THE OBSERVATORY

Vol. 135 2015 OCTOBER No. 1248

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2015 March 13 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Hello and welcome. Our first speaker is the winner of the Women of the Future science award; I would say she is very much a scientist of the present as well: Dr. Karen Masters of Portsmouth University. She will speak to us today on 'Galaxy Zoo science results: revealing the impact of galactic bars'.

Dr. Karen Masters. It's a pleasure to have been invited to give this talk about science results from Galaxy Zoo and, in particular, it has been nice, in the work I did preparing for this talk, to reflect on how far we've come as a project since I gave a similar talk at the RAS in 2011 March.

I don't need to introduce Galaxy Zoo much to this audience: Galaxy Zoo (www.galaxyzoo.org) is now almost eight years old, having been launched in 2007 July. It has been demonstrated to be a very successful method for quantifying visual morphology, and has now run in several phases for several surveys (*e.g.*, public *HST* surveys, UKIDSS) as well as the original Sloan Digital Sky Survey. The success of Galaxy Zoo led to the launch of the Zooniverse in 2010 (www.zooniverse.org), which is now much bigger than Galaxy Zoo and has a huge societal impact.

But I'm here to talk about scientific results from Galaxy Zoo. I will also focus only on results to do with bars in galaxies, as otherwise there's far too much to talk about, with now almost 60 peer-reviewed publications based on Galaxy Zoo classifications. I'll just start by explaining what a galactic bar is: it is simply a linear structure of stars stretching across the disc of a spiral galaxy. This structure reveals special kinds of families of orbits in the motion of the stars in the galaxy.

In Galaxy Zoo we find bars by showing people images of galaxies and asking them a series of questions, including whether they see a bar-like structure. We collect 20–40 independent answers for each question. We then apply a weighting for consistency to remove very inconsistent users, and also correct for observational biasses based on the details of the imaging. This results in something we called the 'debiassed' classification morphologies, which we treat like probabilities that the galaxy has a certain kind of morphology. We quantify the bar morphology by something called *pbar*; if this quantity is high the galaxy definitely has a bar, while if it is low, it very likely does not.

We've compared the bar classifications from Galaxy Zoo with other catalogues, *e.g.*, the work of Preethi Nair who classified 15 000 SDSS galaxies. We agree in the mean very well, and usually where we disagree the problem is not with Galaxy Zoo. Individual classifiers, no matter how expert, are subject to errors, which we can minimize with the Galaxy Zoo technique. We also compare with the bar lengths from the EFIGI catalogue, which reveals that the Galaxy Zoo *pbar* correlates very well with bar length, and so might be able to be used as a proxy for that measure.

I first got interested in bars in galaxies when I was working on the red spirals found in Galaxy Zoo 1, and a summer student working with us on the project noticed that almost all of the red spirals had bars. This was before we had Galaxy Zoo bar classifications, so we did the classifications ourselves, and found that red spirals are four times as likely to host a strong bar as blue spirals. Then the Galaxy Zoo 2 classifications became available and I published the first result from Galaxy Zoo 2, showing how the bar fraction increases from bluer to redder spiral galaxies. This seemed really interesting, and links bars with processes which turn off star formation in disc galaxies. So how could they do that?

Bars enable the transfer of angular momentum across the disc of a galaxy. Inside the radius of the bar, the gas will move inwards and stars move outwards (as the bar lengthens). It means that the central concentration in a barred galaxy will grow, central star formation is sparked, and it's possible that has a role in AGN feeding. It might also use up the cold gas in the galaxy more quickly, by concentrating it in the centre, in a process we have called 'bar quenching'. Outside the radius of the bar, there is a positive transfer of angular momentum, so external cold gas may find it hard to get inside the radius of the bar. Bars are very long-lived in disc galaxies, so even if this process has a small effect at any moment, over a long time-scale it may significantly affect the evolution of the galaxy. That is 'secular' (or slow) evolution.

Cold gas is very important to the dynamics of bars, so with Galaxy Zoo we wanted to test some of the predictions of trends of bar fractions with gas content. We used the ALFALFA survey, which provided neutral-hydrogen measurements in about 2000 galaxies, as well as Galaxy Zoo bar classifications. We found that there are indeed more bars in gas-poor galaxies than gas-rich galaxies. That is also true when the well-known trend between gas content and stellar mass is accounted for — galaxies with less H I than expected have higher bar fractions; while those with more H I gas than expected have lower barfractions. So it really seems that it is the gas content in galaxies which drives the trend of bar fraction with stellar mass.

In some work led by Edmond Cheung, now a postdoc at IPMU, we provide proof of the growth of bulges *via* secular evolution in barred disc galaxies. Edmond plotted the specific star-formation rate of the Galaxy Zoo sample against a variety of measures of the central concentration of galaxies. We find that among star-forming galaxies, barred galaxies have larger central concentrations than those which are unbarred — providing evidence for the growth of bulges by bars.

We have also found evidence that barred spirals are more strongly clustered than non-barred spirals on certain scales as well as being destroyed in close pairs, so there's clearly a role for environment in some of these trends. Finally using 'Galaxy Zoo: *Hubble*' we find that there were fewer bars in disc galaxies at high redshift, but they are still there right out to $z \sim 2$.

We are looking at the most recent results for evidence of AGN feeding by bars. At most 16% of AGN feeding could be driven by bars, a result which is broadly repeated at higher redshift.

I have run out of time to talk about the future plans for Galaxy Zoo, other than to say the future is bright. We have a lot more surveys coming. For example, *Euclid* will provide images at the level of detail we had in the SDSS sample for 1 billion galaxies. And with surveys like MaNGA (part of the next phase of SDSS) we're getting much more detailed looks at the internal structure of galaxies.

Galaxy Zoo data are publicly available at www.data.galaxyzoo.org, and *via* the SDSS CasJobs interface. Finally, I want to end by pointing out that none of this work would have been possible without the efforts of thousands of contributors, many of whom are listed at www.galaxyzoo.org/volunteers.

The President. We have some time for questions.

Mr. M. F. Osmaston. I think you should make an effort to study galaxies in which you are seeing a bar and bulge, because this will tell you a lot more about the dynamics. I maintain that the bar is straight because it's self-rotating about its own axis. That keeps the system dynamically in shape.

Dr. Masters. We will have the dynamics of lots of bars soon. This gives me a chance to mention my slide about moving into the third dimension, and getting lots of spectra across galaxies. We will get dynamics of lots of bars that way.

Mr. Osmaston. You said that bulges might be built by bars, but the bulges are typically old. You said the gas moves in but the stars move out along the bar. Is the idea then that the gas forms stars in the centre and that's what forms the bulge?

Dr. Masters. I was a bit loose in my use of the word 'bulge'. There are two kinds of bulges: classical bulges, which are old, and pseudo-bulges, which typically rotate, are not as de-Vaucouleurs-like in their shape, and are younger. It's those pseudo-bulges that bars would be building.

Professor P. van Dokkum. Which way round does the secular evolution work? Are the bars preventing stars from forming or have all the stars formed in a starburst, used up the gas and those are the galaxies which then develop bars? Are the stars forming the bars or are the bars stopping the stars?

Dr. Masters. I'm beginning to dislike bimodalities in astronomy and these 'either-or' questions! I suspect that the answer is both.

Rev. G. Barber. How is the popularity of Galaxy Zoo going over time?

Dr. Masters. The Zooniverse was set up with the idea of becoming media independent: we could build a community of people who like doing citizen science. Then it doesn't matter if you get on Stargazing Live or not, because you have the community. There's good evidence that is working. The Zooniverse user numbers grow linearly, which is typical of social media. Every time a new project is launched you get new users to all projects. Galaxy Zoo maintains its position as one of the most popular projects.

The President. Let's thank Karen again for a great talk. [Applause.]

Our next speaker is Dr. Andrew Siemion from Berkeley, on 'Searching for extraterrestrial intelligence with the next generation of radio telescopes'.

Dr. A. Siemion. [No summary of this talk was available at the time of going to press.]

The President. An exciting topic, do we have any questions?

Dr. P. Wheat. If you look at our current military communications, a lot of them are spread-spectrum and encrypted. At some point we would probably also use quantum. Do you have any thoughts as to what the window of opportunity is, because we've moved quite fast through the basic communications we have been using into what will at some point become undetectable against the background noise?

Dr. Siemion. You're absolutely right. If a very advanced civilization were intentionally concealing itself, it would be very difficult for us to detect its communications. There are a couple of aspects to that. First, we are using auto-correlation and other kinds of more detailed signal-detection techniques that would allow us to be sensitive to broad-band communication. Second, at least 50% of people in the SETI community believe that the first signal we intercept will be intentional. From that perspective, the signalling civilization would want their communication to be detectable, and intentionally make it look different from the astrophysical background. I think there is a lot of overthinking and anthropocentrism in that argument, but nevertheless it is worth considering. I think that the first signal we detect will be something pathological, something we are not expecting — some accident or an artefact of technological activity that the creating civilization didn't intend to be detectable.

Rev. G. Barber. There is a natural desire not to make any false positives. That of course means you enhance the chances of making a false negative. Normally you have the chance to repeat the experiment. But if you just have a sporadic detection, like the 'Wow!' signal, it would be very difficult to work out if you have a real signal or not.

Dr. Siemion. That's true. Concerning the 'Wow!' signal, we do capture a lot more information about the electromagnetic field when we're doing experiments now: we record raw voltage as a function of time from the radio telescopes, so we can do a much more detailed analysis. For example, if we received an intermittent signal, which exhibited some kind of modulation we had never seen before or that no known technology on Earth used, that would make it very interesting. But it certainly is a very big challenge to tell the difference between our own technology and extraterrestrial technology. We do our best but we are very, very, cautious, and I think we have to be because it would be such a profound announcement that we would not want to get it wrong.

The President. Let's thank our speaker again. [Applause.]

I am delighted to introduce this year's Eddington lecture, given by Professor Pieter van Dokkum of Yale University: 'Ghostly galaxies: exploring the Universe with the *Dragonfly* Telescope'.

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

The President. It's fascinating to see such cutting-edge science coming from a relatively low-cost system in this era of *ELT* and *SKA*, so I'm going to take the prerogative of asking the first question: how much money have you spent so far? [Laughter.]

Professor van Dokkum. About $150 000.

The President. Good value, I would say.

Dr. R. C. Smith. Is it possible to improve the resolution from 2·9 pixels?

Professor van Dokkum. Yes, we've thought a lot about what lens to use. There are longer-focal-length lenses with the same aperture, for example a 600-mm lens with a 14-cm aperture; they become slower, so you get less signal per pixel but you get smaller pixels in angular size. We decided that being optically fast was the most important thing. Ideally we'd have a detector with smaller pixels because the site can offer better seeing than 5 arcseconds, and the lenses can do better. If at some point a commercial detector comes out with smaller pixels we'd find that very useful.

Dr. M. Bureau. I was curious about the comparisons you made with the results of other telescopes. You showed the comparison with the *CFHT* image, which is famous for the quality of its imaging. I wasn't sure how typical that image was, so I was wondering if you could compare that to, for example, the *CFHT* Next Generation Virgo Survey. You could compare the objects you're seeing with the same things they're seeing.

Professor van Dokkum. It's complementary. We have been talking with them about the Virgo Survey, and that is exactly what we plan to do if we can get the upgrade done while that survey is still in progress; we will combine efforts. We can detect very large galaxies. If a galaxy is above about 20 arcseconds in effective radius, then we win hugely; we detect it easily with our set-up. Our single 6000-pixel images are clean and very easy to analyse, whereas the *CFHT* mosaics are huge and very difficult to analyse, and made more difficult because of having to deal with chip edges and PSF issues, *etc.*

Dr. Masters. Regarding the design, where you put the telephoto lenses together to increase the number, is there any reason why you couldn't have 30 000 individual telephoto lenses spread across the world and add everything up virtually?

Professor van Dokkum. It would be a little unwieldy because building something that works isn't that trivial. I've made it sound that way but Roberto Abraham is actually a master of making it work. The reason it works is because we have a lot of software and we've done a lot of debugging of the mounts and cameras, and we have excellent support at the site. What you could do, though, is have multiple sites, and not have all these things looking at the same part of the sky — looking at different objects at once or even mapping large areas by having them pointing next to one another.

Rev. Barber. Do you have any handle on the masses of these ultra-diffuse galaxies?

Professor van Dokkum. The colour tells us roughly what the stellar mass is, about $10^7\ M_\odot$. The real question is what's the dark-matter mass? Is it similar to dwarf galaxies of the same stellar mass or to elliptical galaxies of the same size? That's my simplistic way to look at it and it's probably somewhere in between. There's a very rough estimate you can make just from the fact that these galaxies exist in the Coma cluster and don't seem to be in the process of being disrupted. The tidal field of Coma would rip them apart very quickly if they were just made of stars. Based on that argument, they are probably at least 98% dark matter, which is not that unusual for dwarf galaxies, but it's a lower limit, a survival argument. We'd love to measure a velocity dispersion of the stars and that would take about 20 hours with *Keck*.

Dr. G. Q. G. Stanley. With larger telescopes we have active mirrors to improve the seeing, which you can't do with your refractors. Are you stacking images or are you not concerned about this with the field of view you have?

Professor van Dokkum. We spent a lot of effort getting the tracking right and doing the guiding properly. Right now we are Nyquist-sampled with the pixels we have. We are stacking to make a deeper image. Again, if we could have a detector with smaller pixels, that would be good. The sweet spot for *Dragonfly* is the 20-arcsecond régime and there the pixel size doesn't really matter.

The President. Another fascinating afternoon of talks. Let's thank Pieter and all our speakers again. [Applause.] A reminder that there is a drinks reception following the meeting and that the next A&G meeting of the society will be on Friday, 2015 April 10. I hope to see you all there.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2015 April 10 at $16^h\ 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. Hello and welcome. I'd like to begin with the award of the Jackson-Gwilt Medal, which this year is awarded in the field of the history of astronomy. The award is presented to Dr. Allan Chapman. [Applause.]

Dr. Allan Chapman is a renowned historian of astronomy. Through his extensive public lecturing, publications, and television appearances, Allan Chapman has brought astronomical history to new audiences, and in doing so he has raised the profile of the history of astronomy and stimulated historical research.

Allan Chapman's book, *The Victorian Amateur Astronomer: Independent Astronomical Research in Britain 1820–1920* is of particular note and is an essential resource for any researcher of 19th-Century astronomy. The *Victorian Amateur Astronomer* identifies and honours the 'grand amateurs', a term he used to describe a group of people, seemingly unique to Britain, who made major discoveries from privately-funded observatories or who popularized astronomy among the masses. Allan Chapman's in-depth research documents the work and achievements of the often self-educated assistant astronomers, many of whom were previously unknown and who did so much of the groundwork that led to published results. The era spanned by this book is of great importance to British astronomy and it puts into social context the foundation of both the Royal Astronomical Society and the British Astronomical Association. Allan Chapman's work for this publication resulted in the foundation of the Society of the History of Astronomy, a society which seeks to understand the history of astronomy and the important contributions by lesser-known figures in history as well as those more famous. For these reasons Dr. Chapman is awarded the Jackson-Gwilt Medal. [Applause.]

We now move on to today's excellent programme. Our first speaker is Professor Michele Dougherty from Imperial College London. She is the group-achievement award winner for Geophysics. She will be speaking on: 'Scientific highlights from the *Cassini* magnetometer at Saturn'.

Professor Michele Dougherty. I want to talk about two instruments on the *Cassini* spacecraft — the flux-gate magnetometer (built at IC) and the vector/scalar helium magnetometer (JPL) which together form the *Dual Technique Magnetometer* (*MAG*). I took over from David Southwood 15 years ago and we have had a large team of about 70 people over the years from the UK, USA, Germany, and Hungary who have been collaborating on the project.

There are eleven scientific instruments on *Cassini*, divided into four main groups, and in particular I want to talk about what the *MAG* science team intends to do at Saturn in 2017 when the mission is in a close polar orbit. Although *Cassini* has had 100 fly-bys of Titan so far with another 20 scheduled, today I will talk about Enceladus, and the environment around Saturn.

The *Cassini* imaging camera shows the surface of Enceladus to be relatively young with only a few craters and cracks apparent, and it can be contrasted to Mimas which has many craters. The implication from these observations is that Enceladus is being resurfaced somehow. The *Voyager* spacecraft in the 1970s showed that the surface was composed almost entirely of water-ice, as is the E-ring inside the orbit of Enceladus, so could that moon have been the source of material for the E-ring?

Three fly-bys that we did in 2005 are the earliest observations that convinced us that Enceladus has an atmosphere. The first fly-by passed 1200 km from the surface of Enceladus, which has a diameter of 500 km. We found that the azimuthal component of the magnetic-field data has a wave-like component which varies with a period close to the planetary rotation rate. If we look at the radial and azimuthal variations we see large perturbations in the field which were due to the fact that the spacecraft was flying past Enceladus. We need to subtract these variations so that we can measure the magnetic field of Enceladus itself. It seems to tell us that the co-rotating plasma flow from Saturn sees an obstacle to its flow which is much larger than Enceladus itself, and the field lines appear to be draped around the moon, due, we believed, to a large quantity of water ions close to Enceladus. If Enceladus was a dead body the field lines of Saturn would not see it, but instead as they moved towards Enceladus they were draped upstream of the moon. Another fly-by a month later confirmed these observations, *i.e.*, that there is a diffuse extended atmosphere surrounding Enceladus. Since the gravity at Enceladus is low we assume that there is material which is being generated all the time. The third fly-by passed just 173 km above the surface, and fortuitously we were flying over the South Pole. We found that the atmosphere was focussed at the South Pole and the bending of the field lines coincided with a plume of water vapour being emitted from the South Pole.

Imaging done on the same pass showed cracks which the imaging team called 'tiger stripes'. In the data from the *Composite IR Spectrometer* (*CIRS*) we expected to see a very cold surface with the warmest part (80 K) being at the equator, but instead we saw a hotspot at the south pole (91 K) which coincided with one of the cracks, indicating that heat was leaking out from the centre of Enceladus. The *Visible and IR Mapping Spectrometer* (*VIMS*) created a mosaic map of the surface from three separate fly-bys and found an abundance of hydrocarbons which correlated with the largest grain sizes and lying along the cracks. Looking back towards Enceladus we could see the enormous extent of the water-vapour plume at the South Pole. The *Ion and Neutral Mass Spectrometer* (*INMS*) can resolve complex chemicals including organics, and when it flew through the plume it found methane, water vapour, simple organic compounds, carbon monoxide, carbon dioxide, and complex organics, and on a later flyby we also found ammonia. That is an indicator of the presence of liquid water as it acts as an anti-freeze and helps to keep water liquid down to 176 K. These observations have led to demands for many more fly-bys and an Enceladus-orbiting mission, but it requires a lot of fuel to attain orbit because of the low gravity.

The primary target for the end of the mission is to ascertain whether there is an asymmetric component of the planetary magnetic field at Saturn. One of the current surprises is that the planet's rotation axis and the dipole axis are almost exactly coincident. Planetary-dynamo theory suggests that you cannot keep a dipole self-sustained unless you have a dipole tilt. This may be because of oscillations in the magnetic field at the planetary period. We have 35 years of data on the magnetic field and also radio-wave observations over the same period. We can see that there are two oscillatory-system periods present — one associated with the North Pole region and the other with the South Pole region, varying between 10·6 and 10·8 hours, changing over time, and varying with the seasons. The cycles close up near equinox but then afterwards they separate again. We need to model them really well so that we can subtract the effects at the end of the mission.

As observed during Saturn orbit insertion, the internal magnetic field has extreme axi-symmetry, which seems to contradict Cowling's anti-dynamo theory, but as the spacecraft was travelling so quickly we could not see any longitude change in the axial symmetry. We can clearly see that the azimuthal component of Saturn's magnetic field is about two orders of magnitude smaller than the radial and tangential components, and we ran the data through a spherical harmonic analysis. We found that the rotation periods ranged between $10^h\ 30^m$ and $10^h\ 50^m$ allowing us to determine the size of the dipole tilt. We want to see what the magnetic field looks like at varying depths in Saturn's atmosphere. What is clear is that the planet's magnetic field is very different from that of the Earth, both at the poles and the equator.

The end of the mission comes in 2017, and we need to model what is going on in the planetary period oscillations. In the second half of 2016 and the first nine months of 2017 there are two areas of research. We want to fly above the F-ring and check how the spokes relate to the rotation period. The most exciting prospects are the proximal orbits — just 3000 km above the planet's atmosphere — which will eventually lead into the impacting orbit. There are two favoured hypotheses. First, has the internal dynamo stopped operating? Second is an idea from Dave Stevenson who suggests non-axisymmetric components are being filtered out by a differential-rotation layer in the atmosphere, *i.e.*, there is a dynamo but it is being shielded by the differentially-rotating atmospheric layers. Shielding is more effective for higher-order moments and possibly more effective at high latitudes. We also plan to compare these observations with those taken around Jupiter by the *Juno* mission. [Applause.]

The President. Nice, exciting results. We have some time for a couple of questions.

Professor D. Lynden-Bell. Does Enceladus have a field of its own or is it just that it has got a wind coming off it?

Professor Dougherty. We haven't been able to measure a magnetic field; certainly not a dynamo field. There is a thought that there might be some induced currents flowing in an ocean but they would be really hard to measure.

Mr. S. Young. I wondered what the risks might be when you come closer to Saturn.

