec.	19·19	549·19	−5·5	·077	−0·4
2011	May	9·90	55690·90	−9·3	4·150	0·0
	Dec.	5·23	900·23	−10·8	·257	+0·2
2012	Jan.	4·15	55930·15	−11·7	4·273	−0·7
	Feb.	2·03	959·03	−10·7	·288	+0·4
	Apr.	17·90	56034·90	−11·1	·327	+0·1
	Nov.	11·24	242·24	−11·0	·433	−0·1
2013	Feb.	7·01	56330·01	−10·4	4·479	+0·2
	Apr.	6·90	388·90	−10·1	·509	+0·3
	Dec.	20·18	646·18	−9·3	·641	−0·4
2014	Feb.	6·00	56694·00	−8·6	4·666	−0·1
	Mar.	3·95	719·95	−7·9	·679	+0·4
	Dec.	30·16	57021·16	−4·0	·834	0·0
2015	Jan.	31·05	57053·05	−3·6	4·851	−0·4
	Mar.	27·03	108·03	−0·8	·879	+0·8
	May	12·88	154·88	−0·1	·903	−0·2
		18·90	160·90	+0·3	·906	0·0
	Dec.	21·18	377·18	+3·1	5·017	−0·2
2016	Jan.	1·10	57388·10	+3·5	5·023	+1·1
	Feb.	16·06	434·06	−1·1	·047	+0·3
		19·04	437·04	−1·4	·048	+0·2
	Mar.	5·02	452·02	−3·8	·056	−1·1

*Observed with Haute-Provence *Coravel*

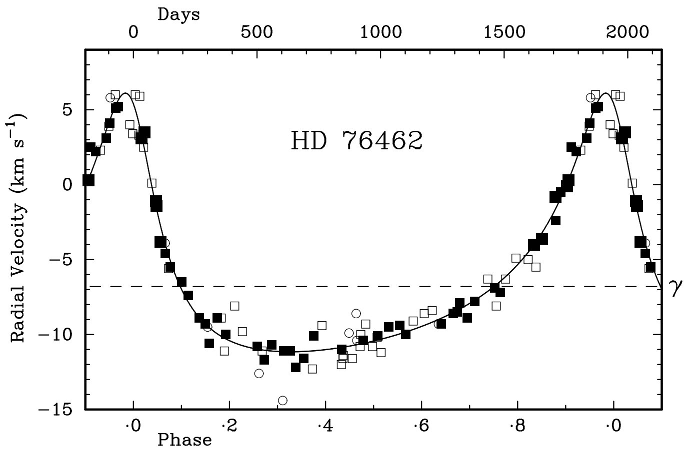

FIG. 2

The observed radial velocities of HD 76462 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Open circles plot the first nine observations, which were obtained with the Haute-Provence *Coravel* and have been weighted 0·1 in the solution of the orbit. The other 87 measurements were all made with the Cambridge *Coravel* and are plotted with square symbols, but have been divided into three groups representing successive intervals of time, as the observer learnt to give longer integrations and the velocities determined from the broad shallow 'dips' given by this star accordingly became more reliable. The first group (open symbols) has been given weight 0·2, the second (filled symbols) 0·6, and the final one (larger filled ones, phases restricted to the vicinity of the velocity maximum) 1·0.

The period of somewhat over five years is established to within not much more than two days, thanks in part to the fairly high orbital eccentricity of a little over 0·5. If the mass of the primary star is taken as 1·3 $M_\odot$, as befits a star that is (or started out at) F5 V, then the secondary has to be, as a minimum, nearly 0·7 $M_\odot$ to fulfil the mass function. The secondary, even if it is a single star, could therefore be as late as K5 V, getting on for five magnitudes fainter than the primary, so there is no difficulty in accepting its invisibility in the radial-velocity traces.

HD 78141

HD 78141 was added to the Cambridge observing programme as recently as the beginning of 2013, as a star in Area 5 of the 'Clube Selected Areas'. It was one of many stars that were added then to the northern Areas[9] of that programme, by deeming the Areas to be of increased size. That was done in order to make the number of stars per Area, and therefore the precision of the mean radial velocity of the stars in each Area (the ultimate objective of the work) more comparable with the corresponding quantities for the southern Areas[10], where the density of stars in the underlying *Henry Draper Catalogue* is much greater than in the north.

Like HD 76462 (the object treated in the section immediately above), HD 78141 is in Cancer; in fact the two stars are less than 4° apart. (HD 76462 is itself well within Area 5, but did not qualify for the 'Clube Selected Areas' programme because its *HD* type is not K0.) HD 78141 is about 1° north-preceding the 5^m K0 III star ξ Cancri*. Only about 7′ north of it is the 6^m obvious visual binary HR 3617 (ADS 7187), a pair of early-F stars with a Δm of about $0^m\cdot4$ and a separation of about 7″. There has been little change in their relative positions since the system was first properly measured by Struve[12] nearly 200 years ago; it was actually first noticed as a double star by Sir William Herschel as long ago as 1782 December 28 and listed[13] among his discoveries as III–92.

Such literature as there is on HD 78141 is mostly related to the star's comparative youth, which probably accounts for the activity which resulted in its detection by the *ROSAT* X-ray satellite and may have been what prompted a number of unsuccessful searches for planetary companions. *Tycho 2*[4] has provided the photometry $V = 7^m\cdot98$, $(B - V) = 0^m\cdot89$. *Tycho* has also been reported[14,15] as offering a distance measurement of 21·4 pc (even in one case[16] 21·40 pc), but more recently the impression has been given that *Tycho* distances have been implicitly discounted, since they are no longer listed (for example by *Simbad*). For a main-sequence star, the colour index agrees well with the *HD* type of K0, and if we accepted the formerly listed *Tycho* distance, equating to a modulus of about $1^m\cdot7$, we would arrive at an absolute magnitude of $6^m\cdot3$, not too far from the $5^m\cdot9$ listed in *AQ*[8] for type K0 V.

Whereas almost all the other stars that were newly included in Clube Area 5, after the northern Areas were arbitrarily enlarged to embrace more stars, were first measured in 2012, HD 78141 was not observed until 2013 February. It did, however, 'catch up' with the majority by being observed a second time only two months later; it then overtook them in terms of observational coverage because the large discrepancy between the first two measurements prompted observations to be made on nearly every clear night — 16 in April and early May — until the close of that initial observing season. There are now 48 observations, all obtained with the Cambridge *Coravel*; they show 'dips' that are slightly broadened, to an extent interpreted as a projected rotational velocity of 5·1 km s^{-1}, the standard error of the mean being little more than 0·2 km s^{-1}. The radial velocities that they give are set out in Table III. They readily yield the orbit that is illustrated in Fig. 3; its elements are included in Table VI below. The orbital period of 160 days is quite modest in comparison with most of those found by the writer, and is determined to better than an hour. The eccentricity is moderate, 0·29, and the mass function is 0·06 $M_\odot$. If the primary star is taken to have the mass of about 0·8 $M_\odot$ that is normal for a K0 V star, then the secondary must have a mass no less than about 0·46 $M_\odot$. That is acceptable, as such a star would be nearly four magnitudes (a factor of 40) fainter than the primary in the *B* region of the spectrum where the radial-velocity spectrometer operates, and so would not be expected to give an observable 'dip' in the velocity traces unless the orbital inclination were well away from 90°.

*ξ Cnc has itself been known as a spectroscopic binary for nearly 100 years; an orbit was given[11] for it, on the basis of only 17 spectrograms, in 1957.

TABLE III

Radial-velocity observations of HD 78141

All the observations were made with the Cambridge Coravel

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2013 Feb. 7·03	56330·03	−17·5	0·731	+0·1
Apr. 1·94	383·94	−3·8	1·067	−0·2
5·91	387·91	−7·2	·092	+0·1
6·90	388·90	−8·1	·098	+0·1
16·89	398·89	−15·9	·161	+0·3
17·98	399·98	−16·9	·167	0·0
18·87	400·87	−17·5	·173	0·0
19·85	401·85	−17·9	·179	+0·2
26·89	408·89	−21·7	·223	+0·1
29·87	411·87	−22·6	·242	+0·4
30·88	412·88	−23·3	·248	0·0
May 2·90	414·90	−23·8	·260	+0·2
3·87	415·87	−24·6	·267	−0·3
5·91	417·91	−24·8	·279	+0·1
6·88	418·88	−25·3	·285	−0·1
13·90	425·90	−26·7	·329	−0·1
16·89	428·89	−27·0	·348	0·0
Oct. 29·25	594·25	−27·5	2·378	0·0
Nov. 9·25	605·25	−27·7	·447	0·0
13·25	609·25	−27·6	·472	−0·1
19·26	615·26	−27·0	·509	0·0
Dec. 20·19	646·19	−19·6	·702	−0·1
28·12	654·12	−16·2	·752	−0·2
2014 Jan. 12·10	56669·10	−6·8	2·845	+0·2
Feb. 3·06	691·06	+4·4	·982	+0·1
6·03	694·03	+3·8	3·001	+0·1
11·10	699·10	+1·1	·032	+0·1
13·00	701·00	−0·7	·044	−0·3
26·08	714·08	−12·2	·126	−0·3
Mar. 1·97	717·97	−15·1	·150	−0·1
8·97	724·97	−19·8	·193	−0·4
11·94	727·94	−21·1	·212	−0·2
Apr. 14·95	761·95	−28·0	·424	−0·2
May 2·94	779·94	−26·7	·536	−0·2
Nov. 24·25	985·25	−10·3	4·816	−0·2
Dec. 6·29	997·29	−1·6	·891	+0·2
2015 Jan. 11·18	57033·18	−10·2	5·115	+0·3
Mar. 27·02	108·02	−25·0	·581	+0·2
31·96	112·96	−24·3	·612	−0·2
Apr. 5·95	117·95	−22·2	·643	+0·6
11·88	123·88	−21·0	·680	−0·1
29·90	141·90	−12·4	·792	0·0
May 12·89	154·89	−4·0	·873	−0·3
18·89	160·89	+0·6	·911	+0·2
Dec. 9·23	365·23	−18·6	7·185	0·0
2016 Feb. 24·00	57442·00	−21·6	7·663	+0·2
Apr. 5·87	483·87	+1·2	·924	−0·4
6·96	484·96	+2·4	·931	+0·2

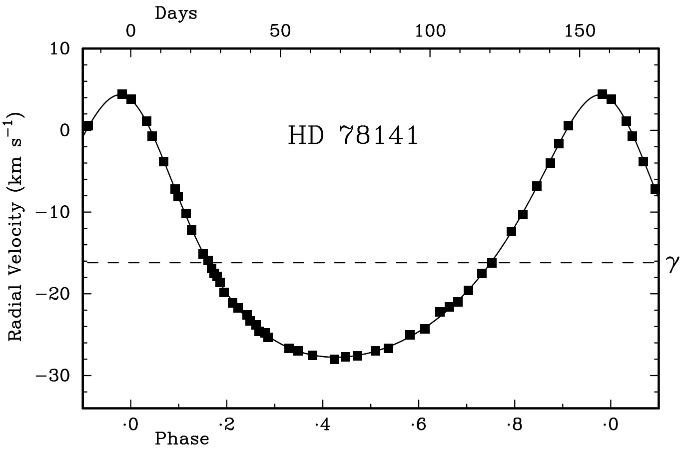

FIG. 3
Exactly as Fig. 1, but for HD 78141.

HD 111285

This star is one of nearly a thousand that were observed in the writer's survey, published together[17] with a photometric survey undertaken by (the now late) Kenneth Yoss, of the radial velocities of late-type stars in the vicinity of the North Galactic Pole (NGP). Its velocity was, however, measured only twice in the course of that programme. The first time (1974) it was not observed by the writer in person but by his then graduate student G. A. Radford, who was co-author with the present writer (in several cases appearing as the senior author) of Papers 3–17 of this series. He was using the original photoelectric radial-velocity spectrometer[18] at Cambridge. A second measurement was made by the writer, with the same instrument, in 1980, and was in tolerable agreement with the first one, so in the context of the large programme on the NGP no further measurements were made for a long time, and the discovery of the binary nature of HD 111285 was delayed more than 20 years.

HD 111285 is a 7^{m} star in Coma, quite near the Galactic Pole — its Galactic latitude is nearly 85°. It is almost equally near to the centre of the Coma star cluster (Melotte[19] 111), but its distance from the centre is about twice the visually apparent radius of the cluster, and the radial velocity of the star shows that it is not related to Mel 111. Photometry of HD 111285 has been given by Yoss & Griffin, in their comprehensive paper[17] on NGP photometry and radial velocities, as $V = 7^{m}{\cdot}21$, $(B - V) = 0^{m}{\cdot}94$. Reasonably similar photometry has been published by Bakos[20] and Strassmeier *et al.*[21].

The star was observed at the David Dunlap Observatory in the course of the work there[22] on the late-type stars in the +25° to +30° declination zone; the results included a 'photographic' magnitude of 8·93, a spectral type of G8 III, and a radial velocity, from four plates (not reported individually, but the mean is attributed a 'probable error' of 1·1 km s^{-1}), of −31·2 km s^{-1}. A type of G6 III had previously been given (though it is not retrieved by *Simbad*) by no less an expert than Keenan[23]; Yoss's *DDO* photometry[17] implied an 'mk' type of g8 III — the lower case is intended to distinguish a type derived photometrically from one obtained by classification of actual spectra. The *DDO* photometry also provided estimates of [Fe/H] and M_V of –0·46 and $+1^{m}\cdot1$, respectively. Bartkevičius & Lazauskaitė[24] included HD 111285 in their paper on the classification of Population II stars by means of the Vilnius photometric system[25]; the star appears as no. 365 in their table, with the spectral type given as "MD-G9III, Ba?", [Fe/H] = –0·19, and $M_V = +1^{m}\cdot56$. Their Table A3, which has "notes from the literature", notes "VAR?: ΔV 0·47"; the present writer is not aware of the source of that suggestion, which does not seem to have received any independent support.

The radial velocity of –31·2 km s^{-1} that was obtained for HD 111285 at the David Dunlap Observatory has already been mentioned above. A Mount Wilson determination of –31·8 km s^{-1} was published by Sandage & Fouts[27]; despite coming from the 100-inch telescope, it was obtained with a *reticon* system whose accuracy was disappointing, with an r.m.s. observational error amounting to 6·7 km s^{-1}. The two observations published in the writer's NGP survey[17] had a mean of –28·5 ± 0·8 km s^{-1}, so up till that time there was no strong evidence for real variation of HD 111285's radial velocity. More systematic measurements made at Cambridge from 2007 onwards, however, testified to a gradual decline in the star's velocity, which reached a minimum of about –37 km s^{-1} in 2011/12; a *comparatively* rapid rise to –28 km s^{-1} ensued, but it has now levelled off. The general form of the orbit is now apparent, but the orbital period is evidently of

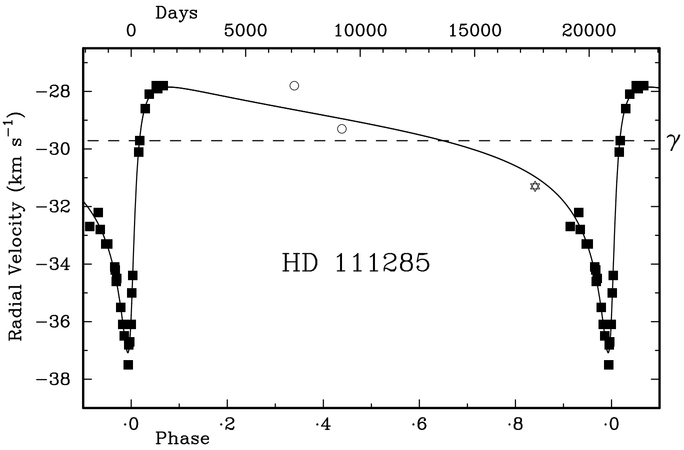

FIG. 4

The observed radial velocities of HD 111285 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The radial velocities cover less than a single cycle of the orbit, whose period is accordingly not well determined. All the observations were made by the writer. The earliest ones (the two open circles) date from about 40 years ago and were obtained with the original spectrometer[18] at Cambridge; they have been weighted ¼ in the solution of the orbit. The star symbol represents an observation from the spectrometer[12] made by Dr. J. E. Gunn and the writer for use with the Palomar 200-inch telescope. It seems unlikely that an accurate period can be determined for this star until the next minimum of velocity, due about 2070, has been observed; the uncertainty of the period should then be reduced from 10–20 years to as many days.