Professor Dougherty. Yes, it's pretty risky. It's also complicated for us because the vector/scalar helium magnetometer is no longer working so we are left with only one instrument. That means we can't calibrate the instrument. The only way we can now calibrate it is to roll the spacecraft, which we have just persuaded the *Cassini* project to allow us to do during closest approach. We can then calibrate the instrument in high fields. There are discussions about the density of the atmosphere of Saturn, because if it's denser than we think when we get closer in, we could lose control of the spacecraft. Therefore, they are talking about the last five orbits being marginal.

Mr. H. Regnart. What is reckoned to be the source of heat beneath the tiger stripes?

Professor Dougherty. It depends which modelling team you talk to, but I think the easiest explanation is that the orbit of Enceladus used to be more elliptical around Saturn than it is now, so tidal heating has kept the interior warm.

Professor Lord Rees. There was an article which claimed to detect a gravity anomaly in Enceladus, due to an ocean. Do you believe that and is that the same part of the moon as where you saw the flare?

Professor Dougherty. I do believe it. They were very close to their observational limits but they were very careful in the work they did. It's an ocean at the south pole.

The President. Thanks very much. Now we will be hearing from the Fowler Award winner for Astronomy, Professor Joanna Dunkley of Oxford University: 'Observing the early Universe'.

Professor Joanna Dunkley. Much of my research focusses on the Cosmic Microwave Background, and how we have been able to use it to understand the earliest moments in the Universe and its later evolution.

The CMB is light that has travelled since the Universe became neutral, 380 000 years after the Big Bang. It was first detected 50 years ago, in 1965, and was later mapped out over the whole sky by NASA's *COBE* satellite. A map made by *COBE* in the 1990s of the average temperature of the CMB looks perfectly uniform over the whole sky, telling us that at that time the Universe was remarkably featureless. But if we examine the light closely, we do find irregularities in the light's temperature that follow features in the underlying density of matter in the Universe. In the last 20 years we have been able to look in greater detail at those features, using NASA's *WMAP* satellite and more recently the ESA *Planck* satellite, as well as a set of ground- and balloon-based experiments. Many astronomers in the UK are part of the *Planck* collaboration, and have made significant contributions to both instrumentation and analysis.

The *Planck* satellite was launched in 2009 and has made a new map of the CMB temperature anisotropy, as well as its polarization. The map shows in detail the anisotropies, typically one part in 100 000, measured with high sensitivity and a resolution of a few arcminutes. The underlying density fluctuations are the seeds of cosmic structure. Regions that were slightly denser than average at recombination evolved over millions of years, gravity increasing the density of those regions until the first stars and galaxies could form. It is extremely helpful to be able to capture the behaviour of the fluctuations at an early time when they were still linear. With current numerical codes we are able to evolve the Universe from time zero to 380 000 years, and predict the outcome. We compare these theoretical predictions to what we see in the data. In detail, we do this by examining and predicting the angular power spectrum of the CMB fluctuations, which captures almost all of the information in the anisotropy. The power spectrum acts as a cosmological fingerprint, a unique signature that depends on the contents, geometry, and initial conditions of the Universe.

By matching the spectrum measured by *Planck* to a suite of theoretical models, we can estimate rather accurately the make-up of the Universe at the epoch when the CMB formed. At that time, just over 10% of the energy density was in normal atoms, 25% was photons and neutrinos, and about 60% was dark matter. Dark matter and baryonic matter can be distinguished physically *via* the CMB power spectrum, because before recombination the baryons and photons were tightly coupled, sourcing sound waves in the plasma. In contrast, dark matter is collisionless and so evolved differently, gravitationally collapsing. This dominant dark-matter component now forms the backbone of the cosmic web. We do not yet know what it is fundamentally, but large numerical simulations can now track its expected evolution on a wide range of scales from the time of recombination through to today, revealing haloes, filaments, and voids, with baryonic galaxies and clusters populating the densest dark-matter regions.

By examining the angular power spectrum, we also estimate the initial conditions, quantifying the nature of the primordial fluctuations. This provides a window on the earliest moments in the Universe, and allows us to test the currently favoured model, inflation. Inflation is an effective field theory, positing that an initial exponential expansion of the Universe was driven by the potential of some scalar inflaton field. We do not yet know if inflation is the correct scenario, but it predicts primordial fluctuations that are highly consistent with the observations from *Planck* and *WMAP*. Imprinted as quantum fluctuations that became macroscopic during the inflationary expansion, they are expected to be Gaussian, adiabatic, super-horizon, and almost scale-invariant. The *WMAP* satellite data were consistent with all of those properties, and *Planck* has now significantly tightened constraints.

A key remaining prediction of inflation is the generation of stochastic gravitational waves, also imprinted as quantum fluctuations during inflation. This is now a major focus of CMB experiments, as these gravitational waves should have polarized the CMB light due to their quadrupolar effect on space–time, resulting in polarized light being emitted through Thompson scattering of photons off free electrons during the recombination process. Part of this polarized signal should have a divergence-free 'B-mode' pattern not predicted by other early-universe processes. The size of the expected signal depends on the energy scale of inflation, which is theoretically unknown. There is therefore a chance we will be able to detect it, but also a possibility that the signal will be too small ever to see, even if inflation is correct.

In 2014 a detection of a B-mode polarized CMB signal was made by the *BICEP2* experiment at the South Pole. It was initially interpreted as an inflationary gravitational-wave signal, but the community soon pointed out that it could not be distinguished from Galactic emission: polarized thermal emission from dust grains aligned with the Galactic magnetic field. Separating primordial from Galactic and extragalactic light has long been a challenge in CMB science. Measurements are typically made at multiple wavelengths to extract the blackbody CMB from the non-blackbody emission mechanisms including synchrotron, Bremsstrahlung, and thermal dust. And, in fact, by combining the *BICEP2* data at 150 GHz with measurements at shorter wavelength from *Planck* at 353 GHz, an improved estimate of the primordial signal was made in 2015 through a joint analysis by our two teams, resulting in no evidence yet for gravitational waves. Interestingly, the new upper limit now disfavours arguably some of the simplest models for inflation, including a quadratic potential.

These new polarization measurements mark the start of a decade or more of anticipated progress to reach ever-smaller B-mode signals in search of evidence for inflation, and if found, its characterization. The technology is improving rapidly, with new ground-based and balloon experiments mapping the CMB polarization with exquisite precision from sites in Chile and the South Pole, and from Antarctic balloons. On that front, my group in Oxford is preparing for the analysis of *Advanced ACTPol*, an experiment in Chile that builds on two earlier experiments using the same telescope. It will map half the sky at five wavelengths from 2016–19, targeting a detection of the elusive gravitational waves. At the same time we aim to measure the mass of neutrino particles *via* their effect on the clustering of matter. The matter, both dark and baryonic, gravitationally lenses the CMB light, providing us with a new view of the invisible Universe.

The President. I hardly dare ask the obvious question: do you need another space mission?

Professor Dunkley. I don't think we need one yet. I think it's a big shame that the UK decided to pull out of any of the ground-based CMB experiments. All of the ground-based CMB work happens in the US; none of it happens in Europe. I work on it because we work on a US experiment. We can make a huge amount of progress from the ground.

Professor E. Priest. So you don't sound very confident that the current extension of *BICEP* will solve the problem?

Professor Dunkley. I think that, while it will help, it will take more than another year or two to get there. If you have only three wavelengths, as the *BICEP* currently has, you get synchrotron emission, dust emission, and the CMB. If you have only one wavelength per component, they have to be very simple components. They may be, but it's not trustworthy. We may get interesting upper limits, but to make a detection you need more wavelengths than that and you need to look in other regions of the sky to make sure it's isotropic. Progress will be made but if we're looking for a watertight detection then we need a few years after that.

Professor Priest. They're hoping to double the sensitivity.

Professor Dunkley. Yes, but you need more than that. It's not enough, you need multiple wavelengths.

Dr. S. Jheeta. I don't know how you differentiate between dark matter and dark energy?

Professor Dunkley. They do actually behave quite differently. We write down the dark-matter equations of motion as something that behaves gravitationally and clusters but doesn't interact electromagnetically. With dark energy, it's a cosmological constant, a vacuum energy. So, when you write it down it's just a background energy that has an equation of state equal to –1. Dust would be zero. They are quite different.

The President. Let's thank Joanna again. [Applause.] On to our next speaker, Dr. Josh Emery of Tennessee University: 'Trojan asteroids and the evolution of the Solar System'.

Dr. J. Emery. Trojan asteroids occupy the L4 and L5 Lagrangian points which precede and follow Jupiter by 60°. Along with the main asteroid belt and the Kuiper Belt they form one of three islands of stability in the Solar System. There are thought to be about a million objects in the main belt and another million in the Trojans. The Trojans orbit at 5·2 AU, beyond the main asteroid belt, have probably been stable for more than 4 billion years, and are thought never to have been heated significantly since the epoch of formation.

We want to know whereabouts in the early Solar System the Trojans formed — was it here, at 5·2 AU, or were they transported from somewhere else? Since they are primitive, if we find out more about them and their compositions then this will give us a clue about conditions and composition in the solar nebula in which they formed. My own approach is to look at the composition and surface properties of the Trojan asteroids.

There are two views on the dynamical evolution of the Solar System. First, the solar nebula was quiescent and material accreted into asteroids and larger planetesimals with little radioactive mixing. We expect a gradient in composition as we move away from the Sun, and we see different spectral types at different distances from the Sun within the main asteroid belt. First silicates condense at high temperature, then silicon at slightly lower temperatures, and at 5 AU, where there are lower temperatures still, we find carbon-bearing material. If that is right, and the Trojans formed there, then they would have organics and water-ice only — not carbon-dioxide ice or methane ice.

We know that the early Solar System was not quiescent, and the evidence comes from the orbits of the Kuiper Belt objects. For those trapped in an exterior resonance with Neptune, some were scattered into high-eccentricity orbits with Neptune and some into low-eccentricity orbits near 42 AU. The dynamicists explain this by saying Uranus and Neptune migrated relatively early in the Solar System history.

The Nice model for the early Solar System, which is ten years old, puts all of the outer planets closer to the Sun with the original belt of material further out. As time went on Uranus and Neptune scattered these bodies inward and the planets then moved outwards. When the bodies got to the orbit of Jupiter they were scattered out and Jupiter moved inwards a little bit. In this model Saturn and Jupiter migrated away from each other until they reached a mutual resonance and they increased the eccentricities of Uranus and Neptune towards the small bodies and scattered them. For the early Solar System this model predicts several things. Anything trapped in the Trojan swarm before resonance was destabilized and got moved, and when Saturn and Jupiter migrated out of resonance, anything left in the Lagrangian regions stayed there. What was left became Kuiper Belt objects. One implication of the Nice model is that the Trojans formed at 30 AU and not 5 AU — they would have had a lot of organics, water-ice, and volatile ices. The two models predict two different compositions for the Trojan asteroids.

I would like to turn to observations of the Trojans themselves. In the case of the size distribution, the larger objects are primordial whereas the smaller ones will have been collisionally ground down. Evidence from light-curves and shapes indicate bodies that are 60–80 km in diameter are primordial, and the smaller bodies are fragments of larger objects. The Trojans have low albedos (5 to 7%), which is darker than coal (10%). These dark objects are also faint, and spectra in the visible are uniformly featureless, whereas the main-belt asteroids show absorption features at longer wavelengths. In the 1980s the interpretation was that the organics consisted of fine-grained amorphous silicates maybe with some space-weather interaction. Ice, organics, hydrated silicates, and crystalline silicates all feature in the IR — particularly water-ice and organics. We have the potential to see the materials on the surface of the Trojans.

I have measured the infrared spectra of about 100 objects out to 2·5 μm. They have no absorption features but we do find that there are two distinct spectral groups, which possibly points to two different compositions. When we look at the size distribution of each group, the larger sizes have the same spectral slope whereas the smaller ones have different slopes and the less-red ones are steeper than the redder groups. Is this a clue to the internal strengths of the objects? If we go to 3 μm we should be able to see features due to water-ice, silicates, and organics but we find nothing from observations of about a dozen objects. This allows us to put constraints on the amount of these materials, but clearly Trojans are not organically rich. In the mid-IR we get spectral features due to fine-grained silicates, which tells us about the surface structure, and adaptive-optics observations of two binary asteroids give us a direct measure of the density of the objects. Patroclus is 1 gm per cc — so it is consistent with a lot of water-ice, whereas Hector is 2·4 gm per cc and therefore rocky.

This indicates something about the regions in which these objects originated. They were not rich in organics, the silicates were mostly amorphous and not crystalline, and we don't see any hydrated silicates, which points to insufficient heating or insufficient ice in the bodies on formation.

Two scenarios which I put forward are that the Kuiper Belt originating group passed inward and the less-red group maybe formed in the 5-AU area and survived there (Nice model).

We still do not know if water-ice still is in the interiors. We'd like to understand the organics better and although we can do spectrometry between 2·8 and 4 μm from the ground it would be much better using the *JWST*. I believe that the mid-IR offers a lot of hope for doing detailed mineralogical analysis. The Trojan-asteroid missions have ranked highly in the last two NASA Decadal Surveys.

In conclusion, the Trojan asteroids form a significant population about which very little is known but which, we believe, could constrain the initial conditions in the solar nebula between 5 and 30 AU and the dynamical evolution of the Solar System. A spacecraft mission to the Trojan asteroids to sample surfaces and interiors is necessary.

The President. A fascinating talk. Any questions?

Rev. G. Barber. How do the Trojans relate to Jupiter? How are they captured around the Lagrangian points?

Dr. Emery. There are two different possibilities depending on which of the origin scenarios is right. If you imagine Trojan asteroids as bodies that grew around 5 AU, they can be trapped in the Lagrange regions, then either by gas drag slowing them down enough to get them captured, or as Jupiter is growing, that can trap them. It turns out that some of the orbital characteristics of Trojans aren't consistent with either of those. They have inclinations that are higher than is easy to replicate through those mechanisms. The other option is that the Lagrange points become stable when Jupiter and Saturn leave their mean-motion resonance, so whatever happens to be around at that time gets trapped. So when Jupiter and Saturn are in resonance with one another, Saturn is perturbing the orbits and making them unstable. When they leave that resonance they become stable so that whatever is there just stays there.

The President. Time is pressing, I'm afraid, so we'll have to move on. Finally today we will be hearing from Dr. Geraint Harker of UCL: 'Astronomy from the Moon'.

Dr. G. Harker. The Moon has long been considered as a possible site for astronomical instruments, but I have to admit to something of a bias in terms of the sort of observations I would like to see taking place from the lunar surface: low-frequency radio astronomy from the far side. The benefits of observing from the Moon are, I will argue, even more pronounced in the low-frequency radio than in other wavebands. Moreover, some of the most exciting frontiers in astronomy will be opened up by observations below about 100 MHz, especially if they extend down to frequencies of a few MHz. Finally, the deployment of a low-frequency radio interferometer would leverage developments in, for example, telerobotics, which would be valuable not just for the exploration of the lunar far side, but other Solar System bodies as well.

Early concepts for lunar observatories did not concentrate on the radio, however. For example, in Arthur C. Clarke's 1951 short story (and 1955 novel) *Earthlight* — set in 2015, incidentally — the quartz mirror of the 1000-inch reflector at the lunar observatory was described as being "the human race's most valuable single possession". Naturally, Clarke had even considered some research problems it might tackle, including studying the light-curves of variable stars in Andromeda.

In practice, lunar observatories have been somewhat more modest. In 1972, *Apollo 16* deployed a small telescope with a far-UV (50–160 nm) camera, with a field of view of 20° and a resolution of 2–4 arcmin. Though sensitive only to stars brighter than 11th magnitude in *V*, it obtained the first far-UV atlas of the Large Magellanic Cloud, as well as of several fields at various Galactic latitudes. It also produced the first images of the Earth in the far UV, capturing the fluorescence of oxygen atoms on the day side, and the so-called far-UV equatorial arcs on the night side.

Moving closer to the present day, the *Chang'e 3* lander, which reached the lunar surface in late 2013, is equipped with a 150-mm near-UV telescope sensitive down to 13th magnitude. It benefits, of course, from the low levels of opacity and scattered light from the lunar exosphere. Nonetheless, the diffuse background present in its images has allowed for the study of OH radicals and atomic species in the exosphere. For astronomical observations, the slow rotation of the Moon allows for long, uninterrupted views of individual targets.

There are other promising areas outside the UV: in the thermal infrared, an observatory would benefit from passive cooling, and the lack of an atmosphere. Optical or infrared interferometry, in addition, would make use of the very stable observing platform offered by the lunar surface, and the lack of existing man-made structures means that telescopes can be placed in optimal locations. This emptiness and stability — and the long observing times made possible by a lunar location — could also benefit high-energy (X-ray or gamma-ray) detectors.

I will, however, concentrate on the low-frequency radio. In this case, the far side of the Moon affords especially favourable conditions. Not only do we escape the influence of the Earth's ionosphere (a layer of the atmosphere containing plasma which can refract, absorb, and emit radio waves, and even become totally opaque below the plasma frequency), but we also avoid anthropogenic radio-frequency interference (RFI), a significant headache for earthbound telescopes. No other location in the inner Solar System affords a site that is permanently as well shielded from RFI as the lunar far side.

One of the most exciting targets for low-frequency radio observations is the highly redshifted 21-cm (1420 MHz) line of hydrogen. This is a forbidden transition between the two hyperfine levels of the ground state of a hydrogen atom, with the proportion of atoms in each state being parametrized by a 'spin temperature'. Depending on the spin temperature, a patch of neutral hydrogen may be seen in emission, or in absorption with the cosmic microwave background (CMB) as a backlight. The low optical depth means that such absorption or emission can be detected to very high redshift: in particular, we can use it to study the Universe at redshift $z > 6$ (the first billion years), before the re-ionization of the Universe, the last major phase transition of the intergalactic medium. Higher redshifts (earlier times) correspond to the cosmic dawn — when the first sources of radiation formed and started influencing their surroundings — and the 'Dark Ages', before even those first sources.

At present, we only have theory to inform us about the Universe at such early times: interpolating between observations of the CMB at ~ 380 000 years after the Big Bang, and sparse observations in the infrared or optical (such as the *Hubble* Ultra-Deep Field) reaching back to perhaps the 0·5 Gyr mark. Theory leads us to expect the first sources at $z \sim 35$ (40 MHz for the 21-cm line), and the first significant sources of X-rays, which heat the IGM, at $z \sim 20$ (70 MHz, or around 150 Myr after the Big Bang). The timings, and even the basic picture, are all somewhat uncertain, however.

The spin temperature of the 21-cm line (and therefore the strength of its emission or absorption relative to the CMB at a given redshift) can be coupled to the kinetic temperature of the neutral hydrogen *via* Lyman-α radiation (the Wouthuysen–Field effect). Therefore it acts as a probe of the radiation field, and as a thermometer for the neutral hydrogen, which can be heated by X-rays from, for example, X-ray binaries or active galactic nuclei. The strength of the 21-cm line also depends on the total amount of hydrogen present, and therefore on its density and ionization state.

The simplest observations one could think of making at this epoch would be of the mean 21-cm signal averaged over the entire sky (or a large enough area of sky for it to constitute a representative volume of the Universe) as a function of redshift. Several groups are attempting such observations at < 200 MHz, but they are hampered by, amongst other things, RFI and the ionosphere. The *Dark Ages Radio Explorer* (*DARE*), led by Jack Burns at the University of Colorado, is a proposed NASA Small Explorer mission that would avoid these problems by taking data at 40–120 MHz while over the far side of the Moon in a low lunar orbit. The far side was demonstrated to be radio-quiet by the second *Radio Astronomy Explorer* satellite (*RAE-2*) in 1973.

The Earth's ionosphere absorbs ~ 1% of incoming radio signals at 100 MHz, and also produces emission from a population of hot electrons (at temperatures of order 1000 K). It also causes refraction: for sky-averaged 21-cm observations we can consider it to act as a lens, focussing emission so that it appears to come from closer to the zenith, and enters the antenna from a direction to which it has more sensitivity. Because the electron content of the ionosphere is time-variable (as measured by GPS sensors, and as modelled by, for example, the International Reference Ionospheric model), this acts similarly to a time-variable gain, modulating the sky signal. These time-variable effects can easily amount to several kelvin, whereas the cosmological 21-cm signal reaches at most ~ 100 mK. They are also frequency-dependent, generally scaling as the square of the observing wavelength.

There are further challenges that face even a mission such as *DARE*. Synchrotron and free–free radiation from the Milky Way and external galaxies is more intense than the redshifted 21-cm signal by several orders of magnitude, even in the quietest parts of the sky. We hope to use its spectral smoothness and spatial variations, which contrast with the spectral features and spatial uniformity of the 21-cm signal, to separate the astrophysical 'foregrounds' from the signal. These strong foregrounds still affect the thermal noise in the observations *via* the radiometer equation, but 1000 hours of integration from lunar orbit, which can be obtained over the course of a two-year mission, will provide sufficient sensitivity to distinguish between models of early star formation.

To observe the true Dark Ages would require observations below 40 MHz, which are feasible only from the Moon since at low-enough frequencies the ionosphere becomes opaque. They would require a larger antenna, which might best be deployed on the lunar surface. The lack of astrophysical objects at the corresponding high redshifts ($z \sim 100$) implies that we can predict the 21-cm absorption signal from cosmology alone, assuming no exotic physics is required. Turning this argument on its head, heating from, *e.g.*, the annihilation of dark-matter particles would wipe out the absorption signal, making such observations a probe of high-energy physics as well as cosmology.