TABLE IV

Radial-velocity observations of HD 111285

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

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
1974 May 7·87*	42174·87	−27·8	0·340	+0·8
1980 Jan. 13·23*	44251·23	−29·3	0·439	−0·4
2003 Feb. 21·97†	52691·97	−31·3	0·841	−0·3
2007 May 8·05	54228·05	−32·7	0·914	−0·6
2008 May 6·94	54592·94	−32·2	0·932	+0·4
July 21·90	668·90	−32·8	·935	−0·1
2009 Mar. 30·06	54920·06	−33·3	0·947	−0·1
June 23·92	55005·92	−33·3	·951	+0·1
2010 Apr. 20·93	55306·93	−34·1	0·966	+0·2
May 23·90	339·90	−34·2	·967	+0·2
June 22·95	369·95	−34·6	·969	0·0
July 17·90	394·90	−34·5	·970	+0·2
2011 Jan. 19·25	55580·25	−35·5	0·979	+0·1
Apr. 9·11	660·11	−36·1	·982	−0·1
June 16·96	728·96	−36·5	·986	0·0
Dec. 5·27	900·27	−37·5	·994	−0·4
2012 Jan. 4·26	55930·26	−36·8	0·995	+0·2
Feb. 2·24	959·24	−36·7	·997	+0·1
Mar. 8·09	994·09	−36·1	·998	+0·2
Apr. 6·04	56023·04	−36·1	1·000	−0·2
May 11·97	058·97	−35·0	·001	+0·2
June 15·98	093·98	−34·4	·003	0·0
2013 Mar. 12·10	56363·10	−30·1	1·016	−0·2
Apr. 20·08	402·08	−29·7	·018	−0·1
Dec. 20·29	646·29	−28·6	·029	−0·1
2014 June 10·96	56818·96	−28·1	1·038	+0·1
2015 Apr. 1·07	57113·07	−27·8	1·052	+0·1
May 24·98	166·98	−27·9	·054	0·0
June 23·92	196·92	−27·9	·056	0·0
2016 Feb. 19·21	57437·21	−27·8	1·067	+0·1

*Observed with original Cambridge spectrometer
†Observed with Palomar 200-inch telescope

the order of 20000 days (55 years), of which only a small fraction (but a very significant one, embracing both nodes of the orbit) has been witnessed. Since the completion of the orbital cycle promises to be far beyond the observer's potential longevity, it seems appropriate to present the orbit as it stands now. The observations are set out in Table IV; the orbit is shown in Fig. 4, which incidentally illustrates how the two early observations made with the original spectrometer offer reassurance that the assigned period is of the correct order and could not, for example, be halved. The elements are given in the last column of Table VI. The uncertainty of more than a decade in the orbital period is regretted; on the other hand the high eccentricity of 0·83 has helped the epoch of periastron to be determined within an uncertainty of only 12 days.

On some occasions when HD 111285 was observed, the radial velocity of a visual companion a little over 100″ distant in a position angle of about 163° was also measured. The companion has its own entry in the *BD*, as 24° 2496 (HD 111285 itself being 24° 2495). *Tycho 2*[4] has provided its photometry, as $V = 9^{m}\cdot76$, $(B - V) = 0^{m}\cdot47$, while Sandage & Fouts[27] have listed its spectral type as F8 and have given a radial velocity of +4·3 km s^{-1} for it. The new velocities, all obtained with the Cambridge *Coravel*, are listed in Table V; they are measured from 'dips' that are hardly 10% of the 'continuum' deep and are smeared out by an apparent projected rotational velocity of about 13 km s^{-1} (not very well determined owing to the faintness of the star and the weakness of the dips). The radial velocities themselves do not agree together very well, but in view of the troublesome characteristics of the traces on which they are based their dispersion is not considered large enough to warrant (nor, for that matter, is it small enough to deny) a claim of real variability; the mean value is +3·8 ± 0·4 km s^{-1}.

TABLE V

Cambridge radial-velocity observations of BD +24° 2496 (HD 111285 B)

Date (UT)			*Velocity km s^{-1}*
2007	May	8·05	+2·8
2008	May	6·94	3·4
2009	Mar.	30·06	2·4
	June	23·92	3·4
2010	June	22·95	2·4
2013	Dec.	20·29	5·7
2015	May	24·98	3·3
2016	Feb.	3·23	+5·1

TABLE VI

Orbital elements for HD 32662, HD 76462, HD 78141, and HD 111285

Element	*HD 32662*	*HD 76462*	*HD 78141*	*HD 111285*
P (days)	998·51 ± 0·11	1943·4 ± 2·2	160·414 ± 0·036	21000 ± 4400
T (MJD)	55442·70 ± 0·38	55400 ± 5	56693·95 ± 0·23	56028 ± 12
γ (km s^{-1})	+30·00 ± 0·06	−6·80 ± 0·08	−16·20 ± 0·04	−29·71 ± 0·22
K_1 (km s^{-1})	13·65 ± 0·11	8·63 ± 0·16	16·02 ± 0·06	4·61 ± 0·09
e	0·7700 ± 0·0030	0·541 ± 0·011	0·2895 ± 0·0029	0·830 ± 0·022
ω (degrees)	299·3 ± 0·7	23·5 ± 1·7	15·6 ± 0·6	224·1 ± 2·1
$a_1 \sin i$ (Gm)	119·5 ± 1·2	193·9 ± 3·9	33·83 ± 0·12	742 ± 161
$f(m)$ ($M_\odot$)	0·0684 ± 0·0020	0·077 ± 0·005	0·0601 ± 0·0007	0·037 ± 0·010
R.m.s. residual (wt. 1) (km s^{-1})	0·37	0·48	0·21	0·22

References

(1) M. A. C. Perryman *et al.*, *A&A*, **331**, 81, 1998.
(2) *The Hipparcos and Tycho Catalogues* (ESA SP-1200) (ESA, Noordwijk), 1997.
(3) R. F. Griffin, *The Observatory*, **133**, 144, 2013.
(4) [Announced by] E. Høg *et al.*, *A&A*, **355**, L27, 2000.

(5) F. van Leeuwen, *Hipparcos, the new reduction of the raw data* (Springer, Dordrecht), 2007.
(6) J. E. Gunn *et al.*, *AJ*, **96**, 198, 1988.
(7) [Announced by] B. Nordström *et al.*, *A&A*, **418**, 989, 2004.
(8) C. W. Allen, *Astrophysical Quantities* (Athlone, London), 1973, p. 206.
(9) R. F. Griffin, *MNRAS*, **219**, 95, 1986 (Paper XIII).
(10) R. F. Griffin, *MNRAS*, **371**, 1140, 2006 (Paper XVI).
(11) E. S. Jackson *et al.*, *ApJ*, **125**, 712, 1957.
(12) 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. 94.
(13) J. F. W. Herschel, *Mem.RAS*, **35**, 21, 1867 (see p. 60).
(14) D. Lafrenière *et al.*, *ApJ*, **670**, 1367, 2007.
(15) A. N. Heinze, *ApJ*, **714**, 1551, 2010.
(16) E. L. Nielson & L. M. Close, *ApJ*, **717**, 878, 2010.
(17) K. M. Yoss & R. F. Griffin, *JA&A*, **18**, 161, 1997.
(18) R. F. Griffin, *ApJ*, **148**, 465, 1967.
(19) P. J. Melotte, *Mem.RAS*, **60**, 175, 1915.
(20) G. A. Bakos, *AJ*, **73**, 187, 1968.
(21) K. G. Strassmeier *et al.*, *A&AS*, **142**, 275, 2000.
(22) J. F. Heard, *PDDO*, **2**, 105, 1956.
(23) P. C. Keenan, *ApJ*, **91**, 506, 1940.
(24) A. Bartkevičius & R. Lazauskaitė, *Baltic Astr.*, **5**, 1, 1996.
(25) G. Kakaras *et al.*, *Vilnius Astr. Obs. Biul*, **22**, 3, 1966.
(26) R. F. Griffin & J. E. Gunn, *ApJ*, **191**, 545, 1974.
(27) A. R. Sandage & G. Fouts, *AJ*, **93**, 592, 1987.

CORRESPONDENCE

To the Editors of 'The Observatory'

If only …

So, I am not alone in being frustrated at the misplacement of the word 'only' in sentences and tired of reconstructing such sentences to obtain the 'most likely' meaning for the context[1]. To illustrate the problem, I use the simple sentence: "I walked down the street." Placing 'only' before each word in succession gives five sentences, each with different meaning and conveying different information about the situation described.