The next step in ambition is to conduct radio interferometry from the lunar surface. Current interferometers, such as *LOFAR* and the *MWA*, and upcoming ones, such as *HERA* and *SKA*, will study the spatial fluctuations of the redshifted 21-cm signal, as well as their evolution in time. They will probe the epoch of re-ionization and the cosmic dawn. To reach back into the Dark Ages requires a lunar interferometer, however, a fact recognized even in NASA's 2013 Astrophysics Roadmap, in the form of a vision for the *Cosmic Dawn Mapper*. The constraints on cosmology and fundamental physics from such an instrument could exceed even those provided by CMB observations.

The scientific case for such an instrument would certainly extend beyond this: it could detect transient emission from extra-solar planets, if they produce bursts similar to Jupiter's; study ultra-high-energy particles or meteor impacts *via* the radio emission they produce as they interact with the lunar regolith; and conduct surveys in previously inaccessible frequency régimes.

An advantage of low-frequency radio is the simplicity of the antennae: for a far-side array, the best-studied concept is that of antennae printed onto a polyimide film, which can then be unrolled from a rover to deploy the telescope. The rover could be controlled remotely, for example, by astronauts in orbit. This was tested recently using a rover at the NASA Ames Roverscape and an astronaut on the *ISS*. For a far-side array, a capsule at the Earth–Moon L2 Lagrangian point would allow astronauts to communicate with the Earth and a far-side rover simultaneously, without imposing a large time lag. This would also facilitate other scientific experiments on the lunar far side, for example a sample-return mission from the South Pole–Aitken basin. Moreover, a similar concept could be used for crewed missions (with a robotic component) to, *e.g.*, Mars or an asteroid. Building a lunar far-side array therefore aligns the interests of astrophysics with those of planetary science and exploration.

The President. Thank you. I'm afraid we are out of time for questions so you'll have to reserve those for the drinks reception which follows immediately in the RAS Library. Finally, I give notice that the next A&G monthly meeting will be on Friday 2015 May 8th.

IMPROVING SUNSPOT RECORDS: OBSERVATIONS IN 1728–1729 BY J. F. WEIDLER

By V. M. S. Carrasco,
Departamento de Física, Universidad de Extremadura, Badajoz, Spain

J. Villalba Álvarez
Departamento de Ciencias de la Antigüedad, Universidad de Extremadura, Cáceres, Spain

& J. M. Vaquero
Departamento de Física, Universidad de Extremadura, Mérida, Spain.

We revisit the sunspot observations published by Johann Friedrich Weidler in the book *Observationes Meteorologicae, atque Astronomicae ann. MDCCXXIIX et MDCCXXIX* (1729) covering the period 1728–1729. We present the original text and a detailed English translation of these records. We have analysed Weidler's comments which give the sunspot count for each day of observation. We note that Weidler registered the total number of sunspots observed and not the total number of sunspot groups, as is assumed in the Group Sunspot Number data-base. We have added three sunspot records that had not been compiled previously. Finally, the area of one of the largest sunspots registered by Weidler is estimated.

Introduction

Although sunspots have been observed with the naked eye over the past two thousand years, they did not begin to be systematically recorded until the 17th Century with the use of the telescope[1,2]. The two main indices of solar activity based on sunspot counts are the International Sunspot Number (ISN)[3] and the Group Sunspot Number (GSN)[4]. Those indices show similar behaviour from about the year 1880 onwards. Before that, however, there are discrepancies between the two indices[5]. It is therefore necessary to revise the sunspot observations recorded historically by astronomers so as to obtain solar activity indices that are as trustworthy as possible. It is fundamental for scientific studies in solar physics and solar–terrestrial physics that those indices faithfully reproduce past solar activity.

Many historical handwritten and printed documents contain information about sunspot observations recorded by early astronomers which can provide us with information on past solar activity[6–9]. There also exist documents with other information, such as aurorae, which can improve our understanding of the behaviour of the Sun in recent centuries[10]. The main objective of this article is to retrieve and analyse the information on sunspots contained in the meteorological records made by J. F. Weidler[11] during the years 1728 and 1729. The importance of those records is that they are the only observations of sunspots available for 1728 and early 1729. They were used by Hoyt & Schatten[4] to construct the GSN. Since that index has poor temporal coverage for the first half of the 18th Century, it is necessary to seek new sources of information and to revise the records that are available to correct any errors that might exist[12].

It is worthy of mention that the observations by Weidler correspond to the recovery phase of solar activity after the Maunder Minimum, the only grand minimum of solar activity that has occurred during the last 400 years, when the astronomical telescope has been available. The Maunder Minimum has been characterized by a very low solar activity[13] and a strong hemispheric asymmetry in the solar activity[14,15].

J. F. Weidler's sunspot records

The records of sunspots made during the period 1728–1729 by the German astronomer Johann Friedrich Weidler were published in the book *Observationes Meteorologicae, atque Astronomicae ann. MDCCXXIIX et MDCCXXIX* (1729). Weidler[11] presented his meteorological observations in a table divided into six

METEOROLOGICAE. 35

1728 Aprilis St. N. D. H.	Barom. alt. Dig. Lin.	Therm. alt. Gr.	Vent.	Tempeſtas.	Pluuia Cub. Lin.
20. a. m.	27. 11	78	N. N. W. 1	nubes interruptae - -	- -
21 -	- 5	61½	W. 1	pluit tota nocte praec. -	13. 5
				pluuia antemer. - -	7. 7
22 -	- 7	72	W. 3	caliginoſum. pluu. noct. -	4. 22
				maculae 6. maxima $\frac{1}{30}$ diam. ☉	
23 -	- 10½	74	S. W. 1	ſerenum. pluu. noct. -	1. 15
24 -	- 9	69		ſerenum. - - -	- -
25 -	28. 0	63	O. 2	nubiloſum. nouae maculae duae	- -
				in ipſo ſolae natae. - -	
26 -	27. 8	60	S. O. 1	ſerenum. - - -	- -
27 -	- 9	52	W. 2	caliginoſum. pluu. a. m.	2. 10
28 -		60	N. W. 1	nubiloſum. pluu. noct.	- 12
29 -	- 11	71		idem. maculae 13. in ſole maxima uicinae adhaerens capit $\frac{1}{24}$ diam. ☉.	
30 -	- 11	65	N. W. 2	ſerenum.	
				Pluuia M. Aprilis	78. 23
				Nota	
				1. altitudo maxima ☿. 28. 0. d. 25.	
				2. altitudo minima ei. 27. 2. d. 1.	
				3. altitudo max. Thermom. 52. d. 15. 27.	
				4. altitudo min. Therm. 78. d. 19. 20.	
				M. Maius, 1728.	
1 -	- 6½		W. N. W. 1	coelum pluuioſum. pluit. a. m.	3. 8
2 -	- 4½	52	W. N. W. 1	nubes interruptae. pluu. noct.	- 16
				tonuit h. 2. a. m. poſtea ſerenitas.	
3 -	- 3	44	S. O. 1	nubiloſum. 7. maculae ſub ☉	- -
1. p. m	- -	23	- -	- -	
4 -	- 5	52	N. W. 3	nubiloſum. - - -	- -
5 -	- 8	44	O. S. O. 2	ſerenum . -	- -
6 -	- 9	49	O. S. O. 3	nubes ſparſae - -	- -

E 2 7 -

FIG. 1

An example of a page of Weidler's records.

columns (Fig. 1). The fifth column of that table, labelled 'Tempestas' (Storm), apart from reflecting the state of the weather, contains Weidler's annotations about sunspots. The first column indicates the date of observation, and the remaining columns contain weather data.

In this section, we shall present the solar observations recorded by Weidler during the years 1728 and 1729 (Table I) in Wittenberg, Germany (51° 52′ N, 12° 39′ E). We have studied those observations with care and, after translating the original Latin text into English, explicitly extracted the information regarding sunspots. Thus, we constructed Table I to present all the information currently available on those observations with the following structure: (*i*) the first column gives the date (year, month, day) when the observation was made, and (*ii*) the second column gives the original comments made by Weidler in Latin and our English translation (in brackets). Apart from providing information about the weather conditions, Weidler repeatedly provided information about the size (relative to the solar diameter) of the largest sunspots he observed.

As Weidler indicated in the text that introduced the observations (which we reproduce, with its translation into English, in the Appendix), his main motive for carrying out the observations of sunspots was to look for a connection between them and the weather. Weidler noted that some scientists had estimated that the size of the sunspots could influence meteorological changes due to the reduction of the intensity of sunlight (see the Appendix).

To carry out his observations, Weidler acquired an astronomical, 3·7-Paris-feet-long, telescope, and incorporated into it a cone-shaped tube which functioned as a pinhole camera for the comfortable observation of sunspots by projection. Note that 1 Paris foot is equal to 32·48 cm[16], so the telescope would have had a focal length of about 1200 mm. Weidler projected the image of the Sun into a circle in which he could straightforwardly determine, for example, the position and number of spots (see the Appendix).

We found an error of omission in Hoyt & Schatten's database[4] in that they overlooked incorporating three observations by Weidler corresponding to the days 1728 July 31, 1728 August 12, and 1728 August 15. Thus, we have 25 observations made by Weidler in 1728, instead of the 22 reported by Hoyt & Schatten[4] in their data-base.

Analysis

Weidler's sunspot observations, which we present in this communication, although not very numerous (30 records in total), have a good temporal distribution. They began on 1728 April 2 and ended on 1729 March 18. There are records for all of the months in that period except October and December of the year 1728. Table II lists the number of sunspots recorded by Weidler for each of his days of observation.

Fig. 2 shows the sunspot counts of the principal observers during the period 1723–1733, and the annual values of the GSN index provided by Hoyt & Schatten[4]. There were many observers who kept sunspot records during that period, although the only available records of sunspots for the year 1728 and the beginning of 1729 are those of Weidler. We selected the principal observers in the figure on the basis of their number of records and the temporal coverage of their observations. For example, the figure does not include the observation made by the Jesuits I. Koegler and A. Pereira on 1730 July 15, in which they recorded seven sunspots (not seven groups of sunspots as given in the GSN index database)[17–19].

One sees in Fig. 2 that, for that solar cycle, Weidler reports the greatest sunspot count of all the observers. That is because, according to Weidler's own

TABLE I

Weidler's sunspot observations during the period 1728 to 1729

Date	*Original comment [translation]*
1728 Apr. 2	*caliginosum. pluv. noct. duae magnae maculae sub ⊙ altera 1/30 altera 1/36 diametri.* ⊙. [Dense clouds, night rain; two large spots on the Sun, one 1/30 of a solar diameter, the other 1/36 of a solar diameter.]
1728 Apr. 4	*Serenum. maculae 4 sub ⊙; quarum maxima d. 2 iam spectata, nunc crevit et 1/26 diam. ⊙ aequalis evasit aurora borea vesp. h. 9 conspecta.* [Calm. Four spots on the Sun; the largest, which was already noticed on the 2nd of the month, has now grown and measures 1/26 of a solar diameter; the aurora borealis, noted at 9 pm, has disappeared.]
1728 Apr. 5	*nubes interruptae pluv. vesp. maculae 7 sub* ⊙. [Scattered clouds, evening rain, seven spots on the Sun.]
1728 Apr. 18	*albae nubes. maculae 11. maxima 1/24 diam.* ⊙. [White clouds, eleven spots, the largest 1/24 of a solar diameter.]
1728 Apr. 22	*caliginosum. pluv. noct. maculae 6. Maxima 1/30 diam* ⊙. [Dense clouds, night rain, 6 spots, the largest 1/30 of a solar diameter.]
1728 Apr. 25	*nubilosum. novae maculae duae in ipso sole natae.* [Overcast. Two new spots have appeared on the Sun.]
1728 Apr. 29	*nubilosum. maculae 13 in sole maxima vicinae adhaerens capit 1/24 diam.* ⊙. [Overcast. Thirteen spots; the largest, adhered to another, presents 1/24 of a solar diameter.]
1728 May 3	*nubilosum. 7 maculae sub* ⊙. [Overcast. Seven spots on the Sun.]
1728 May 8	*serenum. Maculae 5. Duae ex illis aequalis 1/30 diam.* ⊙. [Calm. Five spots. Two of them equal to 1/30 of a solar diameter.]
1728 May 12	*nubes sparsae. maculae 10.* [Scattered clouds. Ten spots.]
1728 May 19	*serenum. Maculae 6. Duae ex his proxime inter se connexae aequales 1/18 diam.* ⊙. [Calm. Six spots; two of them, joined together, equal to 1/18 of a solar diameter.]
1728 Jun. 11	*serenum. maculae duae magnae sub sole, quaelibet 1/26 diam.* ⊙. [Calm. Two large spots on the Sun; both 1/26 of a solar diameter.]
1728 Jun. 15	*serenum. tres maculae sub sole. maxima 1/24 diam. solis.* [Calm. Three spots on the Sun. The largest, 1/24 of a solar diameter.]
1728 Jun. 22	*octo maculae sub ⊙. maxima 1/26 diam. solis.* [Eight spots on the Sun, the largest 1/26 of a solar diameter.]
1728 Jun. 28	*serenum. duae paruae maculae sub sole.* [Calm. Two small spots on the Sun.]
1728 Jul. 6	*maculae duae sub sole.* [Two spots on the Sun.]
1728 Jul. 9	*maculae septem sub sole.* [Seven spots on the Sun.]
1728 Jul. 14	*serenum. maculae 5 sub sole, maxima 1/28 diametri solis.* [Calm. Five spots on the Sun, the largest 1/28 of a solar diameter.]

TABLE I (*concluded*)

1728 Jul. 18	*serenum. maculae 4 sub sole.* [Calm. Four spots on the Sun.]
1728 Jul. 31	*maculae 7 sub ⊙. Maxima 1/26 diam. solis par. stipata utrimque paruis quatuor, quae totum tractum cui incumbebant obscurum reddere et unam maculam referre videbantur.* [Seven spots on the Sun. The largest 1/26 of a solar diameter, surrounded on both sides by four small ones, that seemed to darken the entire zone towards which they were inclined, resulting in a single spot.]
1728 Aug.12	*serenum. duae magnae maculae in sole spectatae quaevis 1/30 diam. solis.* [Calm. Two large spots sighted on the Sun, both 1/30 of a solar diameter.]
1728 Aug. 15	*macula una ex iis quae apparuere d.* 12 *iam magnitudine sic creuit, ut 1/26 diam. solis caperet.* [A spot which appeared on the 12th has grown to reach 1/26 of a solar diameter.]
1728 Sep. 17	*nubilosum. duae maculae sub sole, maior 1/32 diam. solis.* [Overcast. Two spots on the Sun, the larger 1/32 of a solar diameter.]
1728 Sep. 28	*maculae sub sole tres una ex duabus coniunctis conflata duae maximae 1/36 diam, pares.* [Three spots on the Sun; one composed of two which are joined; the two largest, equal in size, 1/36 of a solar diameter.]
1728 Nov. 24	*sol 9 maculis conspersus. Maxima 1/30 diam. solis.* [The Sun flecked with nine spots. The largest 1/30 of a solar diameter.]
1729 Jan. 10	*serenum. duae maculae sub ⊙ magnae, altera 1/36 diam. solis. altera parum minor.* [Calm. Two large spots on the Sun, one 1/36 of a solar diameter; the other a little smaller.]
1729 Feb. 1	*albae nubes. Glacies concreuit noctu. Macula una sub sole aequalis 1/28 diam. solis.* [White clouds. There was frost at night. A single spot on the Sun, equal to 1/28 of a solar diameter.]
1729 Feb. 4	*serenum. maculae 8 sub sole, maxima 1/24 diam. ⊙.* [Calm. Eight spots on the Sun, the largest 1/24 of a solar diameter.]
1729 Feb. 7	*serenum. maculae novem sub sole, maxima unica 1/36 diam. solis.* [Calm. Nine spots on the Sun; the largest 1/36 of a solar diameter.]
1729 Mar. 18	*serenum coelum. in sole macula ingens ex duabus consflata spectatur. longa 1/18 lata 1/30 diametri solis.* [Clear sky. On the Sun, one observes a great spot composed of two; it measures 1/18 in length and 1/30 in width with respect to a solar diameter.]

comments, his records referred to the total number of sunspots and not to the total number of sunspot groups (as is stated in Hoyt & Schatten's database[4]). Another reason that makes us think that Weidler really recorded the total number of sunspots is found in the observations made in early 1729 February: if he had registered sunspot groups, he would have gone from registering a single group on February 1 to eight groups on February 4, only three days later. One can see how likely that would have been by using the sunspot-group catalogue

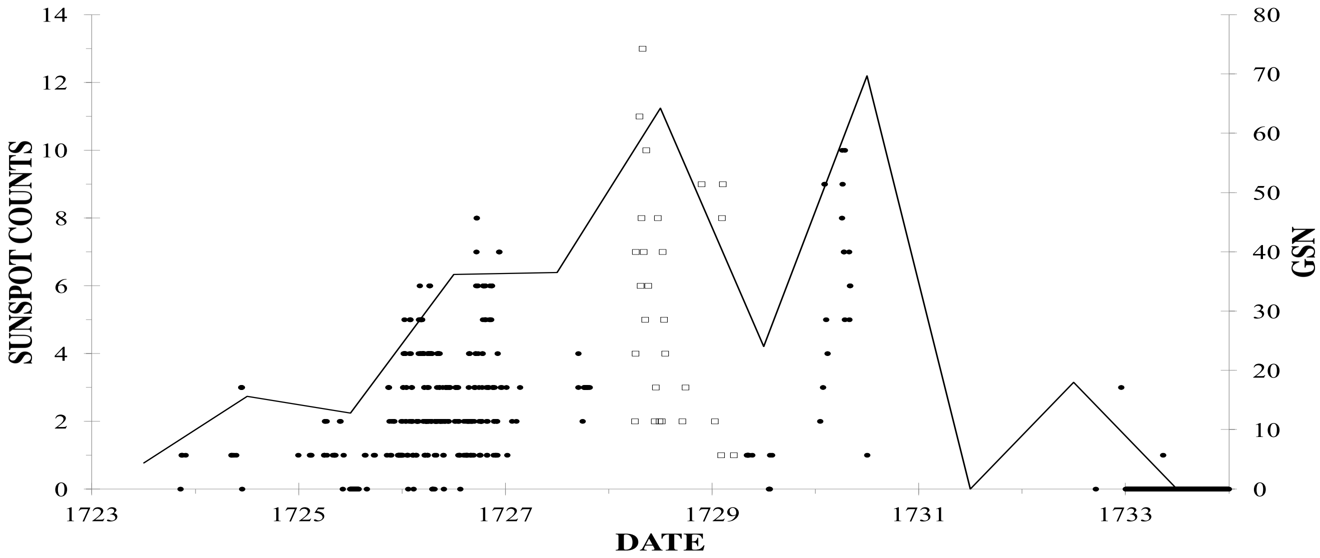

FIG. 2

Temporal evolution of daily values of sunspot counts from 1723 to 1733 showing Weidler's records (squares), other main observers (black dots, including Alderburner, Alischer, Beyer, Blanchini, Cassini, Gaulbil, Kirch, Kraft, Le Monnier, Plantade, and Walther), and the annual GSN values (black line).

TABLE II

Weidler's sunspot counts during the years 1728 and 1729

	Date	*Sunspot Count*		*Date*	*Sunspot Count*		*Date*	*Sunspot Count*
1728	Apr. 2	2	1728	May 19	6	1728	Aug. 12	2
	Apr. 4	4		Jun. 11	2		Aug. 15	2
	Apr. 5	7		Jun. 15	3		Sep. 17	2
	Apr. 18	11		Jun. 22	8		Sep. 28	3
	Apr. 22	6		Jun. 28	2		Nov. 24	9
	Apr. 25	8		Jul. 6	2	1729	Jan. 10	2
	Apr. 29	13		Jul. 9	7		Feb. 1	1
	May 3	7		Jul. 14	5		Feb. 4	8
	May 8	5		Jul. 18	4		Feb. 7	9
	May 12	10		Jul. 31	7		Mar. 18	2

prepared by the Royal Greenwich Observatory (1874–1976) and the US Air Force/NOAA (1976–present) during the period 1874–2014. With those modern data, the probability of such an increase occurring (going from one group of sunspots to eight groups in three days or less) is 0·06%. Specifically, only four such cases have occurred: 1908 March/April, 1915 December, 1961 August, and 1985 April. In view of that fact, and given that, as we have mentioned above, the only observations available for 1728 and early 1729 are those by Weidler, the Group Sunspot Number index might well be overestimated for that period. Furthermore, one can see in Fig. 2 that one of the two peaks in the sunspot counts coincides with Weidler's observation period.

Some of Weidler's comments allow us to estimate the area of some of the sunspots that he recorded. That is the case, for instance, for 1729 March 18. According to Weidler's records, on that day he observed a large sunspot, separable into two, with a length and width of $1/18$ and $1/30$ of the Sun's diameter, respectively. As a lower bound of the area, we shall assume that the spot was at the centre of the solar disc. Thus, taking an elliptical geometry for the sunspot, and given that the area of an ellipse is given by the expression $A = \pi ab$, where a and b are the semi-axes of the ellipse, the area occupied by the sunspot would have been approximately equal to 925 millionths of the solar hemisphere. Owing to the projection of the solar disc, the real area of the sunspot would have been greater than that lower bound according to how close the actual position of the spot was to the solar limb.

Conclusions

In this communication, we have presented the sunspot observations made by J. F. Weidler during the years 1728 and 1729, and that were published in the book *Observationes Meteorologicae, atque Astronomicae ann. MDCCXXIIX et MDCCXXIX* (1729). To that end, we gave a translation into English of the original Latin text. We also added three new observations that had been overlooked by Hoyt & Schatten[4]. In his annotations, Weidler recorded the total number of sunspots and not the number of sunspot groups as given in the GSN index data-base. That index may therefore be an overestimate since Weidler's observations are the only ones available for that period. In addition, we have been able to estimate a lower bound for the area of one of the largest spots that Weidler recorded (925 millionths of a solar hemisphere) from his annotations concerning the size of some of the spots relative to the solar diameter. Finally, we wish to highlight the need for a revision to be made of the observations currently available and for new sources of information to be found so as to improve the characterization of the solar activity in the first half of the eighteenth century.