Native English-speakers are (now) custodians of an international language and, so, should make the effort to get it right — especially those writing with 'scientific' accuracy. Stating the intended meaning takes only a little effort but it should be the author who makes the effort (once) not the reader (many times).

Yours faithfully,
LES BALDWIN

1 Upton Grove Cottages
Tetbury, Upton
Gloucestershire
GL8 8LP

2016 March 31

References

(1) P. Helbig, *The Observatory*, **136**, 98, 2016. (See p. 100.)

REVIEWS

The Road to Relativity, by Hanoch Gutfreund & Jürgen Renn (Princeton University Press, Woodstock), 2015. Pp. 237, 25·5 × 19 cm. Price £24·95/$35 (hardbound; ISBN 978 0 691 16253 9).

The somewhat prolonged centenary of General Relativity (key papers written, submitted, and published across 1915–16) has, not surprisingly, brought forth a pride of books, a coven of papers, and a gaggle of talks, including a few committed by me.

The Road, subtitled *The History and Meaning of Einstein's 'The Foundations of General Relativity'*, has as its core 101 facsimile pages of what is generally called *Die Grundlage*, hand-written with many of Einstein's own corrections, and not to be confused with the 1913 *Entwurf* theory of Einstein and Grossmann. It was submitted and published in 1916 and includes the ideas plus amplification and correction of four 1915 papers.

Another 101 pages of annotations are preceded by assorted prefaces, foreword (by John Stachel), and introductions, and followed by notes on the annotation pages, a postscript (on solutions of the equations, cosmology, and so forth), a time-line from the *wunderjahr* of 1905 to the 1932 withdrawal of the cosmological constant (a joint paper with de Sitter), capsule biographies of forty "physicists, mathematicians and philosophers relevant to Einstein's thinking", some suggestions for further reading, and a translation of *Die Grundlage* from previously-published sources.

The authors emphasize (in a grey box) that their annotations contain "background material related to the relevant scientific developments and correspondence with peers … not the social and political environment, nor … the deteriorating family relations during those years." They also say that Einstein felt isolated by 1916 because "in contrast to most of his German colleagues, he was openly critical of the war".

The forty (with Albert as Ali Baba?) range alphabetically from Max Abraham to Hermann Weyl and chronologically from Euler and Hume (b. 1707 and 1711, respectively) to Synge (d. 1995). Only two were American, Hubble and Hale, and I met only one, George Gamow. Nördstrom made the cut but not Reissner, and missing is Max Planck, whom Einstein once said he loved.

The timeline reveals many curiosities, including the Thursday effect: the four key November 1915 papers were submitted on the 4th, 11th, 18th, and 25th, all Thursdays. Ditto for the 29 October 1914 submission of an amplification of the *Entwurf* theory, the handover of Schwarzschild's paper with his radius (1916 February 24), the *Foundation* itself (1916 March 20), and the two papers on cosmology (1917 February 8) and gravitational waves (1918 January 31). Originally I checked the 1915 November days hoping for some insight into 'work habits' — all Mondays? (Aha! He worked weekends); all Fridays? (Aha! He liked to take the weekend off.) But all Thursdays? Perhaps that was the day the editor opened his mail?

Gerald Holton has written and recently reaffirmed the view that only there, and then, and from Einstein could the general theory have come about in the way it did. The large number of other outstanding physicists and mathematicians in and around Berlin, and Einstein's appointment (which brought in a decent income with almost no formal responsibilities) were essential. The authors' annotations somewhat confirm this, with discussions of the roles of Hilbert, Lorentz, and von Laue, as well as the generally-recognized Besso and Grossman. Others of the 40, including Ehrenfest, were more relevant to

Einstein's developing views on quantum mechanics. My own thought is that the relative absence of international meetings and invitations to give talks in other countries might also have helped.

Conflict-of-interest statement: I purchased my copy of *The Road to Relativity*, and so am bound to like it at least $35·00's worth. There would certainly have been other more learned reviewers possible, and I can only beg to be excused from Holton's variant of Wittgenstein's principle, "Whereof one is not competent, thereof one must be silent." — VIRGINIA TRIMBLE.

Captain Cook's Computer: The Life of William Wales, F.R.S. (1734–1798), by W. Wales (Hame House, through YPD books), 2015. Pp. 499, 20 × 12·5 cm. Price £13·99 (paperback; ISBN 978 0 9933758 0 4).

William Wales led the British expedition to Hudson Bay at the transit of Venus in 1769 and sailed with Captain Cook on his second voyage of discovery (1772–1775). He assisted the Astronomer Royal (Maskelyne) with the calculation of the *Nautical Almanac* and served for many years as the head of the Royal Mathematical School at Christ's Hospital.

He was born in Warmfield, Yorkshire, and a great deal is known about his family but less about his education. He walked to London in about 1760; the exact year is not known. To start with, he occupied himself with writing an ode in honour of William Pitt the elder and writing mathematical articles for the *Ladies Diary*.

At that time there were two problems at the forefront of astronomers' minds. First was the determination of longitude at sea, and second, the determination of the Sun's distance in terrestrial units. The longitude problem was so important to Britain's naval and commercial interests that a substantial prize, administered by the Board of Longitude, was offered to anyone who could solve it. There were two viable solutions, both involved with determining time on the standard meridian. One was to produce a chronometer capable of accurately reproducing the time on the standard meridian, during and after a rough sea voyage. The other depended on measuring the position of the Moon, relative to the stars, and comparing it with the position calculated from the best available theory of her motion. Both methods required the navigator to measure the local time by observing the altitude of the Sun or a bright planet, and to consult a reliable almanac for the position of the object observed.

Nevil Maskelyne, later Astronomer Royal, was a Cambridge mathematics graduate who sailed on an East India Company ship to St. Helena to observe the transit of Venus in 1761. During the voyage he regularly determined the ship's position by the method of lunar distances. The ship's captain was impressed by Maskelyne's accurate prediction of the landfall on St. Helena. However, even with his mathematical training, it required several hours' work to derive the ship's longitude from an observation. Maskelyne realized the necessary time could be much reduced if an almanac were published in advance, easy to use and to understand. He petitioned the Board of Longitude for money to hire computers, who in those days were real people, to calculate the tables in the almanac. With the Board's approval he set up two teams; each consisted of two computers and a comparer. The computers worked at home and did not know the identity of the other. They sent their work to the comparer who checked them for agreement, and who, when satisfied with their accuracy, sent them to Maskelyne. Each team was given a six-month period to cover. The first *Nautical Almanac* was published in 1766 for the year 1767. Wales was one of the first computers and he may have been introduced to Maskelyne by Charles Green, Maskelyne's assistant at Greenwich and later Wales' brother-in-law. Alternatively Maskelyne may have known of Wales' reputation as a mathematician. Wales worked intermittently on the *Nautical Almanac* until the year before his death.

Transits of Venus provided the best way to measure the distance of the Sun. Observations of the transit in 1761 had not been a success because of poor planning and the effects of weather, warfare, and disease. The Royal Society determined to plan the observations better for the transit of 1769, particularly as there would not be another transit until 1874. They eventually sent out three expeditions, each of two people, to the South Seas, the North Cape, and Hudson Bay. In spite of his preference for a warm country, Wales was to be the senior of the Hudson Bay party. The Royal Society provided their instruments and equipment and they left in 1768 because their passage would be blocked by ice during the winter and might not be open again before the transit. Their chosen site was Prince of Wales Fort, a trading post of the Hudson Bay Company who afforded the astronomers every support. The same was not true of the British colonial customs which confiscated their cold-weather gear. This was not the first oppressive act of the customs which led to the Boston Tea Party five years later. During the winter Wales determined the position of their observatory on clear nights and calculated navigational tables on the cloudy ones. Such tables were an aid to the determination of local time and so to finding the longitude by both methods. He also kept a journal describing the natural history of the area. The sky was partly cloudy on the day of transit but they made the planned observations. They returned home again before the ice blocked their passage.