Acknowledgements

Support from the Junta de Extremadura (Research Group Grant No. GR10131) and from the Ministerio de Economía y Competitividad of the Spanish Government (AYA2011-25945 and AYA2014-57556-P) is gratefully acknowledged.

References

(1) J. M. Vaquero & M. Vázquez, *The Sun Recorded Through History* (Springer), 2009.
(2) I. G. Usoskin, *Living Rev. Solar Phys.*, **10**, 1, 2013.
(3) F. Clette *et al.*, *Adv. Space Res.*, **40**, 919, 2007.
(4) D. V. Hoyt & K. H. Schatten, *Solar Phys.*, **179**, 189, 1998.
(5) F. Clette *et al.*, *Space Sci. Rev.*, **186**, 35, 2014.
(6) J. M. Vaquero, *Adv. Space Res.*, **40**, 929, 2007.
(7) R. Arlt, *Solar Phys.*, **247**, 399, 2008.
(8) V. M. S. Carrasco *et al.*, *New Astron.*, **25**, 95, 2012.
(9) V. M. S. Carrasco, J. Villalba Álvarez & J. M. Vaquero, *Solar Phys.*, submitted, 2015 (arXiv:1502.06270).
(10) M. Vázquez, J. M. Vaquero & M. C. Gallego, *Solar Phys.*, **289**, 1843, 2014.
(11) J. F. Weidler, *Observationes Meteorologicae, atque Astronomicae ann. MDCCXXIIX et MDCCXXIX* (Gerdesia), 1729.
(12) J. M. Vaquero, M. C. Gallego & R. M. Trigo, *Adv. Space Res.*, **40**, 1895, 2007.
(13) J. Eddy, *Science*, **192**, 1189, 1976.
(14) J. Ribes, & E. Nesme-Ribes, *A&A*, **276**, 549, 1993.
(15) J. M. Vaquero, J. M. Nogales & F. Sánchez-Bajo, *Adv. Space Res.*, **55**, 1546, 2015.
(16) H. H. Lamb, *Weather*, **41**, 230, 1986.
(17) I. Kegler & A. Pereira, *Phil. Trans. XXXVII*, **420**, 179, 1733.
(18) I. Kegler & A. Pereira, *Memorias de Mathematica e Phisica da Academia R. Das Sciencias de Lisboa*, **2**, 36, 1799.
(19) J. M. Vaquero, M. C. Gallego & F. Sánchez-Bajo, *The Observatory*, **127**, 221, 2007.

Appendix

Weidler's text in *Observationes Meteorologicae, atque Astronomicae ann. MDCCXXIIX et MDCCXXIX, Part XI*

Original Latin text

Cum porro eruditi quidam viri existimaverint tempestatis mutationes a solarium quoque macularum magnitudine, qua aestus solis vis nonnihil temperetur, proficisci posse, dedi operam ut et haec spectacula subinde annotarentur, inprimis quoniam his annis tribus proxime elapsis sol plurimas et saepe etiam magnas eiusmodi nebulas ostendit. Et ut haec obseruatio sine ulla temporis iactura, et absque camerae obscuratae subsidiis, commodius fieret, assumsi notae melioris telescopium astronomicum tres pedes Parisinos et septem digitos longum, eidemque tubum conicum paulo astronomico ampliorem, sive cameram obscuram portabilem sic aptavi, ut excepta intra hanc solis pictura, in circulo observationis rite diviso, statim magnitudinem, numerum, situmque macularum sisteret, ut inde in diarium illius rude simulacrum referri posset. Reddidi *autem huius organi usum tam facilem, ut vel ex inquilinis aut auditoribus meis aliquis, vel servus quoque meus me absente eodem maculas inspicere et delineare, mihique illarum conditionem repraesentare potuerit. Harum itaque contemplationum in diario, cuius nunc specimen prodit, subinde mentio iniicitur.*

English translation

Given that some scholars have estimated that meteorological changes may also be caused by the size of sunspots, which can cause the Sun to temper its warmth somewhat, without more ado I set myself to recording my observations, especially because in the last three years the Sun has shown many and often very large shadows of that type. And to perform more comfortably my observations, without any loss of time, and being without the aid of a camera obscura, I bought an astronomical telescope of the best quality, 3·7 Paris feet in length, to which I incorporated a tapered tube slightly broader than the telescope, by way of a portable camera obscura. Thus, the Sun's image contained within it immediately determined the size, number, and position of the spots in a circle of observation that was perfectly divided, so that then it could be copied straightforwardly into my journal. And I managed to make the use of this instrument so easy that, if I were absent, one of my guests or my students, or even one of my servants could observe for themselves the spots, draw them, and describe their appearance to me. Thus, of such observations we then commented in the journal of which a small sample is now offered.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 244: SIX UPGREN STARS IN THE NORTH-GALACTIC-POLE FIELD

(U 27° 10, U 28° 12, U 29° 15, U 28° 32, U 29° 56, AND U 25° 40)

AND THE BRIGHT STAR 4 COMAE BERENICES (U 26° 58), A NEWLY DISCOVERED SB2

By R. F. Griffin
Cambridge Observatories

The distributions of the radial velocities of stars near the Galactic Poles, where radial velocities largely equate to z motions, offer some insights into Galactic dynamics. Yoss & Griffin[1] published in 1997 an analysis based on a comprehensive catalogue of the radial velocities and photometrically estimated spectral types and luminosities for about 900 late-type stars, including almost all of those having types of G5 and later in the *Henry Draper Catalogue*,

at Galactic latitudes above 75°. An extension of the investigation to fainter and more distant stars was begun, but has not been completed or published, on the basis of a listing of such stars by Upgren[2]. 'Copenhagen-style' narrow-band photometry was obtained a long time ago for many of the Upgren stars, and radial-velocity measurements that were begun in 1972 have proved certain of them to be spectroscopic binaries. The orbits of six of them, including one double-lined object (U 29° 56) are presented in this paper. The proximity of one of them to the $5\frac{1}{2}^{m}$ star 4 Comae — itself a late-type North-Galactic-Pole star and an Upgren object — led to a renewed interest in that star (for which an orbit was published before the present writer was born) and the recognition that it is double-lined; a preliminary SB2 orbit is presented.

Introduction

In 1967, soon after the original photoelectric radial-velocity spectrometer[3] was constructed at Cambridge, observations were begun on a programme embracing all the late-type *Henry Draper Catalogue* stars within 15° of the North Galactic Pole, with a view ultimately to estimating the velocity dispersions of different groups of stars in the z direction in the Galaxy, and perhaps also the total density per unit area (solar masses per square parsec) of the Galactic disc. Those ends were partly achieved after thirty years, in a paper[1] published by the late Dr. Kenneth Yoss and the writer in 1997. It was Yoss who master-minded the discussion. He personally obtained both *BV* and *DDO* photometry, the latter enabling estimates to be given of spectral types, M_V, and [Fe/H] for the great majority of the stars, while the present writer supplied the radial velocities. Well over 100 spectroscopic binaries were identified; orbits have been given for 65 of them (so far) in the series of papers of which *this* one is a member, and 34 others in 24 papers in a specially designated series, *Spectroscopic binaries near the North Galactic Pole*, in the Indian *Journal of Astrophysics & Astronomy*; four more have been published elsewhere in collaboration with other authors.

The radial velocities constituted only one facet of the project to investigate Galactic structure, as indicated in the paragraph above, through characterization of the z-motions of the stars in a column perpendicular to the Galactic plane at our location in the Galaxy. The discussion of those motions needed, in addition, knowledge of the individual z distances of the stars above the Galactic plane. Much of the initial Cambridge work on the field of the North Galactic Pole (hereinafter abbreviated NGP) represented the PhD project of G. A. Radford, who was at the time (the early 1970s) a graduate student under the present writer's supervision; he concluded that the best way of obtaining the desired stellar distances was through appropriate narrow-band photometry. Such photometry was by no means a new idea at Cambridge, where it had been developed[4] through the use of spectrometers that could isolate bands of wavelength very cleanly and reproducibly, with accurately specified cut-off wavelengths, and measure several of them simultaneously. There was even a routine arrangement for shifting the bands to follow the topocentric radial velocities of the individual stars that were observed, so that the bands always corresponded to the same set of spectral features despite the velocity shifts.

That method was particularly appropriate to rather narrow bands, and also to the nature of observing conditions in Cambridge, where the sky is very rarely such as a photometric astronomer would find acceptable.

Photometry analogous in character to conventional *UBV* observations, but with appropriately chosen narrower bandwidths (usually isolated by custom-made interference filters) had, however, been developed at other places with a view to determining salient facts about stars; Radford surveyed the literature and concluded that for the purpose of determining the absolute magnitudes (and thereby distances) for the large number of late-type NGP stars, the 'Copenhagen' photometric system[5,6] was the most promising. It was with some reluctance that the writer, who had been apt to extol the advantages of the spectrometer method (whose development had formed part of his own PhD work), with its accurate and reproducible cut-off wavelengths and neat 'top-hat-function' transmission bands, felt constrained to return to the sort of photometry whose transmission bands are rather nebulously defined by filters of one sort or another; but the exigencies of the work did seem to demand such a climb-down. The Copenhagen photometry gave approximate *V* magnitudes and allowed estimates to be made of the stars' absolute magnitudes (and thus distances) and metallicities ([Fe/H] values); in addition, a quantity '*res*(*k*)' that could be formed from it, if greater than about $0^{m}\cdot04$, flagged spectroscopic compositeness — the source in such a case must really consist of at least two separate sources, of different temperatures.

We were fortunate in being able to enlist the coöperation of the Copenhagen astronomers for the project, and to obtain observing time at a photometrically suitable site. In what was in the nature of a pilot photometric programme[7] in 1975 February, Radford, with Dr. L. Hansen of the Copenhagen University Observatory, obtained narrow-band 'Copenhagen photometry' with a 36-inch telescope at Kitt Peak for nearly 300 NGP stars. Two observing runs that were scheduled at another observatory in the same NGP observing season were entirely lost through failure of the essential instrumentation of the host observatory. A much larger but not unreservedly successful effort was mounted in 1976 to obtain, in an analogous way but at the Palomar 20-inch [*sic*] and 60-inch telescopes, mostly by Radford, comprehensive Copenhagen photometry for all the late-type NGP *HD* stars and many fainter ones as well. Radford was at Palomar for the whole NGP observing season, while the writer made four visits there from Cambridge, to 'hold the fort' at the 20-inch while we simultaneously had the use of the 60-inch.

As an aside, the writer recalls (not without some pride and due gratitude) that at the time he was already fully familiar with, and *persona grata* at, the Palomar Observatory, where he was privileged to use the 200-inch reflector on a regular basis in furtherance of other projects. The 20-inch telescope was not much regarded at an observatory that had a 200-inch one, nor was its observing time in much demand, so our project was able to monopolize it for the whole 1976 NGP season, from February to June. The telescope was in an enclosure analogous to a scaled-up dog-kennel, rectangular, with a pitched roof having two leaves hinged at the eaves, which were at about shoulder level. The hinges ran north–south. The roof elements could be opened all the way to the outward-facing horizontal, leaving the observer effectively in the open air, with no need to turn a dome to reach 'extinction stars'; but they could also independently be left partly open, and on occasion one of them would be left standing vertically to give the observer some shelter from a cold wind. It was an effective telescope, but the output system for the photometry was not of the most up-to-date character for the time, consisting of a type-writer-like printer that wrote the results on adding-machine ('butcher') paper tape. With several photometric bands observed for every star, and many stars to be observed, the printing covered a lot of tape (and subsequently needed to be re-typed into a computer). The project was intended to have been undertaken with a (computerized) photometer that was to have been provided by our Danish collaborators, but in the event was mainly performed with a two-channel (star/sky) photometer made very suddenly at Cambridge to the writer's design after it transpired almost at the last minute that the sophisticated Danish photometer would after all not be available. The observations left Palomar in the form of a suitcase-full of paper tape, which I (for one) did not wish to come to Cambridge. On my proposal, Radford left it with Dr. Hansen in Copenhagen 'on his way home'; the measurements were kindly reduced by Dr. Hansen, who managed to employ an assistant to cope with the large and arid task of transcribing the 'butcher tapes' into a computer, and there is occasion to note a few of the results below.

The Upgren stars

Meanwhile, it was quite apparent that, in furthering the same astronomical interests, there would be merit in observing stars more distant from the Galactic plane than most of those listed in the *Henry Draper Catalogue*, implying that they would be of fainter apparent magnitudes. The difficulty of identifying faint stars with spectral types appropriate to radial-velocity measurement with the photoelectric spectrometer seemed to be partly met by a catalogue published by Upgren[2] in 1962, giving classifications for stars in a considerable area of sky near (though very asymmetrically disposed, as Upgren's Fig. 1 makes clear, with respect to) the Galactic Pole. The Upgren stars overlapped with *Henry Draper Catalogue* ones at the bright end, but ran down to about 12^{m}. The radial velocities of about 170 of them were first measured during an observing run (most of which was actually devoted to other projects) at the 200-inch telescope at Palomar in 1972, quite shortly after Dr. James Gunn and the writer made a radial-velocity spectrometer[8] for it. It was an easy project at Palomar — no matter how faint the Upgren stars were they could be well observed in integrations lasting only a minute or so. The big telescope cut through such a programme like a knife through butter. Smaller numbers of additional Upgren stars were measured in two subsequent observing runs with the same telescope, in rather brief sessions filched from other programmes. Most of the stars were really too faint for observation from Cambridge with the original spectrometer, which was in any case fully occupied with the *HD* stars in the self-same area of the sky, so there was little further progress on the project for about 15 years, until the writer had the good fortune to obtain regular observing time with the *Coravel* at the Geneva 1-m telescope at the Observatoire de Haute-Provence (OHP). Subsequently, when an analogous instrument was brought into use at Cambridge, the stars could be observed from the writer's home site, although the faintest ones were still a bit marginal for the 36-inch telescope.

Naturally, as the observations accumulated, certain of the Upgren stars were found to exhibit velocity variations, and they have been observed as opportunity has occurred, with a view to determining their orbits. The principal underlying programme has never been described previously, let alone had its results made public, but orbits are offered here for a number of the Upgren stars that are too faint to have featured in the *Henry Draper Catalogue*. Since the whole purpose of the radial-velocity programme on Upgren stars was to reach larger distances from the Galactic plane, the stars selected for measurement were as far as possible giants, so only stars that Upgren classified with luminosity class III were admitted to the programme. Upgren seemed less certain about his luminosity classifications of G-type stars, so the selection was restricted to those that he classified as K-type giants.

U 27° 10

This star is faint enough to require reasonably accurate coördinates and/or a finding chart to identify it on the sky, but for orientation it may be said to be in the north-following corner of Leo, about 6° north of 93 Leo, an interesting composite-spectrum binary system whose spectrum and nature were discussed[9] some time ago by Dr. R. E. M. Griffin and myself. Under the convention of the *Centre de Données Astronomiques* (formerly known in English as the *Stellar Data Centre*), the star of present interest would be known as 'NGP 27 10'. It cannot be retrieved from *Simbad*, however, under that name; it seems that only certain of the Upgren stars — it looks as if it may be only the ones that are also in the *BD*[10] — were ever entered into that data-base under such designations. At least some of the others, however, are in fact present in *Simbad*, but not under their Upgren designations, and Upgren's paper[2] is not retrieved in their bibliographies. U 27° 10 is a case in point: by estimating its coördinates from Upgren's identification chart one can thereby locate it in *Simbad*, where it appears as *Tycho* 1987-1474-1, and the picture that can be brought up of the corresponding area of sky matches the finding chart in Upgren's paper and confirms the identity of the *Tycho* star with U 27° 10. The *Simbad* bibliography is, however, empty, not even retrieving the Upgren paper[2]; but we can learn from it the V magnitude of $10^{m}{\cdot}50$ and B of $11^{m}{\cdot}92$, which have been transformed from the *Tycho* V_T and B_T more or less in accordance with the equations given by *Hipparcos*[11] (vol. **1**, p. 57), though with uncertainties listed as $0^{m}{\cdot}07$ and $0^{m}{\cdot}23$, respectively. The Upgren spectral classification of K0 III is not retrieved, either; it is not in good accord with the $(B-V)$ found from *Tycho*, but that quantity is so poorly determined that the

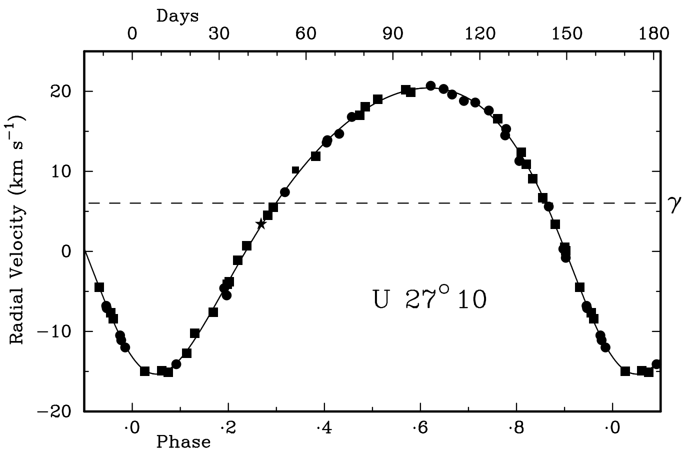

FIG. 1

The observed radial velocities of U 27° 10 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Circles and squares represent measurements made with the OHP and Cambridge *Coravel*s, respectively. One square that is plotted smaller than the others refers to an observation that was noted as uncertain at the time of observation, is identified with a colon in Table I, and was given half-weight in the solution of the orbit. The star symbol plots the early (1972) Palomar observation.

discordance with the spectral type can hardly be regarded as significant. The Palomar narrow-band photometry referred to in the *Introduction* above implied for U 27° 10 a V magnitude of 10·68, an M_V of +2·1, and an [Fe/H] of –0·46; the quantity *res*(*k*) which can be formed from the Copenhagen photometry and flags photometric anomaly was $0^{m}\cdot043$, which is considered just large enough to be significant evidence of duplicity, *i.e.*, that the photometry must arise from two separate sources having different temperatures.

As for so many Upgren stars, the initial radial-velocity observation of U 27° 10 was made with the 200-inch telescope in 1972. The next measurement was not made until 1989, at OHP; it was discordant with the Palomar one and led to the systematic re-observation of the star in subsequent observing runs at OHP, where 25 measurements were made in all. Thirty more have been made with the Cambridge *Coravel*, making 56 in total; they are set out in Table I. In all such tables, OHP velocities have been increased by 0·8 km s^{-1} from the values obtained from the data-base in Geneva, in an effort to place them on the scale adopted in Cambridge and traceable to the project[12] to tie the velocities of the original Cambridge reference stars to those of a large number of field stars whose velocities were listed in the *Radial Velocity Catalogue*[13]. The OHP and Palomar observations have been given half-weight in the solution of the orbit, which is plotted in Fig. 1 and has the following elements.