James Cook had recently returned from his first voyage to the South Seas where he charted the coastline of New Zealand and eastern Australia. A further voyage was being planned and Wales was to be Cook's astronomer. The Royal Society was anxious to confirm or deny the existence of a great southern continent to be found south of the latitudes reached on the first voyage. Cook reached the pack ice of Antarctica and the island of South Georgia but never found a major continent. On the first voyage Cook had found his longitude from lunar distances and the eclipses of Jupiter's satellites. By the time of Cook's second voyage, Harrison had built a reliable and accurate chronometer and secured the Longitude Prize. Harrison's chronometer was too precious to risk being lost by shipwreck or capture. Instead, Cook carried a chronometer made by Kendall in imitation of Harrison's prize-winning one. Cook revisited New Zealand and Tahiti and many other islands, some of them new discoveries. At every opportunity Wales measured their position, the declination and inclination of the compass, and the tides. When ashore he observed the natural history and the ethnography of the inhabitants and recorded everything in colourful prose, occasionally interspersed with poetry.

Readers of Pepys' diary will remember his persuasion of Charles II to found a mathematical school within Christ's Hospital. It was a perennial problem to find a master fit to head the school; one who had sea-going experience, was versed in mathematics, and was neither habitually drunk or idle. On his return from the Pacific, Wales filled those requirements exactly. Discipline was poor when he took up the post but he soon restored order, armed with his experience in the navy. His students went on to join the Royal Navy, Merchant Navy, or commercial houses; some went to university. One of his notable pupils was Samuel Taylor Coleridge who read Wales' journal in manuscript. Its influence can clearly be found in *The Rime of the Ancient Mariner*. Wales remained at Christ's Hospital for the remainder of his life and, in addition to his teaching duties, carried on with the *Nautical Almanac*, and was secretary to the Board of Longitude. He was elected a Fellow of the Royal Society in 1779.

This book is aimed at two readerships: students of the history of navigation and students of family history, especially the Wales family. Genealogical charts are presented separately at the end of the book, together with some useful appendices. Nevertheless the two aims do not always sit happily together. The author, a descendant of the subject, is clearly widely read and widely travelled, which is shown by the extensive references at the end of each chapter. — DEREK JONES.

Inspiration of Astronomical Phenomena VIII: City of Stars (ASP Conference Series, Vol. 501), edited by B. P. Abbott (Astronomical Society of the Pacific, San Francisco), 2015. Pp. 337, 23·5 × 15·5 cm. Price $88 (about £60) (hardbound; ISBN 978 1 58381 886 2).

The series of INSAP (Inspiration of Astronomical Phenomena) conferences has become established in the calendar, and this is the third set of their proceedings to be published by the ASP and reviewed here. It contains 34 contributions, most of about ten pages, covering an incredibly broad range of topics, split between headings like Art, Architecture, History, Music, Literature, Myths, Ancient Civilizations, Resources, and Contemporary Artists. Two excellent contributions look at astronomical decoration of ceilings. Krupp's *Stars on the Ceiling* gives us a broad historical sweep of ceilings with astronomical images, from tombs in pharaonic Egypt to Grand Central Terminal in New York, followed by a detailed discussion of the ceilings and decoration of the Griffith Observatory. Ricci's contribution on the relatively unknown astronomical ceiling in the Pennsylvania State Capitol, *The Passage of the Hours*, introduces us to the American patriot and astronomer David Rittenhouse, also not as well-known as he ought to be. He observed the 1769 transit of Venus, calculated the solar parallax and, echoes of Newton, became the first director of the United States Mint. I was intrigued by Assasi's paper on Roman Mithraism, his association of the Mithras with the north ecliptic pole, and the possible interpretation of the tauroctony in terms of the sequence of ages caused by axial precession. The absence of written records leaves this all rather uncertain. This lack also plagues the study of alignments of ancient monuments: Belmonte & González-García measured orientations of large numbers of Hittite and Nabatean religious structures and found peaks in the frequency distributions of the declinations (some labels in their Fig. 3 need to be shifted by 20°) to coincide with solar and lunar phenomena. Such results are very suggestive, but one longs for textual corroboration. Contemporary art and music are not ignored, and I liked Fraknoi's comment, in his survey of music inspired by astronomy, on a project of John Cage: "Such music is, frankly, more fun to contemplate than to listen to."

Overall the standard of production, including the illustrations, is very high and a credit to the editor. The contributions range in content and style, from the 'historical' to 'creative', and I struggled with sentences like Cro-Ken's "By mixing paints and catalysts I 'launch' a painting into motion to reveal the invisible push-pull forces that shape and form all things on Earth and places scattered throughout the universe." I was dismayed to read in Miller's paean to progress that "Photographic images are captured by the Hubble Space Telescope, amongst many others". Nearly all the articles are amply referenced — providing valuable jumping-off points for readers wanting to follow up particular topics. What I missed was any sense of *Conference*: the interactions and interconnections between participants coming from so many different directions must have been fascinating and it would have been valuable to have at least some idea of them. Perhaps the very heterogeneity of the subjects inhibited easily recordable discussion? There is no conference summary as such, but I thought Corbally & Rappaport's 'Art as an evolutionary adaptation' wrapped up the theme of the meeting very well. I recommend this book as a resource for those responsible for outreach or teaching astronomy to non-scientists, and a source of great interest for the rest of us. — P. M. WILLIAMS.

Asteroids IV, edited by Patrick Michel, Francesca E. Demeo & William Bottke (University of Arizona Press, Tuscon), 2015. Pp. 895, 28·5 × 22·5 cm. Price $75 (about £51) (hardbound; ISBN 978 0 8165 3213 1).

Since asteroids are just the left-over fragments of a failed attempt to accrete a planet between the orbits of Mars and Jupiter, it is surprising that they captivate scientific curiosity much more than their total mass seems to justify. Maybe their interest is boosted by the fact that, unlike the vast majority of other astronomical objects, some asteroids 'come to see us' — here on Earth — in the form of meteorites and the progenitors of crater-producing events. Maybe Solar System astronomers are just fascinated by looking at the celestial equivalent of car crashes. It is also obvious that the total asteroidal mass is decreasing with time and we do not see the original members of the asteroidal family, we just see the bits left over after large asteroids have smashed into small ones and been broken up. These fragments also give us a unique chance of looking inside what could have been another terrestrial planet. The collisions have let bits of the crust, mantle, and core fly off into space. Also, after the Moon, asteroids are the next-easiest venue for spacecraft trips, and this has led to proposals of manned missions, mining adventures, and the reality of NASA's *Dawn* on-going mission to orbit asteroids (4) Vesta and (1) Ceres and thus characterize their regoliths and cratering.

The asteroidal scientific community is very good at periodically reviewing its scientific progress, and the book under review is the fifth in a fascinating series of compendia. It started in 1971 with a conference proceedings that is jokingly referred to as '*Asteroids 0*' — the book *Physical Studies of Minor Planets*. The University of Arizona, then organized another conference in Tucson in 1979, and that spawned the book *Asteroids*. Numbering then became the norm and *Asteroids II* and *Asteroids III* came out in 1989 and 2003, respectively. Last year we saw the production of *Asteroids IV*.

Major steps have been taken in the years between books III and IV. The discovery of a multitude of exoplanets has convinced some cosmogonists that planets can migrate, and so astronomers have investigated the effects on the asteroid belt of the gas giants Jupiter and Saturn possibly changing their semi-major axes during the history of the Solar System. Computers have also become much more sophisticated, and orbital-evolution modelling is now much speedier, and the results of inter-asteroidal collisions have been modelled with greater accuracy. The known collection of asteroids has increased significantly: telescopic surveys like *WISE* (NASA's *Wide-field Infrared Survey Explorer* mission, launched in 2009 December) have increased the numbers of asteroids with estimated sizes and spectral details by over a hundredfold. Sample returns and space surveillance have also solved the 'ordinary chondrite paradox' in which space weathering was found to have masked the fact that the commonest meteorite (the carbonaceous chondrite) has the same composition as the commonest asteroid, the S-type. The asteroidal hazard is ever with us and that was underlined by the bolide explosion over Chelyabinsk in 2013 February.

This is a superb book. The world experts in the subject have been rounded up and have produced a detailed, well-referenced, and thorough overview of our current knowledge of the asteroid population and the way in which it influences our knowledge of terrestrial-planet formation and evolution. I was delighted to read the many predictions of how our knowledge is planned to be improved in the future, and I will await *Asteroids V* with eager anticipation. I was, however, slightly disappointed that, even with 895 pages at their disposal, no one was encouraged to overview the investigations into the historical appreciation of these fascinating minor members of our planetary family. — DAVID W. HUGHES.