P	= 165·848 ± 0·006 days	$(T)_{65}$	= MJD 52058·1 ± 0·5
γ	= +6·03 ± 0·06 km s^{-1}	$a_1 \sin i$	= 39·76 ± 0·20 Gm
K	= 17·87 ± 0·09 km s^{-1}	$f(m)$	= 0·0912 ± 0·0014 $M_\odot$
e	= 0·221 ± 0·004		
ω	= 151·8 ± 1·2 degrees	R.m.s. residual (wt. 1)	= 0·34 km s^{-1}

The observations are seen to be well distributed around the orbit, and at 0·34 km s^{-1} their residuals are quite satisfactory for a star that is almost twelfth magnitude in the B region in which the radial-velocity spectrometers operate. Although much higher velocity precision is obtained nowadays with large telescopes in the search for evidence of planets around comparatively bright stars, the performance of the *Coravel*s might well be viewed in the light of the situation that prevailed at the time (1954) that the *Radial Velocity Catalogue*[13] was compiled, not very long before the cross-correlation procedure that has enabled such a great improvement in radial-velocity measurement was developed[3]. In that *Catalogue* a velocity was attributed class *a*, the highest class, if the *mean* value of a star's velocity was considered to be good to 1 km s^{-1} — even for *Bright Star Catalogue* stars, to which in fact that category of precision was largely restricted. Even the first photoelectric radial-velocity spectrometer[3], operating by cross-correlation, immediately extended the reach of the modest Cambridge telescope to ninth magnitude with a precision close to 1 km s^{-1} *for individual observations*[14], and when some experience was gained with it the observational errors were reduced appreciably further. Quite soon afterwards, the reach of radial velocities good to 1 km s^{-1} was extended to the fourteenth magnitude by the first digital cross-correlation instrument, working at the 200-inch telescope[8]. In this present paper, in this section, a $10\frac{1}{2}^{m}$ star has been routinely measured with the 36-inch reflector to an r.m.s. precision of 0·34 km s^{-1}. In the information-theoretical sense, the 'weight' of an observation is related to the inverse square of its uncertainty, so the performance of the *Coravel* on U 27° 10 could be considered equivalent to a 1-km s^{-1} standard error at about 13^{m}, representing a 6½-magnitude (400-fold) gain over the general accessibility

TABLE I

Radial-velocity observations of U 27° 10

Except as noted, the sources of the observations are as follows:
1991–1997 — OHP Coravel (weight ½); 2000–2014 — Cambridge Coravel (weight 1)

Date (UT)			MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
1972	Jan.	6·49*	41322·49	+3·4	0·268	+0·2
1989	Mar.	26·96	47611·96	−4·6	38·191	+0·1
1992	Apr.	24·95	48736·95	−10·5	44·975	+0·1
	June	20·88	793·88	+7·4	45·318	−0·2
	Dec.	18·13	974·13	+13·6	46·405	−0·2
1993	Feb.	12·12	49030·12	+17·6	46·742	+0·4
		18·10	036·10	+15·3	·778	+0·5
	Mar.	18·04	064·04	−7·1	·947	−0·3
		23·06	069·06	−11·1	·977	−0·2
	Dec.	25·20	346·20	+20·3	48·648	0·0
		28·14	349·14	+19·6	·666	−0·4
1994	Jan.	1·22	49353·22	+18·8	48·690	−0·6
		5·15	357·15	+18·6	·714	0·0
	Feb.	19·10	402·10	−12·0	·985	−0·2
	Apr.	29·94	471·94	+13·9	49·406	0·0
	Dec.	13·16	699·16	+14·5	50·776	−0·5
		28·22	714·22	+5·6	·867	+0·4
1995	Jan.	2·23	49719·23	+0·3	50·897	−0·4
		3·09	720·09	−0·8	·903	−0·8
		10·24	727·24	−6·8	·946	−0·2
	June	1·91	869·91	+11·3	51·806	−1·1
1996	Jan.	1·15	50083·15	−14·1	53·092	−0·1
	Mar.	29·01	171·01	+20·7	·621	+0·3
	Dec.	15·23	432·23	−5·5	55·196	−1·3
1997	Jan.	23·10	50471·10	+14·7	55·431	−0·6
	Mar.	30·96†	537·96	+9·1	·834	−0·3
	Apr.	10·97†	548·97	+0·5	·900	+0·2
	May	9·93†	577·93	−15·1	56·075	−0·3
	Dec.	25·13	807·13	+16·8	57·457	+0·2
2000	Jan.	10·15	51553·15	−7·7	61·955	+0·3
	Feb.	20·09	594·09	−3·8	62·202	−0·2
	Apr.	6·93	640·93	+18·1	·484	+0·4
2001	Jan.	11·16	51920·16	−7·6	64·168	−0·4
	May	12·92	52041·92	+0·1	·902	+0·1
2002	Feb.	15·05	52320·05	+19·9	66·579	−0·3
	Mar.	27·02	360·02	+10·9	·820	0·0
	Apr.	5·96	369·96	+3·4	·880	+0·1
	May	5·94	399·94	−14·9	67·061	+0·3
2003	Jan.	11·15	52650·15	+20·2	68·570	+0·1
	Feb.	20·05	690·05	+12·4	·810	+0·4
	Mar.	16·98	714·98	−8·4	·961	+0·4
	May	18·92	777·92	+10·2:	69·340	+0·8
		25·91	784·91	+11·9	·382	−0·5
2004	May	12·96	53137·96	+19·0	71·511	+0·3

TABLE I (*concluded*)

Date (UT)		*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O – C)* $km\ s^{-1}$
2005 Jan.	19·21	53389·21	−15·0	73·026	−0·2
2006 Mar.	1·05	53795·05	+17·0	75·473	−0·3
2009 Feb.	4·18	54866·18	−4·5	81·932	0·0
Mar.	9·09	899·09	−10·2	82·130	+0·8
	26·99	916·99	+0·7	·238	+0·5
2010 June	3·91	55350·91	+6·7	84·854	−0·2
2012 May	27·91	56074·91	−1·1	89·220	+0·6
2013 Apr.	6·98	56388·98	−12·7	91·113	−0·3
	20·95	402·95	−4·1	·198	−0·1
May	4·98	416·98	+4·5	·282	0·0
	6·91	418·91	+5·5	·294	−0·1
2014 Jan.	5·20	56662·20	+16·6	92·761	+0·5

*Observed with Palomar 200-inch telescope; wt. ½
†Observed with Cambridge *Coravel*; weight 1

to such a precision as charted by the *Radial Velocity Catalogue* — and here we are referring to individual *Coravel* observations and not mean values as in the *Catalogue*. It may be recalled that, in describing the photoelectric method to a meeting of the Royal Astronomical Society in 1969, the writer[15] boldly asserted that the radial-velocity spectrometer gave a gain of 4000 (rather than 400) times over photography — and weathered some efforts by commentators from the floor of the meeting to dispute it! The factor of 4000 was in fact explicitly justified in the paper[16] that the speaker was ostensibly describing to the meeting in comparison with measurements made of the same stars, admittedly many years previously, by Redman (who was actually present at the RAS meeting and at that time was the speaker's Director); the new method offered ten times the accuracy in a tenth of the time on a telescope half the aperture used by Redman. The extra factor of ten, over the 400 proposed above, is easily accounted for by the difference in speed of observation, a matter not included in the comparison above; indeed, the photoelectric observations took only a few minutes each, whereas the photographic exposures with which they were being compared ran to an hour or more on a telescope of twice the aperture.

The orbital period of U 27° 10 (about 5½ months) is determined to an accuracy of nine minutes — it will be the best part of 100 years before it may be expected to get a whole day out of phase! The mass function is potentially significant, but what its significance actually *is* depends upon the mass attributed to the primary star, which cannot be estimated at all accurately since the region occupied by giants in the H–R Diagram collects objects evolving from quite a range of the main sequence. For primary masses of 1 and 2 $M_{\odot}$, the minimum masses for the companion star would be about 0·6 and 0·9 $M_{\odot}$, respectively — too faint in comparison with the primary, in either case, to lead to any expectation of visibility in the radial-velocity traces.

U 28° 12

This star is to be found a little over a degree directly following the 7^m star HD 102494 (U 28° 4), which, being a late-type star, is itself featured in the comprehensive paper[1] on such stars in the NGP field. U 28° 12 is bright enough to be plotted[17] in *Uranometria 2000·0*; it is considerably brighter than the one treated above, from which it is distant just under 1° in a north-following direction; it is in the *BD*[10] (as +28° 2056), which is probably what prompted its entry in *Simbad*. There it is listed with *Tycho*-derived magnitudes $V = 9^m{\cdot}18$, $B = 10^m{\cdot}30$, this time with quite small uncertainties of $0^m{\cdot}03$ and $0^m{\cdot}05$ respectively (the *Tycho* designation is 1987-165-1.) In fact U 28° 12 and U 29° 15 (treated next, below) are two of the brightest of the Upgren stars to lack entries in the *Henry Draper Catalogue*, which would have ensured that they would have featured in the radial-velocity survey already published[1] of the late-type stars in the NGP field. The Upgren spectral type is again K0 III, very consonant with the colour index. Upgren's is the one paper that appears in the *Simbad* bibliography. The unpublished photometry from the major Palomar campaign of 1976 gave a V magnitude of $9^m{\cdot}22$, $M_V = +1^m{\cdot}4$, [Fe/H] = −0·23, and a *res*(k) of $-0^m{\cdot}007$, so there is no indication of spectral compositeness.

Just as in the case of U 28° 12, after the initial radial-velocity measurement at Palomar in 1972 no further observations were made until 1989, when three OHP velocities were significantly discordant with the Palomar one. Renewed measurements in 1992 initially agreed with the Palomar one, but then exhibited a gradual change in the direction of, and eventually (in 1993–5) surpassing, the 1989 values. The observations were maintained, and have now reached the generous number of 87: in addition to the Palomar one, there are 24 obtained at OHP, 61 from the Cambridge *Coravel*, and one made with the DAO 48-inch telescope. The Cambridge ones have been subjected to an empirical adjustment of −0·3 km s^{-1} to bring them into systematic accord with the OHP ones; that is more or less in accord with the run of zero-point discrepancies between OHP

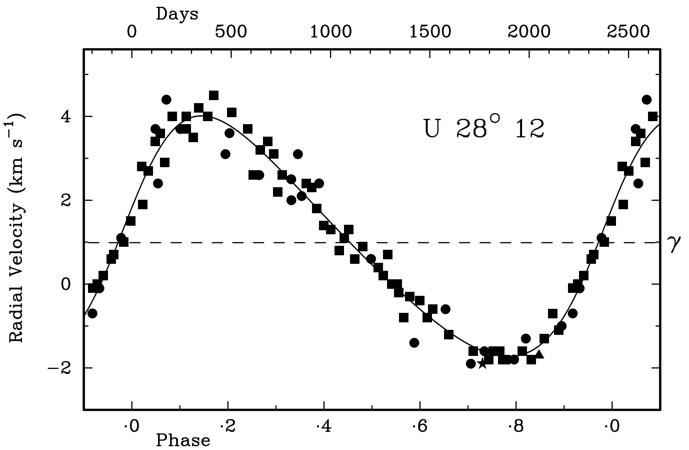

FIG. 2

As Fig. 1, but for U 28° 12. The sources of observations and their respective plotting symbols are the same as before, but here there is in addition an observation made at the DAO and plotted as a triangle.

TABLE II

Radial-velocity observations of U 28° 12

Except as noted, the sources of the observations are as follows:
1989–1998 — OHP Coravel (weight ½); 1999–2014 — Cambridge Coravel (weight 1)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1972 Jan. 7·48*	41323·48	−1·9	0·730	−0·3
1989 Mar. 28·09	47613·09	+2·5	3·332	0·0
28·90	613·90	+2·0	·332	−0·5
Apr. 30·91	646·91	+3·1	·346	+0·8
1992 Jan. 15·17	48636·17	−1·6	3·755	+0·1
Apr. 22·92	734·92	−1·8	·796	−0·1
June 20·88	793·88	−1·3	·820	+0·4
Dec. 18·13	974·13	−1·0	·895	−0·1
1993 Feb. 12·12	49030·12	−0·7	3·918	−0·3
Mar. 18·99	064·99	−0·1	·932	0·0
July 6·88	174·88	+1·1	·978	−0·1
Dec. 26·07	347·07	+3·7	4·049	+0·6
1994 Jan. 8·11	49360·11	+2·4	4·054	−0·8
Feb. 19·10	402·10	+4·4	·072	+0·9
Apr. 29·94	471·94	+3·7	·101	−0·1
Dec. 13·16	699·16	+3·1	·195	−0·7
1995 Jan. 3·10	49720·10	+3·6	4·203	−0·2
June 1·91	869·91	+2·6	·265	−0·6
1996 Jan. 1·15	50083·15	+2·1	4·354	−0·1
Mar. 29·01	171·01	+2·4	·390	+0·6
Dec. 15·23	432·23	+0·6	·498	+0·1
1997 Mar. 29·98†	50536·98	0·0	4·541	0·0
May 2·90†	570·90	−0·2	·555	−0·1
July 20·86	649·86	−1·4	·588	−0·9
Dec. 25·13	807·13	−0·6	·653	+0·5
1998 May 1·89	50934·89	−1·9	4·706	−0·4
July 8·86	51002·86	−1·6	·734	0·0
1999 Apr. 9·34‡	51277·34	−1·7	4·848	−0·2
Dec. 29·17	541·17	+0·6	·957	0·0
2000 Mar. 2·02	51605·02	+1·0	4·983	−0·3
Apr. 6·93	640·93	+1·5	·998	−0·3
June 6·91	701·91	+1·9	5·023	−0·6
2001 Jan. 11·17	51920·17	+3·7	5·113	−0·2
Feb. 14·05	954·05	+3·5	·127	−0·5
Mar. 14·03	982·03	+4·2	·139	+0·2
Apr. 28·96	52027·96	+4·0	·158	0·0
May 29·93	058·93	+4·5	·171	+0·5
Dec. 15·21	258·21	+2·6	·253	−0·7
2002 Jan. 18·15	52292·15	+3·2	5·267	0·0
Feb. 24·01	329·01	+3·4	·283	+0·4
Mar. 27·02	360·02	+3·1	·295	+0·2
Apr. 16·93	380·93	+2·2	·304	−0·6
May 7·93	401·93	+2·6	·313	−0·1
Dec. 5·24	613·24	+1·4	·400	−0·2

TABLE II (*concluded*)

Date (UT)			*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
2003	Jan.	7·20	52646·20	+1·3	5·414	−0·2
	Feb.	20·05	690·05	+0·8	·432	−0·5
	Mar.	16·04	714·04	+1·1	·442	0·0
	Apr.	7·95	736·95	+1·3	·451	+0·3
	May	7·91	766·91	+0·6	·464	−0·3
	June	18·91	808·91	+0·9	·481	+0·2
	Dec.	8·26	981·26	0·0	·552	+0·1
2004	Jan.	9·18	53013·18	−0·8	5·566	−0·5
	Feb.	9·14	044·14	−0·3	·578	+0·1
	Mar.	30·02	094·02	−0·4	·599	+0·2
	May	6·99	131·99	−0·8	·615	−0·1
	June	4·93	160·93	−0·6	·627	+0·3
	Dec.	26·24	365·24	−1·6	·711	−0·1
2005	Mar.	12·10	53441·10	−1·8	5·743	−0·2
	Apr.	3·96	463·96	−1·6	·752	+0·1
	May	7·92	497·92	−1·6	·766	+0·1
	June	6·93	527·93	−1·8	·778	−0·1
	Dec.	18·20	722·20	−1·3	·859	+0·1
2006	Jan.	29·15	53764·15	−0·7	5·876	+0·5
	Mar.	1·04	795·04	−1·1	·889	−0·1
	May	10·97	865·97	−0·1	·918	+0·3
	June	3·95	889·95	0·0	·928	+0·2
2007	Jan.	14·21	54114·21	+2·8	6·021	+0·4
	Feb.	15·10	146·10	+2·7	·034	0·0
	Mar.	22·04	181·04	+3·4	·049	+0·3
	Apr.	15·95	205·95	+3·6	·059	+0·3
	June	15·93	266·93	+4·0	·084	+0·3
2008	Apr.	10·97	54566·97	+4·1	6·208	+0·4
	June	30·93	647·93	+3·7	·242	+0·2
2009	Apr.	19·96	54940·96	+2·4	6·363	+0·3
	May	17·92	968·92	+2·3	·375	+0·4
	June	11·93	993·93	+1·8	·385	0·0
2010	Apr.	17·94	55303·94	+0·4	6·513	+0·1
	May	12·93	328·93	+0·2	·523	0·0
	June	3·90	350·90	+0·7	·533	+0·6
2011	Apr.	6·99	55657·99	−1·2	6·660	−0·1
2012	Jan.	4·21	55930·21	−1·8	6·772	−0·1
	Apr.	10·97	56027·97	−1·6	·813	+0·1
	May	26·91	073·91	−1·8	·832	−0·2
2013	Feb.	15·08	56338·08	+0·2	6·941	0·0
	Apr.	6·99	388·99	+0·7	·962	0·0
	Dec.	20·26	646·26	+2·9	7·068	−0·5
2014	Apr.	8·01	56755·01	+4·0	7·113	+0·1

*Observed with Palomar 200-inch telescope; wt. ½
†Observed with Cambridge *Coravel*; weight 1
‡Observed with DAO 48-inch telescope; wt. ½

and Cambridge velocities documented in a repetition, after more than 40 years, of the 'Redman K-star' programme[18]. All the velocities are set out in Table II, and readily lead to the orbit that is portrayed in Fig. 2 and whose elements are given here; the Cambridge observations have been given full weight and all the others half-weight.

P	$= 2418 \pm 7$ days	$(T)_5$	$=$ MJD 51646 ± 30
γ	$= +0.99 \pm 0.04$ km s^{-1}	$a_1 \sin i$	$= 92.9 \pm 1.8$ Gm
K	$= 2.87 \pm 0.05$ km s^{-1}	$f(m)$	$= 0.0055 \pm 0.0003\ M_\odot$
e	$= 0.227 \pm 0.017$		
ω	$= 284 \pm 5$ degrees	R.m.s. residual (wt. 1)	$= 0.30$ km s^{-1}

The orbit is seen to be quite well determined; its period of about 6·6 years is not near an integral number of years, so seasonal gaps do not coincide in successive cycles, and systematic observations cover four cycles (even discounting the early Palomar measure that extends the time base to some 6½ cycles). Despite the very modest radial-velocity amplitude, the period is determined to a 1-σ accuracy of only a week.

U 29° 15

U 29° 15, which may owe its entry in *Simbad* to its listing in the *BD* (as +29° 2229), is to be found little more than 1° north-following U 28° 12, the star treated above; only about 23′ further in that direction is the 7^{m} K star HD 103660, which features in the Yoss–Griffin survey[1] and is an Upgren[2] star in its own right, U 29° 21. As noted in the section above, U 29° 15 is one of the brightest of the Upgren stars to lack an entry in the *Henry Draper Catalogue*.

Photometry of U 29° 15 by Oja[19] gave $V = 9^{m}{\cdot}31$, $(B-V) = 1^{m}{\cdot}28$, $(U-B) = 1^{m}{\cdot}40$, but in its main heading material for the star, *Simbad* gives (without any reference, but possibly transformed from *Tycho 2* magnitudes) $V = 9^{m}{\cdot}28$, $B = 10^{m}{\cdot}64$, implying $(B-V) = 1^{m}{\cdot}36$, in fair agreement with the Upgren type of K2 III. The object is one of only five Upgren stars to appear in the eventual photometric listing[7] stemming from the 1975 Kitt Peak work of Hansen & Radford, although that paper is not retrieved for any of those five in their respective *Simbad* bibliographies. The V magnitude is shown there as $9^{m}{\cdot}30$, M_V as $+0^{m}{\cdot}3$, and [Fe/H] as –0·14. There is no *res*(k) value, as there is no entry for the k photometric quantity upon which it would rest. The corresponding quantities from the Palomar narrow-band photometry are $V = 9^{m}{\cdot}30$ (agreeing exactly with Kitt Peak), $M_V = +1^{m}{\cdot}5$, [Fe/H] = +0·05; and *res*(k) was found to be $0^{m}{\cdot}018$, not large enough to indicate significant compositeness of the spectrum.

The star features in the *Bergedorfer Spektral-Durchmusterung*[20] (*BSD*) where it is classified "d:: K2". The Upgren[2] type is also K2, but (as noted above) the luminosity class is III; the narrow-band photometry both from Kitt Peak and from Palomar provides confirmation that the star is evolved, though perhaps of luminosity class III–IV rather than III. For such a luminosity, the colour index would suggest a type somewhat later than K2.