Young Stars and Planets Near the Sun, edited by Joel H. Kastner, Beate Stelzer & Stanimir Metchev (Cambridge University Press), 2016. Pp. 316, 25·5 × 18 cm. Price £79·99 (hardbound; ISBN 978 1 107 13816 2).

Every IAU Symposium, of which this was number 314, has some unique feature, though not quite all of them have yielded printed proceedings. The paper version of Symposium 314 is distinguished by the smallest photograph of the conference participants ever. It is 13·5 × 5 cm (compare the page size above). Most participants could nevertheless probably find themselves. (I have the misfortune to be on the stage-left end of the 2nd row, displayed in full 'somnolent ibis' pose, and I gave the historical introductory talk on beginnings of the study of young stars, star formation, star streams, and exoplanets (conflict-of-interest statement).)

Possibly also unique was the structure of the concluding talk by Michael C. Liu, who gave us five take-aways and five predictions. The take-aways (no pizza) included the importance of initial conditions, the increasing complexity of processes when you look at them more closely, and advocacy of the right way to compare models and data (the wrong ones being called "Chi by eye" and "Overlay").

For the five predictions, participants attending the concluding-remarks session (about 92 out of about 135) were invited to vote on possible outcomes. Most agreement occurred for "Will we have a complete predictive theory of how stars get their masses?" which garnered 91 no's and 1 yes. Majorities thought that most of the currently advertised young moving groups (two in common with those of Eggen many years ago) were robust and that two or more new moving groups would be found in the next ten years, within 100 pc (meaning from *Gaia*, *LSST*, *etc.*). Liu also presented illustrations of nine of the more vivid acronyms in use in the field. I understand PALMS, LACEwING, *SPHERE*, HAZMAT, DALI, BASS, and BANYAN, but not SACY and JAXON (not, I think, the government advisory committee, most of whom are slightly, though perhaps only slightly, better looking than the faces shown).

The number of exoplanets that had been imaged to date was said to be seven; TWA revealed itself as an association in Hydra rather than a defunct airline; and magnetic fields were widely said to be important (though, in the customary absence of an index, I cannot say just how widely, this being more dependent on numbers of speakers than on individual breadths).

Sadly not in the proceedings is the evening public lecture by Ben Zuckerman on the potential for existence and discovery of higher forms of extraterrestrial life. I think one of his points was that, if they were intelligent, they would have built the equivalent of the *TPF* (*Terrestrial Planet Finder* mission) and so already know about us. The implication that terrestrial inhabitants, having decided not to build *TPF*, are not intelligent must be taken to exclude speaker Zuckerman, the other two members of the post-talk panel (Karin Oberg and me), and the audience members who brought up many interesting or related topics and a few that were both. — VIRGINIA TRIMBLE.

Lunar and Planetary Cartography in Russia, by V. Shevchenko, Z. Rodionova & G. Michael (Springer, Heidelberg), 2016. Pp. 145, 24 × 16 cm. Price £72/$99 (hardbound; ISBN 978 3 319 21038 4).

While this is a book that deserved to be written, at least for completeness of the record, it is not one that is likely to find a large readership (something, one suspects, that the publisher has chosen to reflect in its price). It is the first detailed history of lunar and planetary cartography in Russia and the Soviet Union and, as such, it merits its place in academic reference libraries and will serve to complement western studies such as Greeley and Batson's *Planetary Mapping* (1990). The coverage of the volume is confined to space-age cartography, from the first images of the lunar far side taken by *Luna 3* in 1959, which led to the first atlas of the averted hemisphere (1960) and the first lunar globe (1961), through to the geological and other thematic maps of the Moon, Venus, Mars, Phobos, and Deimos produced in the first decade of the 21st Century. There is also an interesting chapter on the evolution of nomenclature practices. The authors are well qualified to produce such a volume, having played major roles in planetary science, both in Russia and internationally, over many years.

However, the book is not for the casual reader. Not only is its focus a narrow one, but it is beset by wooden language replete with neo-Soviet phraseology and strewn with institutional acronyms that will surely provide a considerable obstacle to the reader not well versed in the Russian language and scientific world. I am assuming that this volume is a translation from an original Russian text, which might account for the stylistic awkwardness, but nowhere is this made clear and no translator's name is given.

The volume is fully annotated with references and, despite the focus on Russian cartography, it is good to see much evidence of international collaboration and acknowledgement of work done by western cartographers and planetary scientists. There are some slips in the transliteration of Russian titles, and the volume would have benefitted enormously from the addition of an index, but despite that it will prove to be of interest to the specialist in planetary mapping. — Bill Leatherbarrow.

Giants of Eclipse: The ζ Aurigae Stars and Other Binary Systems, edited by T. B. Ake & E. Griffin (Springer, Heidelberg), 2015, Pp. 202, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 09197 6).

This volume was inspired at a meeting sponsored by the American Astronomical Society on 'Giants of Eclipse' held at Monterey, California, in 2013 July. The editors wisely strove to produce something that transcends the ephemeral snapshot of a field such as is the typical conference proceedings, a goal they most certainly attained. Thomas Ake and Elizabeth Griffin invited concise but comprehensive reviews of the important aspects of the observation, analysis, and theory of these uncommon binary systems that offer up rare opportunities for probing the outer atmosphere of a cool giant or supergiant when its hot companion is occulted. In addition to its prototype, this elite class of binaries includes such luminaries (pun intended) as 31 Cyg, 32 Cyg, VV Cep, 22 Vul, τ Per, and γ Per. While technically not a ζ Aur system due to the bizarre nature of its companion, the improbable 27-year-period binary ε Aur, which underwent its most recent eclipse during 2009–2011, is also addressed. That star's lengthy eclipse was particularly noteworthy due to the evolution of traditional observing techniques and instrumentation and the emergence of new capabilities over the last quarter century.

The contributors to the book's seven chapters include Vladimir Airapetian, Thomas Ake, Wendy Hagen Bauer, Philip Bennett, Manfred Cuntz, Joel Eaton, Elizabeth Griffin, Daniel Huber, Brian Kloppenborg, Klaus-Peter Schröeder, Robert Stencel, and Gerard van Belle. In addition to overviews of observational results and the modelling of their chromospheres for all the ζ Aur stars, there are chapters dedicated to the heating and wind acceleration in cool, evolved stars, VV Cep, and ε Aur, as well as to the modern developments in optical interferometry and asteroseismology.

In his foreword, Edward Guinan cites the 1970 *Vistas in Astronomy* review of this binary class by K. O. Wright of the Dominion Astrophysical Observatory. The remarkable advances in modern detectors compared with the photographic emulsions and photomultiplier tubes that fed the science underlying Wright's review, and the advent of new observational and theoretical techniques over the ensuing four-plus decades, will ensure that this excellent volume serves as the gateway to the study of these stars for years to come. — HAROLD A. MCALISTER.

Astrophysical Black Holes, edited by F. Haardt *et al.* (Springer, Heidelberg), 2016. Pp. 314, 23·5 × 15·5 cm. Price £40·99/$59·99 (paperback; ISBN 978 3 319 19415 8).

The editors explain their title as "black holes seen in and from an astrophysical perspective." Indeed the astrophysical sort are the only kind we have evidence for or expectations of today. This assumes that primordial (mini) black holes are astrophysical rather than cosmological and that non-production at the *Large Hadron Collider* will remain true into the future. What the *SSC* might have done we will never know, since it became merely a hole in Texas. It is necessary to go to the footnotes to learn that the school from which these are the 'lecture notes' was held at Lake Como, in 2012 according to the editors. Some of the chapters have dated, therefore.

Units are not consistent among the eleven authors of the seven chapters. Black holes form with $c = G = 1$, but accrete in cgs, while orbits of stars around central black holes in active galaxies are described in sensible astronomers' units of solar masses, parsecs, years, and km s^{-1}. The references are all given as numbered notes, some sequential through the chapter, others alphabetized. And there is no index of any kind. Nor do the authors refer to each other's chapters, though the school was presumably more interactive. Galaxies get four chapters, stellar-mass black holes only one, with a brief nod to the intermediate-mass sort.

The illustration I found most informative is Fig. 3.2 of R. Fender & T. Muñoz-Darias' chapter on accretion and feedback in stellar-mass black holes. It shows, to scale, according to their best current estimates, the dimensions of the 20 then-known black-hole X-ray binaries, the companion star, the Schwarzschild radius, the accretion disc, and the separation. A few donor stars are red or yellow; the brown ones are more mysterious. The largest, brownest, and most 'discy' system is GRS 1915+105, with separation just a bit larger than the Sun and Mercury. The smallest is GRO J0422+32, in total no larger than the disc of Cyg X-1.

The most innovative chapter, from the point of view of an old-fashioned optical astronomer (yes, I learned to cut glass and slosh developer) is the last, by Thibaut Damour & Allesandro Nagar on the effective one-body (EOB) approach to the general relativistic two-body problem. Notoriously, Newtonian gravitation requires numerical or approximate approaches for three or more bodies interacting, and GR even for two. The numerical calculations needed, for instance, to provide templates of expected gravitational waves for the *LIGO* and related projects are very computationally-intensive, extensive, and expensive. The authors propose their method to describe the merger of a pair of comparable-mass black holes and the ring-down of the product, just what has been needed, one supposes, for recent and forthcoming *LIGO* events. They suggest that the EOB technique can fill in parameter space among numerical and post-Newtonian calculations to yield a dense net of templates.

Fifty-four of the 142 references (which go back to Droste and de Sitter (1916) — separately!) are to their own work. Neither of the authors, however, appears among the 1004 of the first *LIGO Physical Review Letter*. This suggests that the method was, in the end, not adopted by the *LIGO* team, although Figures 7.2 and 7.3 show (according to the captions) excellent agreement between their waveforms for in-spiral, merger, and ring-down time and numerical relativity results. "According to the captions" is necessary because the time-scale in Fig. 7.2 compresses all the interesting bits into about 1 mm of graph, rendering both amplitude and phasing agreement impossible to see. This pair of calculations was for an equal-mass binary, and the text notes equally good agreement (to within the numerical errors) for one unequal-mass case.

Astrophysical Black Holes is altogether a puzzling book. I cannot imagine it as a starter-text or as an essential part of a current researcher's library, though at just about 20 cents per page, it is not outrageously expensive by modern standards. Very possibly the summer school was more satisfactory, but the participants are not listed, so one cannot poll them. — VIRGINIA TRIMBLE.

Building and Using Binoscopes, by N. Butler (Springer, Heidelberg), 2015. Pp. 342, 23·5 × 15·5 cm. Price £19·95/$34·99 (paperback; ISBN 978 3 319 07688 1).

It's hard to imagine the disappointment that the author of this fine book must have felt when he first held a copy in his hands. His book is profusely illustrated, with images that are intended to assist visual observers interested in using both eyes to scan the heavens. But the publisher has let both the author and his readers down badly. A large fraction of the digital photos — perhaps as many as a third — are incorrectly formatted, with unconstrained horizontal and vertical proportions. The resulting images range from the mystifying to the utterly bizarre. Yes, the technical material illustrated in the book is probably unfamiliar to the average picture-editor, but I find it staggering that no-one at Springer recognized something amiss with the alternating Michelin-Man and Lowry stick figures. Is that what they think astronomers look like?

Norman Butler is a noted amateur telescope maker with a background in professional optics, and evident expertise in the practicalities of instrument-building. Unlike many of his peers, he has eschewed digital astro-imaging (in this book, at least), and concentrated on the theory and practice of visual observing. As might be expected given his background, his treatment is very complete, and is aimed squarely at anyone — beginner or advanced — who aspires to visual observation with both eyes. In fact, there is also much to interest and inspire conventional telescope users, although the emphasis is definitely on the twin-telescope brigade. The book is a valuable addition to Springer's *Practical Astronomy* series authorized by the late Sir Patrick Moore, and makes a genuine contribution to the 'how-to' literature of amateur astronomy.

In its ten chapters, *Binoscopes* rambles cheerfully through the whys and wherefores of binocular telescopes in a way that is reminiscent of the three volumes of the classic *Amateur Telescope Making*. Indeed, its quirky diagrams and contributions from other authors give it a rather similar flavour. Unlike *ATM*, however, the new book has a fair number of errors, mostly typographical, which give the impression of having been hastily put together against a deadline. Given the wealth of useful material it contains, I think that can be forgiven.

I would certainly recommend this book to any amateur astronomer keen to explore the possibilities offered by doubling up their telescopes, and I hope it does well. In particular, I hope it does well enough to warrant a second edition. Then we might see the end of those weird distorted images. — FRED WATSON.

The Expanding Universe: A Primer on Relativistic Cosmology, by W. D. Heacox (Cambridge University Press), 2015. Pp. 292, 25 × 18 cm. Price £39·99/$74·99 (hardbound; ISBN 978 1 107 11752 5).

Why another introductory cosmology book? As I mentioned in a recent review[1], different books on this topic can and do have different emphases. This book's emphasis is quite different. With fewer, smaller pages, it has less material on cosmology than some more-extensive books reviewed recently in these pages[1,2], even though the latter cover both cosmology and extragalactic astronomy. However, it has more material on General Relativity than probably any other book at the same general level, filling an otherwise vacant niche. Have no fear, though, this is limited to what is necessary in order to understand the derivation of the Friedmann equations. While such material is available in more advanced texts, most introductory cosmology books present them almost as a *deus ex machina*, though often so-called Newtonian cosmology is used to justify their 'derivation', or at least make it plausible. While all 4800 terms of the Ricci tensor are not listed, at least the number is mentioned.

The book also emphasizes theory over observation. For example, there are detailed discussions of various definitions of distance, including, of course, the luminosity distance, but from a very theoretical point of view. The reader should "[c]onsult any good book on astronomical photometry for the many practical details inherent in brightness measurement". The fact that cosmology is now a data-driven science has led to observational aspects being emphasized in many recent books. Basic theory is of course available in older texts, but these often contain archaic notation, series expansions appropriate for small redshifts, and discussion of topics no longer important. While all of those are important in the history of the subject, the beginner is better served by a modern introduction, with just a brief nod to older traditions (primarily as a warning about what one might find in the literature: *e.g.*, q_0, the steady-state model, and so on). Observational data do make their presence felt in the examples, however, where a Hubble constant of 72 km s^{-1} Mpc^{-1} is used. Also, the concordance model has a chapter all of its own.

In addition to the extensive background information on General Relativity, the book offers a detailed overview of homogeneous and isotropic cosmological models based on GR, so-called Friedmann–Lemaître–Robertson–Walker models. Important topics which are usually found only in (some) more-advanced texts, such as the various types of cosmological horizons and the evolution of the cosmological parameters with time (as opposed to simply measuring their current values), are given more than just a brief mention. Despite the emphasis on theory, potential application to observations is never far away; 'theoretical observational cosmology' is a good description of the bulk of the book. Extensions to the standard model, such as MOND, modified field equations, and inhomogeneous models are at least mentioned. Different authors have different notation schemes, and sometimes this is an advantage: some are more useful in certain applications, and understanding the differences and the reasons for them can often lead to deeper insights. The notation here is rather standard, and no reference is made to other schemes, but the inclusion of three different forms of the Robertson–Walker metric is a nice touch.

The preface and introduction set the stage; the nineteen main chapters are grouped into five parts: 'Conceptual foundations', 'General Relativity', 'Universal expansion', 'Expansion models', and 'Expansion history'. The first part, with chapters on 'Newtonian cosmology', 'General Relativity', and 'Relativistic cosmology', is basically a summary of the book, one goal of which is to underpin observational cosmology with a better foundation in GR than other books at this level. The part on GR is a brief but good introduction to the topic, leading up to cosmological applications (as opposed to black holes, gravitational waves, *etc.*). The material of Parts III and IV is often lumped together in other books; here, the theoretical foundations are laid out first, then observational constraints are discussed. (These two parts are similar to the treatment in Chapters 14–19 of Harrison's classic textbook[3], though the latter, like the rest of that book, provides more background, and mathematical details are often relegated to the 'reflections' at the end of each chapter. As such, Heacox's book is, as far as the basic topics go, a compact summary of Harrison's treatment, though Harrison, in his much longer book, covers many related topics (history of cosmology, black holes, *etc.*) as well.) Part V is a summary of structure formation from the particle through the plasma to the galaxy era, and also includes an afterword. Appendices provide more details on 'Differential geometry', 'Newtonian approximations', 'Useful numbers', and 'Symbols' (and acronyms). Each chapter concludes with a few problems (solutions for instructors are available on request). Part III starts with a brief summary "For those skipping Part II". The book — like most introductory cosmology books — can be read without reference to the discussion of GR in Part II, though those wishing to go a bit beyond the usual level can benefit from it, especially because it concentrates on the cosmological applications. This and the first appendix provide a good starting point for those wanting to learn more about GR. (Parts IV and V also start with summaries, but of their own chapters rather than those of the previous part.)