As in the cases of the stars already discussed above, there was a long hiatus after the initial Palomar radial-velocity measurement in 1972, until the next observation was made at OHP in 1989. There are now 79 velocities altogether, listed in Table III; the main sources are as usual the Cambridge and OHP *Coravel*s (51 and 23, respectively), and there are also the Palomar one, one

TABLE III

Radial-velocity observations of U 29° 15

Except as noted, the sources of the observations are as follows:
1989–1998 — OHP Coravel (weight ½); 1999–2015 — Cambridge Coravel (weight 1)

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
1972 Jan. 7·58*	41323·58	+12·5	0·656	+1·2
1989 Mar. 30·07	47615·07	+9·1	1·840	+0·1
1990 Feb. 15·27†	47937·27	+5·6	1·901	−0·1
1991 Apr. 3·99‡	48349·99	−1·1	1·979	−0·2
May 2·89‡	378·89	−1·2	·984	+0·2
Dec. 17·22	607·22	−4·2	2·027	−0·3
1992 Jan. 14·19	48635·19	−3·8	2·033	+0·2
Apr. 21·94	733·94	−4·2	·051	+0·1
June 20·89	793·89	−3·9	·062	+0·4
Dec. 18·13	974·13	−3·3	·096	+0·3
1993 Feb. 12·12	49030·12	−3·5	2·107	−0·2
Mar. 18·99	064·99	−2·7	·114	+0·3
July 6·88	174·88	−2·0	·134	+0·2
Dec. 26·08	347·08	−0·5	·167	+0·2
1994 Jan. 8·12	49360·12	−0·3	2·169	+0·3
Feb. 19·10	402·10	+0·2	·177	+0·4
Apr. 29·94	471·94	+0·1	·190	−0·2
Dec. 13·16	699·16	+1·2	·233	−0·9
1995 Jan. 3·11	49720·11	+1·8	2·237	−0·5
June 1·91	869·91	+2·6	·265	−0·8
1996 Jan. 1·15	50083·15	+4·2	2·305	−0·6
Mar. 29·01	171·01	+5·3	·322	0·0
Nov. 21·25§	408·25	+7·0	·367	+0·4
Dec. 16·15	433·15	+6·7	·371	0·0
1997 Mar. 30·97§	50537·97	+7·0	2·391	−0·2
May 2·90§	570·90	+7·7	·397	+0·3
July 20·87	649·87	+8·3	·412	+0·5
Dec. 25·13	807·13	+9·0	·442	+0·6
1998 Apr. 28·98	50931·98	+9·0	2·465	+0·1
July 8·86	51002·86	+9·2	·478	+0·1
1999 Apr. 9·33¶	51277·33	+10·0	2·530	0·0
Dec. 29·17	541·17	+10·8	·580	+0·1
2000 Feb. 14·04	51588·04	+10·9	2·589	+0·1
Apr. 6·94	640·94	+11·3	·599	+0·4
June 6·91	701·91	+10·6	·610	−0·4
2001 Jan. 14·15	51923·15	+11·0	2·652	−0·3
Mar. 14·04	982·04	+11·2	·663	−0·1
May 4·96	52033·96	+11·6	·673	+0·3
Dec. 15·22	258·22	+10·8	·715	−0·5

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2002 Jan. 18·15	52292·15	+11·3	2·721	0·0
Mar. 1·06	334·06	+10·6	·729	−0·7
May 1·96	395·96	+10·9	·741	−0·3
Dec. 5·24	613·24	+10·2	·782	−0·5
2003 Feb. 15·10	52685·10	+10·3	2·795	−0·1
Apr. 4·94	733·94	+10·4	·804	+0·2
June 18·91	808·91	+10·0	·819	+0·2
Dec. 8·27	981·27	+8·6	·851	0·0
2004 Jan. 9·18	53013·18	+8·3	2·857	0·0
Mar. 17·12	081·12	+8·1	·870	+0·4
May 6·99	131·99	+7·0	·879	−0·1
July 6·90	192·90	+6·8	·891	+0·4
Dec. 26·24	365·24	+4·0	·923	0·0
2005 Mar. 12·10	53441·10	+2·9	2·938	+0·2
May 2·98	492·98	+1·7	·947	−0·2
June 6·93	527·93	+1·2	·954	−0·1
Dec. 18·21	722·21	−1·6	·991	+0·3
2006 Jan. 29·16	53764·16	−2·6	2·999	−0·2
Mar. 1·05	795·05	−3·2	3·004	−0·4
Apr. 4·04	829·04	−3·1	·011	+0·1
May 10·97	865·97	−3·6	·018	−0·1
June 3·95	889·95	−3·8	·022	−0·1
2007 Feb. 3·13	54134·13	−4·4	3·068	−0·1
May 5·95	225·95	−3·7	·085	+0·2
2008 May 14·91	54600·91	−1·2	3·156	0·0
2009 Feb. 11·10	54873·10	+1·1	3·207	0·0
May 6·94	957·94	+1·9	·223	+0·1
Dec. 21·24	55186·24	+3·5	·266	+0·1
2010 Mar. 5·08	55260·08	+3·3	3·280	−0·6
June 3·90	350·90	+4·4	·297	−0·1
2011 Jan. 19·19	55580·19	+6·0	3·341	+0·1
2012 Feb. 19·10	55976·10	+7·5	3·415	−0·3
May 14·93	56061·93	+8·8	·431	+0·6
2013 Feb. 7·16	56330·16	+9·1	3·482	−0·1
Apr. 6·99	388·99	+9·5	·493	+0·1
June 2·93	445·93	+9·5	·504	−0·1
Dec. 20·26	646·26	+10·2	·541	0·0
2014 Mar. 5·05	56721·05	+10·4	3·555	0·0
Apr. 8·01	755·01	+10·7	·562	+0·2
2015 Apr. 6·01	57118·01	+11·6	3·630	+0·5

*Observed with Palomar 200-inch telescope; wt. 0
†Observed with ESO *Coravel*; weight ½
‡Observed with original spectrometer; weight ½
§Observed with Cambridge *Coravel*; weight 1
¶Observed with DAO 48-inch telescope; wt. ½

each from ESO and the DAO, and two made with the original Cambridge spectrometer. All have been given half-weight in the solution of the orbit apart from the Cambridge *Coravel* ones, which have full weight, and the Palomar one, which rather embarrassingly has the largest residual of all and has been rejected. (If in the future U 29° 15 proves to be a triple system, the writer may appear to have been too hasty with his rejection!) The consistency of the OHP zero-point being trusted as before, the Cambridge *Coravel* velocities have been adjusted by −0·3 km s^{-1} from the values given by the original reductions. The orbit has a relatively long period of 14½ years, but is well covered by the available observations; it is plotted in Fig. 3 and has the following elements:

P	= 5311 ± 11 days	$(T)_3$	= MJD 53772 ± 16
γ	= +5·47 ± 0·04 km s^{-1}	$a_1 \sin i$	= 542 ± 4 Gm
K	= 7·86 ± 0·06 km s^{-1}	$f(m)$	= 0·226 ± 0·005 $M_\odot$
e	= 0·326 ± 0·006		
ω	= 140·0 ± 1·2 degrees	R.m.s. residual (wt. 1)	= 0·27 km s^{-1}

The mass function is large enough to require the secondary to be a star of significant mass, that might be expected either to be visible in radial-velocity traces in its own right (if it were a star with a colour index comparable with that of the primary) or else to produce a very significant value for the Copenhagen *res*(k) (if it were not). For primary masses of 1 and 2 $M_\odot$, the mass function demands that the secondary has a mass of at least 0·96 or 1·38 $M_\odot$, respectively, corresponding to main-sequence spectral types of about G3 and F4. Secondaries close to those lower limits would have absolute magnitudes of about 4·4 and 3·2, respectively, and especially in the former case might escape detection either in radial-velocity traces or through a *res*(k) anomaly. There is also plenty of scope, particularly in view of the long orbital period, for the

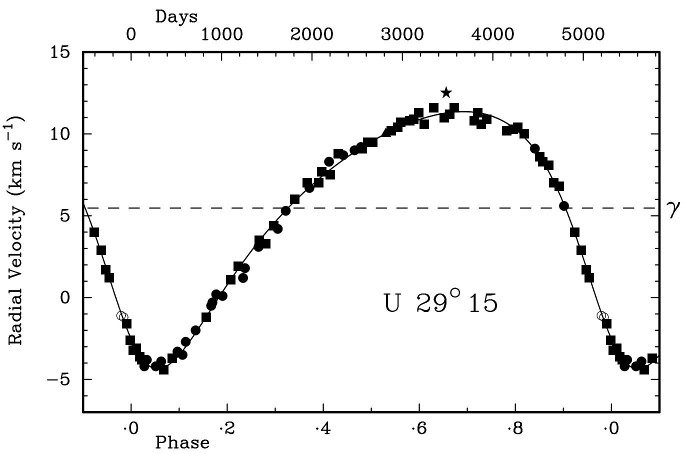

FIG. 3

As Fig. 2, but for U 29° 15. In addition to measures from the same sources as before, there is one from ESO (plotted as an honorary OHP one) and two that were made with the original radial-velocity spectrometer in Cambridge and appear as open circles. The Palomar observation (star symbol) has been rejected.

secondary to be itself a double star with a relatively negligible total luminosity in comparison with a single star of the requisite mass.

U 28° 32

This is another star that cannot be traced through *Simbad* under its Upgren designation but must first be identified, through its coördinates, with a designation that is present in the *Simbad* bibliography, in this case *Tycho* 1988-77-1; the one reference that is retrieved for it is its entry in the *BSD*[20]. It is to be found in the sky a little over 20′ south-preceding the 8^{m} K giant HD 105020 (itself an NGP Survey[1] star and an Upgren[2] star, U 29° 41), which some years ago the present writer had occasion (however uncharacteristically) to defend[21] in this *Magazine* against assertions that it was a short-period eclipsing system, and also that it showed variations (though of a different period) of radial velocity and even spectral type — which it isn't and doesn't.

The *Tycho 2* photometry is transformed by *Simbad* to give $V = 11^{m}{\cdot}27$, $B = 12^{m}{\cdot}68$, but the uncertainties of $0^{m}{\cdot}09$ and $0^{m}{\cdot}28$, respectively, are very considerable, so the implied colour index of $1^{m}{\cdot}41$ could not be said to be in actual conflict with the Upgren type of K0 III. From the Palomar photometry, the V magnitude is $11^{m}{\cdot}36$, M_V $+1^{m}{\cdot}7$, [Fe/H] −0·18, and *res*(*k*) −0·038 — an unusually high negative value, but no specific significance seems to be attributable to negative values.

After the first observation had been made at Palomar in 1972, the radial velocity of the star was not measured again until 1989, when the result was so discordant with the Palomar one that the observation was repeated the very next night, just in case there had been a mistake of some sort, for instance of identification — which in fact there had not. There are now 55 measurements altogether — 34 from the Cambridge *Coravel* and 20 from the OHP one, plus the original Palomar velocity. They are set out in Table IV. The Cambridge observations have again been given an empirical adjustment of −0·3 km s^{-1} to put them into systematic accord with the OHP ones. The OHP and Palomar velocities have been half-weighted; in this case the Palomar residual is small, but it is still not a good idea to give too much weight to an isolated observation. The resulting orbit is shown in Fig. 4 and has elements as follows:

P	$= 554{\cdot}87 \pm 0{\cdot}33$ days	$(T)_{21}$	= MJD 52888·5 ± 1·7
γ	$= -21{\cdot}13 \pm 0{\cdot}08$ km s^{-1}	$a_1 \sin i$	$= 45{\cdot}0 \pm 0{\cdot}8$ Gm
K	$= 5{\cdot}89 \pm 0{\cdot}11$ km s^{-1}	$f(m)$	$= 0{\cdot}0118 \pm 0{\cdot}0007\ M_\odot$
e	$\equiv 0$ (fixed)		
ω	is undefined in a circular orbit	R.m.s. residual (wt. 1)	$= 0{\cdot}50$ km s^{-1}

If the eccentricity is first allowed as a free parameter, it takes the value $0{\cdot}040 \pm 0{\cdot}019$, with $\omega = 71 \pm 26$ degrees; the sum of squares of the deviations falls to 12·59 (km s^{-1})^2, to be compared with the 13·76 given by the solution above with the eccentricity fixed at zero. Bassett's[22] second statistical test compares the cost, of 12·59 (km s^{-1})^2, of the 49 degrees of freedom remaining after the eccentric solution has fitted six unknowns to 55 observations (0·257 (km s^{-1})^2 per degree) with the cost of 1·17 (km s^{-1})^2 incurred by the gain of two extra degrees of freedom (0·585 per degree) when e is fixed. The ratio of 2·28 represents the F-test ratio, $F_{2,49}$, of Bassett's test; tables[23] of F show that it is a little short of the 10%-significance level (2·42), so we here elect to consider it 'not significant' and accordingly have chosen the circular solution for the elements above.

TABLE IV

Radial-velocity observations of U 28° 32

Except as noted, the sources of the observations are as follows:
1989–1997 — OHP Coravel (weight ½); 2000–2015 — Cambridge Coravel (weight 1)

Date (UT)	MJD	Velocity km s⁻¹	Phase	(O – C) km s⁻¹
1972 Jan. 7·50*	41323·50	−18·1	0·157	−0·2
1989 Mar. 28·09	47613·09	−26·1	11·493	+0·9
28·90	613·90	−27·8	·494	−0·8
Apr. 30·92	646·92	−28·0	·554	−1·3
1992 Jan. 15·18	48636·18	−24·5	13·336	−0·3
Apr. 22·92	734·92	−26·4	·514	+0·6
June 20·90	793·90	−24·5	·621	+0·9
Dec. 18·14	974·14	−16·3	·946	−0·7
1993 Feb. 13·05	49031·05	−16·0	14·048	−0·5
Mar. 19·00	065·00	−15·7	·109	+0·9
July 7·88	175·88	−24·6	·309	−1·3
Dec. 26·15	347·15	−26·3	·618	−0·8
1994 Jan. 8·12	49360·12	−24·0	14·641	+0·9
Feb. 19·13	402·13	−22·1	·717	+0·3
Apr. 30·92	472·92	−17·7	·844	+0·1
1995 Jan. 3·11	49720·11	−22·2	15·290	+0·4
June 1·95	869·95	−25·9	·560	+0·7
1996 Jan. 1·16	50083·16	−16·1	15·944	−0·5
Mar. 30·03	172·03	−15·8	16·104	+0·7
Dec. 16·16	433·16	−26·1	·575	+0·3
1997 Apr. 10·98†	50548·98	−20·1	16·784	−0·2
Dec. 25·14	807·14	−21·3	17·249	−0·2
2000 Jan. 10·17	51553·17	−26·3	18·594	−0·3
Apr. 6·94	640·94	−20·9	·752	+0·2
May 7·95	671·95	−18·4	·808	+0·6
2001 Jan. 11·17	51920·17	−21·1	19·255	+0·2
May 7·97	52036·97	−27·3	·465	−0·4
2002 Mar. 27·03	52360·03	−13·9	20·048	+1·6
May 28·97	422·97	−18·3	·161	−0·3
2003 Feb. 15·11	52685·11	−25·4	20·634	−0·3
Mar. 23·99	721·99	−23·0	·700	0·0
Apr. 7·96	736·96	−22·4	·727	−0·4
May 19·93	778·93	−20·2	·803	−1·0
Dec. 27·26	53000·26	−18·9	21·201	+0·5
2004 Jan. 9·19	53013·19	−20·5	21·225	−0·3
Mar. 30·02	094·02	−25·5	·370	−0·3
Apr. 22·04	117·04	−26·1	·412	0·0
May 18·94	143·94	−27·2	·460	−0·4
2005 Jan. 22·19	53392·19	−16·0	21·908	+0·2
Apr. 3·96	463·96	−15·8	22·037	−0·4
May 11·97	501·97	−15·9	·106	+0·6
2009 Jan. 21·18	54852·18	−26·4	24·539	+0·4

TABLE IV (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2010 May 11·90	55327·90	−25·6	25·396	+0·2
June 3·93	350·93	−26·7	·438	−0·1
2011 Apr. 9·00	55660·00	−15·7	25·995	−0·5
2012 Feb. 19·12	55976·12	−26·1	26·565	+0·4
Apr. 15·97	56032·97	−23·3	·667	+0·8
May 26·92	073·92	−21·6	·741	−0·1
2013 Feb. 15·09	56338·09	−20·2	27·217	−0·3
2014 Jan. 5·24	56662·24	−19·9	27·801	−0·6
Feb. 21·13	709·13	−16·9	·886	−0·2
Mar. 10·06	726·06	−16·2	·916	−0·2
Apr. 8·03	755·03	−14·8	·968	+0·6
May 30·94	807·94	−16·2	28·064	−0·5
2015 Apr. 20·98	57132·98	−24·1	28·650	+0·5

*Observed with Palomar 200-inch telescope; wt. ½
†Observed with Cambridge *Coravel*; weight 1

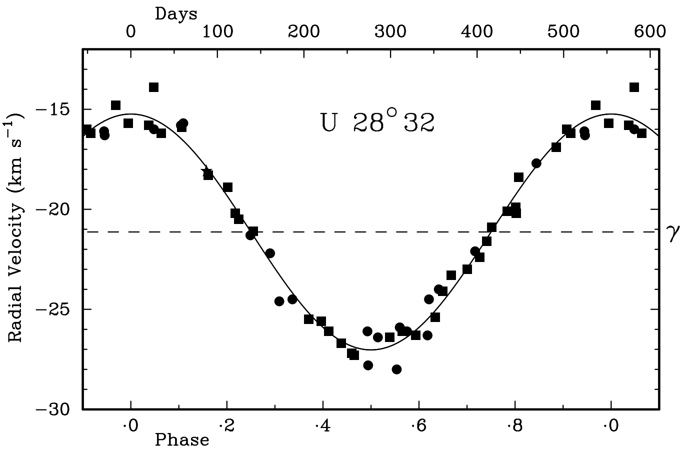

FIG. 4

The observed radial velocities of U 28° 32 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The plotting conventions are the same as in the previous diagrams.

U 29° 56

This is one of the faintest of the Upgren stars, too faint to be included in either the *Tycho* or the Palomar photometry, but given as $m_{pg} = 13^{m}{\cdot}1$ by Upgren[2], whose magnitudes tend, however, to be on the pessimistic side. The only reference found for it in *Simbad* is that of *An astrometric catalogue for the area of Coma Berenices*, by Abad & Vicente[24]; it gives an "estimated photographic magnitude" of $11^{m}\cdot40$, which doubtless errs in the opposite direction. The object is to be found about 16′ north-preceding the $6\frac{1}{2}^{m}$ F5 star HR 4642.

The very first radial-velocity observation, made (as in so many cases) at Palomar in 1972, revealed U 29° 56 to be widely double-lined, with apparently equal components about 50 km s^{-1} apart. Regrettably, the observation was reduced only in the normal fashion as if it were a single-lined one, so only one radial velocity was obtained, and it is only now that that oversight has come to attention. Both the computer and the software that used to perform the Palomar reductions have long been obsolete, and it would be a substantial (and scarcely sufficiently rewarding) task now to try to reduce the trace anew in order to determine the velocity of the other component.

The Upgren spectral type is K0 III, but must be in error — the system must really be a pair of stars that are on or near the main sequence. That was fairly certain as soon as the system was found to be widely double-lined in 1972, since few giant pairs could be in orbits small enough to have such large velocity amplitudes. That conclusion was reinforced as soon as the orbit was found to be of quite short period and quite high eccentricity. Still further evidence in favour of it appeared much more recently, when the annual proper motion was determined[24] at about $0''\cdot05$: it is much too great to belong to a system of giants whose distance modulus would have to be something like 12 magnitudes ($d \sim 2500$ pc), at which it would represent a tangential velocity of about 600 km s^{-1}.

Not until 1989 was the system re-observed, at OHP; initially only one component was noticed, but after that the system was regularly observed as a double-lined binary. It was quite a difficult object at OHP, and more so at Cambridge, where* it has usually been right on the margin of observability, with photon counting rates only of the order of 50 per second at best, inclusive of dark count. Furthermore, the double-lined nature of the spectrum splits the radial-velocity 'dip' into two, so both dips are quite shallow. The trace shown in Fig. 5, which represents about 40 minutes' integration on a good night, is about as good as it is possible to obtain at Cambridge. For such objects, the loss of the use, that the writer formerly enjoyed, of the 200-inch telescope has been keenly felt!

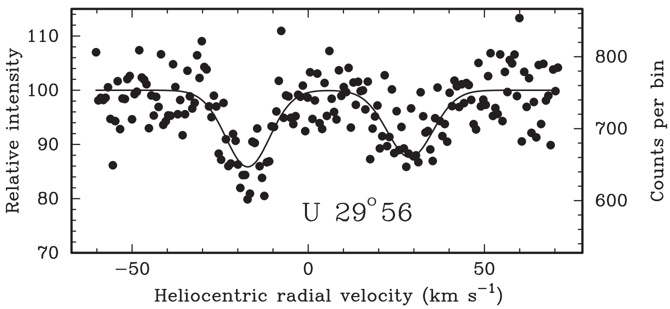

FIG. 5

Radial-velocity trace of the faint double-lined system U 29° 56, obtained with the Cambridge *Coravel* on 2013 April 5.

* Owing to a somewhat smaller telescope; more complicated optical system with five reflections before the focus, therefore smaller overall transmission than the OHP normal Cassegrain; and 8° higher latitude, putting the object that much lower in the sky.

Altogether, 49 radial-velocity measurements have been made of U 29°56 — 27 at OHP, one at ESO, and 20 at Cambridge, plus the one at Palomar. Three of the OHP observations and two of the Cambridge ones were close blends reduced as single-lined and cannot contribute to the solution of the orbit; and only one of the components' velocities was determined from the trace made at Palomar and from the first OHP one. There are, however, measures of both components at 42 epochs, 24 from OHP, and 18 from Cambridge. All the data are entered in Table V. As usual, the OHP observations have been increased by 0·8 km s^{-1} from the values determined from the reductions performed in Geneva; the Cambridge ones have been adjusted by –1·0 km s^{-1} from the 'as initially reduced' velocities, an amount assessed to make them more homogeneous with the OHP ones. That could not be done with any great certainty, because separate assessments of the offsets of the individual components differed by about 1·6 km s^{-1}. The adjustment of –1·0 km s^{-1} chosen is already greater than the corresponding quantity that is needed for most stars*, and in fact approximates to the one needed for the star that is regarded as the primary here, leaving an unpleasant average discrepancy, whose removal would require a further offset of more than 1·5 km s^{-1} in the same sense, between the residuals from OHP and from Cambridge for the other component. For that component, therefore, the mathematically ideal offset would be that much greater — an amount that the writer declines to countenance. It is, of course, only to be expected that the raggedness of the observed radial-velocity traces, of which Fig. 5 is actually one of the best, will be reflected in the raggedness of the radial velocities themselves, which indeed have r.m.s. residuals of about 1·7 km s^{-1} from the adopted orbital solution. That solution, in which all the data have been accorded equal weight apart from two Cambridge observations which were curtailed by cloud and have been half-weighted, is portrayed in Fig. 6, where the quite unusually poor observational accuracy is seen to be adequate in relation to the amplitudes of the velocity variations. In fact the amplitudes are determined to about 1½%, quite as good a proportional accuracy as for U 28°12 and U 28°32 treated above, where no apology for poor precision was thought needful. Furthermore, the reader may be inclined to agree that the systematic discrepancy noticed by the computer between the residuals given by the two sources for the secondary component of U 29°56 does not by any means jump out of the page at sight. The orbital elements are as follows:

P	= 94·4384 ± 0·0039 days	T	= MJD 50795·81 ± 0·27
γ	= +3·59 ± 0·20 km s^{-1}	$a_1 \sin i$	= 35·5 ± 0·6 Gm
K_1	= 31·7 ± 0·5 km s^{-1}	$a_2 \sin i$	= 36·5 ± 0·7 Gm
K_2	= 32·5 ± 0·5 km s^{-1}	$f(m_1)$	= 0·201 ± 0·011 $M_\odot$
q	= 1·027 ± 0·024 (= m_1/m_2)	$f(m_2)$	= 0·217 ± 0·012 $M_\odot$
e	= 0·505 ± 0·010	$m_1 \sin^3 i$	= 0·85 ± 0·04 $M_\odot$
ω	= 98·2 ± 1·6 degrees	$m_2 \sin^3 i$	= 0·82 ± 0·04 $M_\odot$

R.m.s. residual (wt. 1) = 1·68 km s^{-1}

*See ref. 18, Fig. 1, where discrepancies between OHP and Cambridge are plotted for a lot of stars as a function of colour index (a quantity that is not known for the star of interest here but may be expected to be near $+1^{m}\cdot 0$). It should be noticed that the ordinate of the figure is the whole offset between OHP and Cambridge, so in the case here, where OHP has been adjusted by +0·8 km s^{-1} and Cambridge by –1·0, the total offset is already –1·8 km s^{-1} — off the bottom of the scale of the figure.

TABLE V

Radial-velocity observations of U 29° 56

Except as noted, the sources of the observations are as follows:
1989–1998 — Haute-Provence Coravel ; 2003–2013 — Cambridge Coravel
All the observations (except colon = ½) have the same weight

Date (UT)	*MJD*	*Velocity Prim. km s⁻¹*	*Velocity Sec. km s⁻¹*	*Phase*	*(O–C) Prim. km s⁻¹*	*(O–C) Sec. km s⁻¹*
1972 Jan. 28·47*	41344·47	+32·8	—	0·921	+1·6	—
1989 Feb. 24·22	47581·22	+18·4	—	66·961	–2·3	—
Mar. 25·93	610·93	–13·0	+19·1	67·275	–0·3	–1·2
May 2·97	648·97	+18·6	–10·5	·678	–0·6	+1·9
1990 Jan. 31·06	47922·06	+3·6		70·570	—	—
Feb. 12·28†	934·28	+19·8	–15·5	·699	–1·0	–1·4
1991 Dec. 17·23	48607·23	+30·8	–21·7	77·825	+0·5	+2·2
1992 Jan. 14·21	48635·21	–29·7	+37·6	78·122	–1·4	+1·2
16·16	637·16	–24·4	+33·9	·142	+2·0	–0·5
Apr. 21·96	733·96	–23·6	+32·2	79·167	+0·1	+0·5
23·97	735·97	–23·2	+28·8	·188	–1·7	–0·5
30·94	742·94	–15·4	+21·2	·262	–1·5	–0·4
Dec. 18·14	974·14	+20·3	–17·6	81·710	–1·4	–2·6
19·16	975·16	+24·1	–20·7	·721	+1·6	–4·9
1993 Feb. 13·02	49031·02	–9·0	+18·1	82·313	+0·3	+1·3
14·07	032·07	–9·3	+15·1	·324	–1·0	–0·7
19·04	037·04	+1·0		·376	—	—
Mar. 18·06	064·06	+16·3	–12·0	·663	–1·7	–0·8
20·08	066·08	+17·4	–14·9	·684	–2·2	–2·0
Dec. 25·21	346·21	+19·2	–7·0	85·650	+2·2	+3·2
1994 Jan. 5·16	49357·16	+25·9	–20·7	85·766	–0·1	–1·3
Feb. 21·04	404·04	–13·1	+23·2	86·263	+0·8	+1·7
Dec. 12·18	698·18	–4·7	+11·8	89·377	–1·0	+0·7
1995 Jan. 7·13	49724·13	+14·7	–9·8	89·652	–2·5	+0·6
June 6·91	874·91	–13·1	+24·7	91·249	+2·2	+1·7
1996 Dec. 15·24	50432·24	–27·8	+31·4	97·150	–2·3	–2·1
16·16	433·16	–23·9	+30·5	·160	+0·6	–2·0
25·22	442·22	–16·5	+20·7	·256	–1·9	–1·5
1998 May 1·92	50934·92	+3·8		102·473	—	—
2003 Feb. 15·16	52685·16	–7·7	+15·6	121·006	–0·2	+0·6
21·04	691·04	–29·0	+39·9	·069	+0·9	+1·9
Mar. 1·16	699·16	–25·4	+35·0	·155	–0·3	+2·0
3·07	701·07	–22·7	+29·9	·175	+0·2	–0·9
16·09	714·09	–6·6	+16·5	·313	+2·7	–0·3
23·06	721·06	–3·4	+13·2	·386	–0·4	+2·9
31·96	729·96	+3·4		·481	—	—
Apr. 4·94	733·94	+11·5:	–0·3:	·523	+4·0	+0·2
8·93	737·93	+12·8	–3·1	·565	+2·1	+0·6
19·03	748·03	+18·7	–11·3	·672	0·0	+0·6
29·03	758·03	+25·3	–21·0	·778	–1·6	–0·7
May 7·92	766·92	+32·5	–20·9	·872	–0·2	+5·5
9·95	768·95	+35·2:	–29·1:	·894	+2·2	–2·5
17·97	776·97	+13·7	–3·7	·978	+2·4	+0·7
25·95	784·95	–29·2	+35·9	122·063	+0·2	–1·6

TABLE V (*concluded*)

Date (UT)	*MJD*	*Velocity* *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*	*Phase*	*(O – C)* *Prim.* *km s⁻¹*	*Sec.* *km s⁻¹*
2010 May 12·94	55328·94	−6·3	+9·1	149·001	−2·3	−2·3
2013 Mar. 31·01	56382·01	−24·2	+33·8	160·152	+1·2	+0·5
Apr. 5·99	387·99	−18·3	+27·7	·215	+0·3	+1·3
May 3·92	415·92	+4·6		·511	—	—
Dec. 28·25	654·25	−23·4	+31·0	163·035	−0·5	+0·2

*Observed with Palomar 200-inch telescope
†Observed with ESO *Coravel*

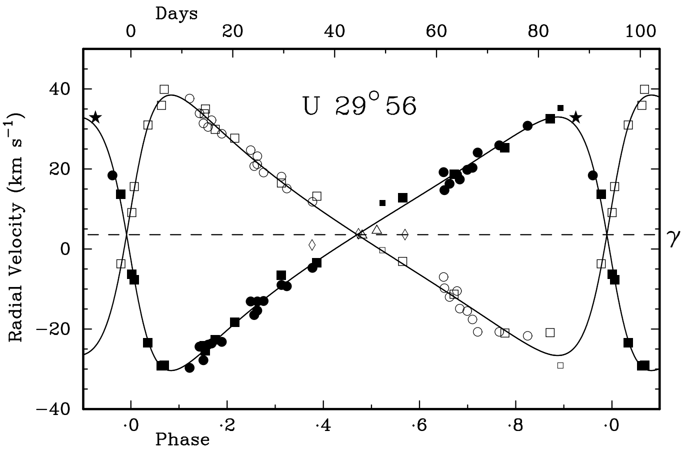

FIG. 6

The observed radial velocities of U 29° 56 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. The plotting conventions are mostly the same as in the previous diagrams, but in this case the measurements of the secondary component are distinguished by being plotted with open symbols. The two open triangles near phase ·5, however, represent measurements made with the Cambridge *Coravel* and reduced as if the system were single-lined, at times when the two dips were hopelessly blended together; three open diamonds in the same vicinity refer to analogous measures made with the OHP instrument.

The period is determined to an accuracy of better than 6 minutes; it will take more than 250 cycles — over 60 years — before the phasing uncertainty reaches a whole day, and well over a thousand years before there could be any ambiguity in the cycle count to renewed observations.

The minimum masses, $m_{1,2} \sin^3 i$, of the two stars are seen to be at least as great as the masses that early-K main-sequence stars are expected to possess, so the orbital inclination must be high. If we accordingly take $\sin^3 i \sim 1$, then the elements above show that major semi-axis of the relative orbit, $a_1 + a_2$, is some 70 Gm. One of the conjunctions occurs very close to periastron (at phase ·9935), when the separation of the stars will be reduced by a factor of $(1 - e)$, *i.e.*, halved, to about 35 Gm. Since the sum of the radii of the stars can be expected to be about 1·2 Gm there will be eclipses if the inclination is within about 2° of 90°. The probability of that is by no means negligible, so it would certainly be of interest to watch the system photometrically for eclipses at one of the times of conjunction, whose times are readily determined from the orbital elements to be at MJD ($T + nP - 0{\cdot}61$), where n is any integer and the numerical quantity –0·61 days represents the offset of conjunction from periastron, corresponding to the phase ·9935.

U 25° 40

U 25° 40 is a faint star that is to be found just over 1° south from, and 11 seconds of time following, the 5½^{m} K4 III star 4 Comae (U 26° 58). 4 Comae is itself a spectroscopic binary, whose orbit was given by the industrious Victoria astronomer Harper[25] before the present writer was born. Although no particular effort has been made to follow its velocity variations in the course of the writer's NGP programme, it seems now that the occasional observations that have been made of it simply as an NGP star do permit a considerable improvement to the orbit, which is presented as a sort of addendum to this present paper, and moreover that system has newly been recognized as double-lined.

The *only* paper that is retrieved by *Simbad* for U 25° 40 itself is the astrometric one by Abad & Vicente[24], which lists the star as no. 467, with a proper motion slightly less than 0″·01 in each coördinate, and gives a 'photographic magnitude' of 11·10. There seems to be a lot of uncertainty as to the actual brightness of the star. Upgren[2], who gives the spectral type as K0 III, lists the photographic magnitude as 12·6; *Simbad* gives $V = 12^{m}{\cdot}37$, $B = 12^{m}{\cdot}77$ by transformation from the *Tycho 2* V_T and B_T, the *Tycho* identification being 1986-603-1. If the star were really as faint, and the colour index as small, as the *Tycho* magnitudes imply, it is doubtful whether the object would have been usefully observable at Cambridge at all; as it is, it is certainly one of the faintest stars that can reasonably be observed with the Cambridge instrumentation. Unfortunately it escaped being observed photometrically at Palomar, so we have no original photometric data of our own.

As with most of the Upgren stars, the first radial-velocity observation was made at Palomar in 1972, and then there was a long gap before the next measurement, made at OHP in 1988 and discordant, leading to a repetition of it the very next night. Altogether, 46 observations have now been made — 25 at OHP, 19 with the Cambridge *Coravel*, and one at ESO, as well as the one at Palomar; they are all set out in Table VI. Observations of U 25° 40 tend to be marginal at Cambridge, with the photon-counting rates sometimes only in the forties per second, including dark counts that are a significant fraction of the total and are not statistically well-behaved. One Cambridge observation, the one that had the lowest counting rate of all (32) has an exceptional residual and has been rejected. Otherwise all the velocities have been given equal weight in the solution of the orbit, which is illustrated in Fig. 7 and has the following elements:

$P = 2063 \pm 4$ days	$(T)_5 = \text{MJD } 51304 \pm 114$
$\gamma = -16{\cdot}18 \pm 0{\cdot}10$ km s^{-1}	$a_1 \sin i = 153 \pm 4$ Gm
$K = 5{\cdot}39 \pm 0{\cdot}14$ km s^{-1}	$f(m) = 0{\cdot}0334 \pm 0{\cdot}0026\ M_{\odot}$
$e = 0{\cdot}069 \pm 0{\cdot}024$	
$\omega = 225 \pm 20$ degrees	R.m.s. residual (wt. 1) = 0·55 km s^{-1}

TABLE VI

Radial-velocity observations of U 25° 40

Except as noted, the sources of the observations are as follows:
1988–1998 — OHP Coravel; 2002–2014 — Cambridge Coravel
All the observations (apart from one rejection) have the same weight

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1972 Jan. 3·52*	41319·52	−14·5	0·160	+0·1
1988 Mar. 14·04	47234·04	−20·3	3·027	−0·9
15·01	235·01	−19·2	·028	+0·2
1989 Mar. 26·89	47611·89	−13·6	3·210	−0·6
Apr. 27·94	643·94	−12·9	·226	−0·3
1990 Jan. 31·08	47922·08	−11·6	3·361	−0·6
Feb. 12·28†	934·28	−11·1	·367	0·0
1991 Jan. 27·16	48283·16	−14·0	3·536	−0·5
Feb. 6·13	293·13	−13·7	·541	−0·1
Dec. 17·24	607·24	−18·1	·693	0·0
1992 Jan. 14·20	48635·20	−18·3	3·706	+0·2
Apr. 22·93	734·93	−19·8	·755	+0·1
June 20·91	793·91	−19·5	·783	+1·0
Dec. 18·15	974·15	−21·9	·871	−0·1
1993 Feb. 14·08	49032·08	−21·6	3·899	+0·2
Mar. 19·03	065·03	−21·8	·915	−0·1
July 7·89	175·89	−20·7	·969	+0·3
1994 Jan. 3·19	49355·19	−17·6	4·056	+0·8
Feb. 21·05	404·05	−18·0	·079	−0·4
Apr. 30·93	472·93	−16·9	·113	−0·6
1995 Jan. 4·15	49721·15	−12·6	4·233	−0·1
June 2·94	870·94	−10·4	·306	+0·9
1996 Jan. 1·16	50083·16	−11·9	4·408	−0·6
Mar. 30·05	172·05	−10·6	·451	+1·2
Dec. 16·17	433·17	−15·0	·578	−0·4
1997 Apr. 16·05‡	50554·05	−17·9	4·637	−1·6
May 10·90‡	578·90	−16·9	·649	−0·2
1998 May 1·93	50934·93	−21·1	4·821	+0·2
July 12·89	51006·89	−22·1	·856	−0·4
2002 Apr. 6·94	52370·94	−13·4	5·517	−0·4
May 28·98	422·98	−14·1	·543	−0·5
2003 Feb. 21·05	52691·05	−16·6	5·673	+0·9
Mar. 31·99	729·99	−18·3	·691	−0·3
2004 Feb. 26·13	53061·13	−22·6	5·852	−0·9
2005 Apr. 19·01	53479·01	−17·9	6·055	+0·6
2009 Apr. 29·93	54950·93	−20·2	6·768	0·0
2010 May 16·93	55332·93	−21·4	6·953	−0·1

TABLE VI (*concluded*)

Date (UT)			*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2011	Apr.	9·02	55660·02	−15·6	7·112	+0·7
2012	Feb.	2·16§	55959·16	−9·6	7·257	+2·4
2013	Mar.	12·07	56363·07	−11·5	7·453	+0·3
		31·03	382·03	−11·5	·462	+0·4
	May	2·92	414·92	−12·2	·478	0·0
	June	2·95	445·95	−12·1	·493	+0·4
2014	Jan.	13·23	56670·23	−15·0	7·601	+0·3
	Feb.	2·22	690·22	−15·2	·611	+0·4
	May	25·95	802·95	−16·7	·666	+0·5

*Observed with Palomar 200-inch telescope
†Observed with ESO *Coravel*
‡Observed with Cambridge *Coravel*
§ Rejected

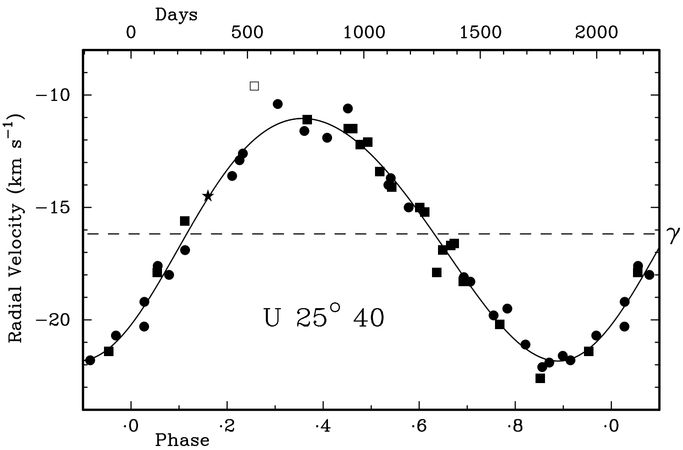

FIG. 7

Orbit of U 25° 40, analogous to the previous figures. The open square plots an observation rejected in the solution of the orbit.

In that solution the Cambridge velocities have been adjusted by –0·4 km s⁻¹ to bring them into approximate systematic agreement with those from OHP. The eccentricity is small, but at 2·9σ it appears fairly secure. The sums of the squares of the residuals from orbits with the eccentricity (*a*) left free, and (*b*) fixed at zero, are 13·58 and 16·16 (km s⁻¹)², respectively. The former figure represents the 39 degrees of freedom in the '*e* free' orbit, while the difference of 2·58 represents the cost of the two additional degrees of freedom gained by imposing the constraint $e \equiv 0$. Those figures yield a value of 3·71 for $F_{2,39}$, which is significant at a level of about 3·4%; that might be seen as a bit marginal, but (particularly since there is no reason to favour an exactly circular orbit when the orbital period is several years) the eccentric orbit is adopted. The mass function is quite small and holds out little hope of detecting the secondary component in U 25° 40.

4 Comae Berenices (HR 4640, HD 105981, U 26° 58)

In the immediately preceding section above, it is noted that U 25° 40 is only about 1° south of 4 Comae, a late-type spectroscopic binary whose orbit was determined at the Dominion Astrophysical Observatory (DAO) the best part of a century ago. The discovery that that star (identified as Boss[26] 3180; the *Bright Star Catalogue* was not published until 1930) is a spectroscopic binary was announced in 1921 by Plaskett *et al.*[27]. They listed five radial velocities, all obtained in 1920, soon after the inauguration of the DAO and its 72-inch reflector (then second in size only to the Mount Wilson 100-inch), that showed a range of 26 km s^{-1}. In 1930 Harper[25] gave an orbit, with P = 461 days, e = 0·169, and ω = 235°·29 (albeit with a 'probable error' given as 10°·23). Five years later he[28] revised the period slightly and also gave a new value for the epoch of periastron, but said that "It is not thought that this change will necessitate a revision of the other elements."

Hipparcos noticed the astrometric wobble corresponding to the spectroscopic orbit, and gave elements[29] for it. 4 Comae is one of 235 stars for which *Hipparcos* gave elements, in a section of just five pages called Part O, to be found in Volume 10 of the printed catalogue. Only in 46 cases were *all* of the elements determined by *Hipparcos* from its own astrometry; in the others some of them (*all* apart from a_0 in a few instances) were adopted from, or made use of, pre-existing spectroscopic orbits or astrometric information. In those cases no entries were given for the standard errors of the adopted quantities in the relevant columns in the table of elements — instead, a reference was given to their source (to Harper[25,28] in the case of present interest). "A blank field [for a standard error] signifies either that the corresponding element was adopted from the literature (in which case a reference number is given in Field DO 16), or that the element was otherwise constrained in the solution (*viz.* by assuming a circular orbit, $e = \omega = 0$)."[30]. The period is given as 462·8000 days, which looks indeed to have been quoted from Harper[28], though with the apparent precision reprehensibly enhanced by a factor of a thousand; e is given as 0·1690 and ω as 235°·29. In the (only Web-based) catalogue by Pourbaix *et al.*[31] (*SB9: The ninth catalogue of spectroscopic binary orbits*), the values of e and ω are slightly rounded to 0·17 and 235°·3, respectively, but the period is given as "461." days, evidently from Harper's initial solution[25] rather than the updated one[28] with the improved value of 462·8 days.

Even for those elements where standard errors *are* given by *Hipparcos*, the precisions to which they are given are in many cases ludicrous and are not sensibly related to the quantities themselves. There can be no sense in giving the standard deviation of the epoch T, for example (or of any other quantity), to six 'significant' figures, or in giving T itself to a ten-thousandth of a day when its standard error is many days — both of which faults are exemplified by the case of 4 Comae. The lack of scientific understanding manifested by such elementary mistakes does tend to undermine the faith that one would wish to be able to place in all the other results put forward by the *Hipparcos* consortium.

As a late-type NGP star, 4 Comae naturally featured in the NGP survey[1], but no new observations were included in the report[1] on the survey because the radial velocity was already known. 4 Comae is also actually an Upgren[2] star, U 26° 58, and so could be said to qualify perfectly fairly in its own right for discussion here. This paper, however, was really intended to deal with Upgren stars that are too faint to appear in the *Henry Draper Catalogue*, whereas 4 Comae is a bright star, both in the sense of being reasonably visible to the (appropriately educated) naked eye in dark-sky conditions, and also inasmuch as it has an entry in the *Bright Star Catalogue*. What prompts its inclusion in this paper is the discovery, which was in fact made in the actual course of writing the section on U 25° 40, that 4 Comae is quite conspicuously double-lined (*cf*. Fig. 8).

The V magnitude and $(B - V)$ colour index measured by Yoss for 4 Comae and published in the NGP survey were $5^{m}{\cdot}62$ and $1^{m}{\cdot}43$, respectively. Häggkvist & Oja[32] gave the same quantities as $5^{m}{\cdot}66$ and $1^{m}{\cdot}39$, so the B magnitudes are in agreement despite the discrepancy in V; Schild[33] agreed with Häggkvist & Oja for V, but found $(B - V) = 1^{m}{\cdot}42$, $(U - B) = 1^{m}{\cdot}60$. Sturch & Helfer[34] gave photometry for 4 Comae as $V = 5^{m}{\cdot}69$, $(B - V) = 1^{m}{\cdot}416$, $(U - B) = 1^{m}{\cdot}573$. The colour indices are those of a late-K giant. *Simbad* credits Hossack[35] with a classification of K4 III obtained by means of his oscilloscopic system[36], but I cannot find 4 Comae among the stars in his listing. Upgren[2] found the type to be K2 V, whereas Schild[33] gave it as K4 III, which is the same type as Hartkopf & Yoss[37] derived from *DDO*-style narrow-band photometry (and was repeated in the NGP survey paper[1]) and which appears for 4 Comae in the *Bright Star Catalogue*. *Simbad*, however, prefers the type of K5 III that was given by Young & Koniges[38]. That may in fact be the latest classification performed in the 'proper' way from a spectrogram, although spectrograms used for that purpose are supposed to be of much lower dispersion than the 13 Å mm^{-1} (from the Kitt Peak 84-inch coudé) used by Young & Koniges, who seem unlikely to have possessed a range of analogous spectrograms of MK standards for comparison. As well as finding a spectral type from their *DDO* photometry, Hartkopf & Yoss obtained an M_V of $+1^{m}{\cdot}1$, from which they derived a distance to 4 Comae of 81 pc, unfortunately listed in their paper as 81 *kpc* through a systematic error in the headings of the several pages of the relevant table.