About three dozen references precede the index. Except for Guth's classic paper on inflation, all of these are books, most of them recent, and some have been reviewed in these pages. These are an excellent starting point for those wishing to move beyond this book, both for more in-depth treatments of cosmology and for more specialized topics. I would add only Harrison's textbook[3], Bondi's classic monograph[4], and Barrow's excellent history of cosmology, reviewed here not long ago[5]. There are several graphs and a few black-and-white photos, but several equations, as appropriate for a mainly theoretical book. Notes are fortunately footnotes, not endnotes. Sometimes they refer to the references at the end of the book, and occasionally more specialized works are cited in the footnotes.

The book is clearly written and there are only a few typographical errors and so on. The author, an emeritus professor at the University of Hawaii, where he founded the undergraduate-astronomy-degree programme, has written papers in many fields in astronomy, but none on cosmology. This is probably responsible for the book's well-balanced approach; experts sometimes don't see the forest for the trees and/or put too much emphasis on their own areas of expertise, which is fine for a monograph but not for a primer. On the other hand, this might explain a few things which are done slightly differently from most introductory cosmology texts: often including, in addition to matter and a cosmological constant, a radiation term (though, except in the early Universe, it can usually be neglected); using "classical" to mean 'non-GR' (usually, 'classical cosmology' is the study of Friedmann–Lemaître–Robertson–Walker models, *i.e.*, without considering inhomogeneities — other than the presence of objects which are essentially test particles, the physics of the hot Big Bang, inflation, and so on); defining the Hubble Constant to be the current value of the Hubble Parameter (although not constant in time, the name refers to its role as a constant of proportionality at a given time, hence a time-dependent Hubble Constant is not an oxymoron — in contrast to the cosmological constant, which is, by definition, constant in time). While I would stick with the standard treatment for those items, in other cases I prefer the author's minority view: I share his distaste of the term "dark energy" to denote the cosmological constant (or something similar)* and it is good to point out that the connection between the cosmological constant and vacuum energy (in the particle-physics sense) is tenuous at best. While it might have been true at some time in the past, I don't think that today most cosmologists assume that the Universe is perfectly flat on 'philosophical' grounds (related to the flatness problem), though this might still play a role at some level; rather, (near) flatness is now an observational datum.

Some readers might be confused by Fig. 10.2, which shows two different models with $\Omega_{\Lambda 0} = 1{\cdot}8$ and $\Omega_{\Lambda 0} = 0{\cdot}27$, a Big Bang universe which collapses, and a bounce model which contracts from infinity to a finite radius before expanding again. Conventional wisdom is that specifying those two parameters completely determines the expansion history, since trajectories in the corresponding plane do not cross. Fig. 10.3 also indicates that this model has no big bang. The confusion is due to discussing cosmological models in terms of the present-day values of the parameters, even if the scale factor doesn't reach the current scale factor. This is confusing, but consistent, if one understands what is being done. I have seen it occasionally elsewhere, so it is good to have it here, but a paragraph explaining the apparent contradiction would have been useful, especially as an example of how different notation schemes can sometimes lead to apparent contradictions.

There are just a few real mistakes: GR is not necessary to resolve the twins paradox; the cosmological constant was not completely neglected between the discovery of the expanding universe and the discovery of the accelerating universe; describing the Hubble time, even approximately, as the time for the Universe to double in size does more harm than good; the same goes for the claim that cosmological redshifts are partially gravitational in origin; only accelerating (not non-decelerating) models have a finite event horizon. But these are almost negligible compared to the strengths of the book.

Recommended? Of course! Most modern introductory cosmology books are light on theory; older ones are often too old-fashioned for easy comparison with more modern works. This is an excellent introduction to theoretical cosmology which nevertheless takes recent observations into account. Its emphasis on GR puts it in a class by itself for books at this level; nevertheless, the non-GR parts of the book stand alone. It leaves out more-general background (which is covered well elsewhere) in favour of discussing details which often aren't

*Sean Carroll has pointed out[6] that lots of things are dark and everything has energy; unfortunately, his better term "smooth tension" has not caught on.

mentioned at all, resulting in a good and compact introduction which does more than just scratch the surface. Although I would always suggest a handful of complementary books, if I had to recommend just one introductory book on cosmology for those who want a bit more detail and a bit less of what they have probably heard before, this would be it. — PHILLIP HELBIG.

References

(1) P. Helbig, *The Observatory*, **136**, 96, 2016.
(2) P. Helbig, *The Observatory*, **135**, 234, 2015.
(3) E. R. Harrison, *Cosmology: The Science of the Universe, 2nd Edition* (CUP), 2000.
(4) H. Bondi, *Cosmology, 2nd Edition* (CUP), 1960.
(5) P. Helbig, *The Observatory*, **133**, 233, 2013.
(6) S. M. Carroll, in S. C. Wolff & T. R. Lauer (eds.), *Observing Dark Energy* (ASP Conference Series, Vol. 339), 2005, p. 4.

Human Missions to Mars: Enabling Technologies for Exploring the Red Planet, 2nd Edition, by D. Rapp (Springer, Heidelberg), 2016. Pp. 582, 24 × 16 cm. Price £153/$179 (hardbound; ISBN 978 3 319 22248 6).

I missed the first edition of this book (2008), nor was it reviewed in *The Observatory*. This second edition of Donald Rapp's book is an impressive, comprehensive, and well-organized work that begins by exploring the history of the planning of human flights to Mars. Subsequent chapters document the choice of propellants and orbital geometry, issues of radiation levels, recycling, habitat design, and the use of indigenous resources. (He explains why the use of resources indigenous to the Moon would essentially be a non-starter.) Chapter 7 brings the main part of the book to a close with the provocative but nicely justified title: 'Why the NASA approach will likely fail to send humans to Mars for many decades to come.' Rapp explains in his Preface: "This book represents the first sceptical analysis of human missions to Mars, and is offered as a counterbalance to the optimism so widely promulgated by NASA, *The Mars Society*, and others." But he also explains that sceptics identify the barriers to success, and the technical developments needed to fulfil the dream.

Getting a manned crew to Mars and back remains a formidable challenge compared with the relatively short hop to the Moon. If the questions of distance, time, payload, and propellant can be solved, then questions of long-term radiation levels, crew fitness, and several serious psychological issues remain. The author shows that there have been many formal studies conducted over some 65 years, but that as of 2015 NASA still possesses no viable manned Mars-mission plan. Some of the technologies likely to be required remain rooted in the development stage. Moreover, the likely mission budget in excess of $100 billion so far outweighs the annual budget that the launch of a manned mission any time soon looks far-fetched. As the author explains, NASA policy has undergone so many changes that no plan with guaranteed long-term funding can be made with any certainty: thus the idea of a return to the Moon has been dropped in favour of continuing the *International Space Station*. It may be that the only way the goal could really be achieved would be to underscore the programme with the same political will that took the USA to the Moon in the 1960s.

There are three very comprehensive Appendices, which derive full statistical data for collecting solar energy on the Moon and Mars, and the quantities of water to be found on Mars at different localities (including a discussion of the topical 'slope lineae'). A Glossary completes the text.

A human mission to Mars could accomplish much. Not only would there be a flexible approach to on-the-spot Mars science, the demonstration that we could live for extended periods upon another planet, and the prospect of technological advances, but there would also be its inspirational impact upon all of humankind. I do hope one day to be able to witness the first human mission to the Red Planet, but frustratingly the concept continues to recede at a rate approaching one year per year. In the meantime we have this worthy book to read. — RICHARD MCKIM.

Here and There

ESCAPED A BIG BOUNCE?

… established that galaxies as old as the universe — perhaps even older — existed … — *A&G*, **57**, 1.15, 2016.

SPACE–TIME DISTORTION

Created by the collision of two black holes a billion light years away and 1·3 billion years ago, … — *The Times*, 2016 February 12, p. 33.

BOTTOM DROPS OUT OF THE IRIDIUM MARKET

The most heard explanation … for the dinosaurs' demise is an asteroid impact which left … a worldwide 30-centimetre-thick layer of iridium … — *New Scientist*, 2016 April 2, p. 8.