The (revised[39]) *Hipparcos* parallax of 4 Comae is $3{\cdot}68 \pm 0{\cdot}38$ arc-milliseconds, corresponding to a distance modulus of $7^{m}{\cdot}17 \pm 0^{m}{\cdot}23$ (distance about 270 pc) and so to an M_V as bright as about $-1^{m}{\cdot}5$, with the same uncertainty. It is slightly embarrassing that, just before the *Hipparcos* results were published, Yoss & Griffin[1] repeated from Hartkopf & Yoss[37] the M_V estimated from *DDO* photometry as $+1^{m}{\cdot}1$, a factor of about ten less than the *Hipparcos* value in terms of luminosity and therefore of about $\sqrt{10}$ in distance, at 'only' 79 pc. The photometry also led Hartkopf & Yoss to tabulate a value of $-0{\cdot}42$ for the [Fe/H] of 4 Comae; in the NGP survey paper[1] [Fe/H] was moderated to $-0{\cdot}19$, but the present writer shares responsibility only vicariously for that figure and can throw no light on why it would have changed from the previous one.

Radial velocities and orbit of 4 Comae

The radial velocity of 4 Comae was first measured with the 72-inch reflector at the DAO, Victoria, almost as soon as that observatory was set up. Five measurements were published by the Director and his colleagues in a paper[27] in the very first volume of the *DAO Publications*, giving velocities (but no orbits) for no fewer than 88 bright stars that were found to exhibit variable radial velocities in the first 2½ years of operation*. 4 Comae (there identified, like all the other stars in the paper, by its *Preliminary General Catalogue*[26] (Boss) number†, in this case Boss 3180), had been found to change its velocity by as much as 25 km s^{-1} in little more than two months in the spring of 1920. Nearly a decade later, Harper[25] published his orbit for the star, on the basis of 42 prismatic spectrograms taken at a dispersion of 29 Å mm^{-1} at Hγ. The orbital period was found to be 461 days and the amplitude just over 14 km s^{-1}. Five years later still, in 1935, Harper[28] published a small adjustment to the orbit, changing only the period (to 462·8 days) and the epoch, in the light of just four new observations, all of which had remarkably small residuals from the revised orbit.

As far as radial velocities of 4 Comae are concerned, up till now matters have largely rested where Harper left them all those years ago. Actually, although they are not retrieved in the *Simbad* 'measurements' section for 4 Comae, two velocities from the Fick Observatory were published by Beavers & Eitter[42], and two others were referred to by de Medeiros & Mayor[43] and can be retrieved from a file that those authors subsequently lodged with the *Centre de Données Stellaires*. Evidently de Medeiros did not consider that his data contributed anything much to the orbit, because in 2002, in collaboration[44] with others, he published a value for the $v \sin i$ of 4 Comae, of 3·9 km s^{-1}, and quoted there the period and eccentricity that had been found by Harper in 1930. In fact that 85-year-old orbit is still the one that is, at the time of writing, current in the *Ninth Catalogue*[31] of spectroscopic binary orbits.

When he came to write this paper, the present author found that he had made nine measurements of the radial velocity of 4 Comae over a total interval of more than 40 years (the first one was actually made by his student at the time, G. A. Radford). There were two made with the original photoelectric spectrometer[3] at Cambridge, one from the instrument at the DAO, Victoria[45], five from the OHP *Coravel*, and one from the Cambridge *Coravel*. Those observations had been accumulated rather casually, just because 4 Comae is a late-type star in the NGP field and therefore qualified for the writer's NGP radial-velocity observing programme[1], but no effort had been made to re-determine the orbit. They are listed in Table VII, together with Harper's much more numerous observations[25,28] and those made by Beavers & Eitter[42] and de Medeiros & Mayor[43].

A trial orbit solution that was made as soon as all the data had been transcribed into a file (analogous to Table VII here) immediately brought to light two interesting features. One was that the mass function (a quantity not given by Harper) is significantly large, being close to a quarter of a solar mass, and the other is that the velocity amplitude given in a solution that is dominated by the writer's few observations has an amplitude very significantly (more than 25%) larger than the one obtained by Harper. Both those features could be understood in terms of the spectrum of 4 Comae being incipiently double-lined. (The relatively modest resolution of Harper's spectra would cause the spectra always to be blended together, so the result of measurement would be a sort of blended mean.) It so happened that, at the time that the investigation

*That paper was a sequel to an even earlier one[40] that gave analogously the velocities observed for 100 other newly discovered binaries.

†The *Bright Star Catalogue* was not published until 1930, at least not under that name, although in fact its selection of stars and their numbering was taken over directly from the *Harvard Revised Photometry* that had been published[41] in 1908.

just described was made, 4 Comae was right at a node of its orbit, a phase ideal for searching for a secondary component. The writer accordingly awaited a fine night with uncharacteristic impatience, and was well rewarded as soon as he was granted one: about five seconds after initiating an integration on 4 Comae he could begin to recognize the secondary 'dip' near the expected position in the trace. Fig. 8 shows the trace after it had been allowed to accumulate a

TABLE VII

Radial-velocity observations of 4 Comae

The observations below were all made at the DAO by Harper[25,28]*; weight* $^1/_{10}$

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
1920 Feb. 8·47	22362·47	+10·6	0·989	−2·8
22·42	376·42	20·4	1·019	+1·7
Mar. 17·36	400·36	29·5	·071	+1·6
Apr. 14·29	428·29	36·1	·132	+0·2
21·27	435·27	34·8	·147	−2·5
1925 Mar. 6·48	24215·48	12·8	4·997	−1·8
18·44	227·44	21·9	5·022	+2·7
Apr. 5·38	245·38	28·8	·061	+2·5
8·32	248·32	29·8	·068	+2·5
1926 Feb. 13·48	24559·48	15·4	5·741	+4·4
26·37	572·37	11·6	·768	+2·7
May 5·28	640·28	6·2	·915	+1·1
10·18	645·18	12·1	·926	+6·3
12·21	647·21	13·8	·930	+7·7
24·23	659·23	15·8	·956	+7·1
30·26	665·26	13·0	·969	+2·6
June 7·25	673·25	21·3	·987	+8·3
July 5·22	701·22	21·1	6·047	−2·6
19·21	715·21	23·0	·077	−5·9
21·21	717·21	26·7	·082	−2·9
27·21	723·21	23·9	·095	−7·6
Aug. 6·20	733·20	31·8	·116	−2·5
1928 Feb. 12·43	25288·43	36·8	7·317	−2·8
Mar. 3·37	308·37	33·8	·360	−4·1
Apr. 13·27	349·27	30·7	·449	−2·5
May 1·22	367·22	30·6	·487	0·0
8·26	374·26	32·0	·503	+2·4
29·22	395·22	26·6	·548	+0·2
June 12·23	409·23	22·4	·578	−1·7
1929 Feb. 10·51	25652·51	26·5	8·104	−6·3
Mar. 23·34	693·34	36·9	·193	−3·1
Apr. 9·32	710·32	39·3	·229	−1·5
30·30	731·30	36·2	·275	−4·5
May 7·30	738·30	36·4	·290	−4·0
1930 Jan. 17·50	25993·50	7·9	8·842	+3·2
Feb. 25·47	26032·47	9·3	·926	+3·5
Mar. 4·39	039·39	7·0	·941	0·0
15·36	050·36	13·6	·965	+3·8
Apr. 8·33	074·33	23·6	9·017	+5·4
22·28	088·28	20·4	·047	−3·3
May 6·24	102·24	27·1	·077	−1·8
8·27	104·27	+28·7	·081	−0·9

TABLE VII (*concluded*)

Date (UT)	*MJD* *km s⁻¹*	*Velocity* *km s⁻¹*	*Phase*	*(O–C)*	*Weight*
1973 Mar. 11·00*	41752·00	+4·1	42·922	−1·4	2·5
1978 May 10·16†	43638·16	15·6	47·001	+0·2	0
1980 May 9·17†	44368·17	23·4	48·580	−0·6	0
1984 Apr. 30·86*	45820·86	14·7	51·721	+2·1	0
1988 Jan. 26·53‡	47186·53	17·9	54·675	+1·6	0
Mar. 14·16§	234·16	8·2	·778	−0·1	25
June 6·87¶	318·87	7·3	·961	−2·0	0
Nov. 7·22§	472·22	40·1	55·293	−0·2	25
1989 Feb. 5·15¶	47562·15	29·5	55·487	−1·2	0
Mar. 28·07§	613·07	22·9	·597	+0·3	5
May 1·86§	647·86	17·4	·673	+0·9	0
1996 Apr. 25·94§	50198·94	39·9	61·190	0·0	25
2013 May 9·00‖	56421·00	19·7	74·646	+1·0	0
2015 May 12·93‖	57154·93	41·2	76·233	+0·3	25
12·93**	154·93	+5·6	·232	0·0	5

*Observed with original Cambridge spectrometer
†Published observation by Beavers & Eitter[42]
‡Observed with DAO spectrometer
§Observed with OHP *Coravel*
¶Observation alluded to by de Medeiros & Mayor[43]
‖Observed with Cambridge *Coravel*
**Observed with Cambridge *Coravel* (secondary star)

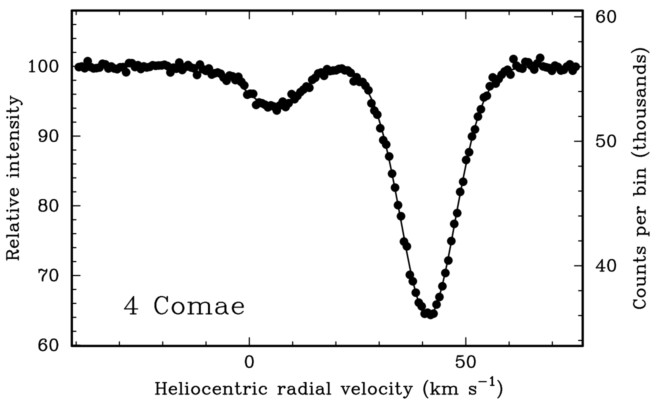

FIG. 8

Radial-velocity trace of 4 Comae, obtained with the Cambridge *Coravel* on 2015 May 12 and clearly illustrating the previously unrecognized double-lined nature of the system.

good deal longer. Thus it could be said that the discovery of the SB2 nature of 4 Comae was actually made in the office rather than at the telescope, but it was immediately verified when put to the touch by actual observation.

Although we are clearly now in a position to produce a more realistic orbit for 4 Comae than was possible previously, the orbit that is divined here is itself only preliminary. All the old photographic velocities are evidently of blends, and under-state the real amplitude of the primary's velocity variation; and as far as the secondary is concerned there is only the one measurement. Furthermore, some of the writer's own previous measurements were made at phases near enough to conjunctions to be affected by blending between the components. The best we can do is to assign very heavy weight to the four photoelectric observations obtained near the nodes of the orbit, *some* weight to one old Cambridge observation that is well enough removed from, and one new one that is so close to, the γ-velocity as not to be too badly affected by blending, and relatively tiny weight to the old DAO observations, to obtain some advantage from their great age and (probably) more or less uniform blending to help fix the shape of the orbit and to refine the orbital period. Their weight in the solution, even collectively, however, is not enough to have any substantial effect on any element other than the period. Three of the four published photoelectric observations[42,43] are so close to the γ-velocity as certainly to be affected by the blending that their authors did not notice, and it was decided not to try to utilize the remaining one but to rely on the writer's own observations — plus Harper's numerous measures that are assigned a very low weight — alone. An empirical adjustment of +3 km s^{-1} has been made to Harper's velocities as listed in Table VII, to bring them more or less into line with the recent ones.

With only six new observations assigned any weight at all, when we need to solve for six unknowns, and only one observation of the secondary to fix the seventh unknown (the secondary's amplitude), we are obviously treading on very thin ice indeed. It will be highly desirable to follow the system around the cycle and to obtain a set of modern observations from which, in the light of dip profiles that are well determined by fully resolved traces such as that shown in Fig. 8 (which is, obviously, the only one available at the moment), reliable velocities can be derived for both components individually.

The new, double-lined, orbital solution is illustrated in Fig. 9, and its elements are set out in Table VIII, with Harper's original orbit[25] given for comparison. It will be seen that the uncertainties of the new orbit are scarcely smaller than those claimed for the old one, although it should be remarked that the latter were 'probable errors' whereas of course the quantities tabulated for the new orbit are standard deviations, whose definition shows them to be about 1·5 times the 'probable errors'. It has already been remarked that the new elements depend on the barest minimum of fresh observations, and that their uncertainties could and should be substantially reduced by systematic observation of 4 Comae round a cycle. Such a campaign will actually take much more than one cycle, owing to the proximity of the orbital period to one year and the resulting impact on the phase distribution of the times of accessibility of the star to observation; that is the reason that this preliminary orbit is given now.

The apparently large value for the r.m.s. residuals refers, as noted, to unit observational weight. The recent *Coravel* observations, which (where they are unblended) have been attributed weight 25 in the solution of the orbit, evidently have mean residuals smaller than the unit-weight value by $\sqrt{25}$, *viz.*, 0·24 km s^{-1}.

TABLE VIII

Orbital elements for 4 Comae

Element	*Harper*[25]	*This paper*
P (days)	461 (fixed)	462·40 ± 0·08
T (MJD)	22360·29 ± 11·35	49649 ± 8
γ (km s^{-1})	+21·30 ± 0·30	+24·15 ± 0·34
K_1 (km s^{-1})	14·25 ± 0·45	18·40 ± 0·27
K_2 (km s^{-1})		20·4 ± 1·0
q		1·11 ± 0·06
e	0·169 ± 0·028	0·227 ± 0·014
ω (degrees)	235·29 ± 10·23	247 ± 6
$a_1 \sin i$ (Gm)	89·032	114·0 ± 1·7
$a_2 \sin i$ (Gm)		127 ± 6
$f(m_1)$ ($M_\odot$)		0·277 ± 0·013
$f(m_2)$ ($M_\odot$)		0·38 ± 0·06
$m_1 \sin^3 i$ ($M_\odot$)		1·37 ± 0·16
$m_2 \sin^3 i$ ($M_\odot$)		1·23 ± 0·08
R.m.s. residual (wt. 1)(km s^{-1})	3·0	1·22

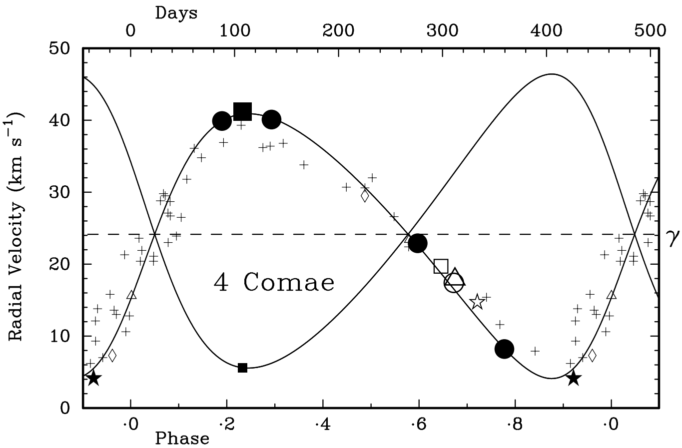

FIG. 9

Orbit of 4 Comae. The uncharacteristically large symbols represent the writer's measurements of the primary star; the ones near the nodes, which are unaffected by blending, have been given overwhelming weight in the determination of the orbit (their weightings are explicitly specified individually in Table VII). Open versions of the same symbols plot blended observations that have been given weight zero. Circles and squares denote the OHP and Cambridge *Coravel*s, respectively, as usual; stars refer to the original Cambridge spectrometer[3], and the triangle to the DAO one[45]. The plusses plot the measurements made the best part of 100 years ago by Harper[25,28], all of which are vitiated by the mutual blending of the components, which accounts for the smaller velocity amplitude that they indicate. They have been included in the solution but with almost infinitesimal weight, with a view to refining the period by greatly extending the time base. The open triangles and diamonds plot the observations of Beavers & Eitter[42] and de Medeiros & Mayor[43], respectively; all but one must be of blends, and all have been zero-weighted. At the moment there is only the one measurement of the secondary star (plotted as a square); it is illustrated in the trace seen in Fig. 8.

It remains to speculate on the nature of the companion star. The dip that it gives in the radial-velocity trace shown in Fig. 8 has an equivalent width (defined in exact analogy with spectral lines) about one-sixth of that of the primary star, or very nearly two magnitudes smaller if the ratio is expressed in terms of stellar magnitudes. The colour indices of the system are not unusual, so the companion is not likely to be of a character very different from that of the primary, whose absolute magnitude and late type show it to be near the top of the giant branch in the H–R Diagram. Indeed, the very fact that there is a dip at all shows that the companion star is of late type, since its spectrum cross-correlates significantly with the (type K2) mask in the *Coravel* instrument. Its mass is somewhat less than that of the primary, so it should be at a rather earlier stage of evolution. We can therefore tentatively suppose the secondary to be simply another late-type giant star, one that has not yet ascended so far up the giant branch as its companion and is about two magnitudes less bright and correspondingly of somewhat smaller colour index. Its spectral type might accordingly be estimated at about K2 III–IV. Its adulteration of the spectrum of the primary probably goes at least a little way towards excusing the misleadingly faint value obtained by *DDO* photometry for the M_V of 4 Comae.

Acknowledgements

This paper includes only a very small sample of a large observational project that was greatly promoted by the granting of observing time at other observatories, to which I am pleased to express my sincere gratitude. The Palomar Observatory (it was then part of a consortium called at the time the Hale Observatories) allowed the use of the 200-inch reflector, at a time when it was by far the largest telescope in the world, to get the radial-velocity work on the Upgren programme off to a splendid start. It also granted the use of the 20-inch photometric telescope for the whole duration of the Galactic Pole observing season in 1976 February–June, and four photometric observing runs on the then-new 60-inch reflector. Of course Radford's prolonged stay on Palomar Mountain, where there was a constant flux of distinguished visiting astronomers, was a highly beneficial experience for the student. He and I are also very grateful to KPNO for the photometric opportunity, in which much was accomplished, the previous year. The delay of forty years in thus offering to those organizations our thanks is regretted but in no way diminishes our sincerity. It is a further pleasure to acknowledge the kind collaboration of the late Dr. Kenneth Yoss, who shouldered much of the photometric side of the NGP work and of the discussion of the overall programme. Dr. L. Hansen shared in the Kitt Peak observations and performed the heavy task of reducing not only those, but the Palomar photometry as well. The staff of the Cambridge Observatories' workshop made substantial efforts to construct, just in the month of 1976 January, the photometer that was used so successfully at Palomar that year from February to June. The late Mr. Horace Dall, amateur astronomer, adventurer, and expert optician, provided at a few days' notice a bespoke re-imaging system needed for the finding eyepiece of the photometer. I am pleased to take this opportunity to express grateful thanks for all of those contributions.

References

(1) K. M. Yoss & R. F. Griffin, *JA&A*, **18**, 161, 1997.
(2) A. R. Upgren, *AJ*, **67**, 37, 1962.
(3) R. F. Griffin, *ApJ*, **148**, 465, 1967.
(4) R. F. Griffin & R. O. Redman, *MNRAS*, **120**, 287, 1960.
(5) B. Strömgren & K. Gyldenkerne, *ApJ*, **121**, 43, 1955.
(6) P. Dickow *et al.*, *A&AS*, **2**, 1, 1970.
(7) L. Hansen & G. A. Radford, *A&AS*, **53**, 427, 1983.
(8) R. F. Griffin & J. E. Gunn, *ApJ*, **191**, 545, 1974.
(9) R. E. M. Griffin & R. F. Griffin, *MNRAS*, **350**, 685, 2004.
(10) F. W. A. Argelander, *Astronomische Beobachtungen auf der Sternwarte zu Bonn* (Adolph Marcus, Bonn), **4**, 1861.
(11) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997.
(12) R. F. Griffin, *MNRAS*, **145**, 163, 1969.
(13) R. E. Wilson, *General Catalogue of Stellar Radial Velocities* (Carnegie Institution of Washington, Washington, D.C.), 1953.
(14) R. F. Griffin, *MNRAS*, **155**, 1, 1971.
(15) R. F. Griffin, *The Observatory*, **90**, 47, 1970.
(16) R. F. Griffin, *MNRAS*, **148**, 211, 1970.
(17) W. Tirion, B. Rappaport & W. Remaklus, *Uranometria 2000·0 Deep Sky Atlas* (2nd edn.) (Willmann-Bell, Richmond, Virginia), 2001.
(18) R. F. Griffin, *JA&A*, **33**, 245, 2012. (See Fig. 1.)
(19) T. Oja, *A&AS*, **61**, 331, 1985.
(20) A. Schwassmann & P. J. van Rhijn, *Bergedorfer Spektral-Durchmusterung* (Hamburger Sternwarte, Bergedorf), **3**, 161, 1947.
(21) R. F. Griffin, *The Observatory*, **120**, 325, 2000.
(22) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(23) R. A. Fisher & F. Yates, *Statistical Tables for Biological, Agricultural and Medical Research* (Oliver & Boyd), London, 1948, p. 37.
(24) C. Abad & B. Vicente, *A&AS*, **136**, 307, 1999.
(25) W. E. Harper, *PDAO*, **4**, 303, 1930.
(26) L. Boss, *Preliminary General Catalogue of 6188 stars for the epoch 1900* (Carnegie Institution of Washington, Washington, D.C.), 1910, pp. 128/9.
(27) J. S. Plaskett *et al.*, *PDAO*, **1**, 287, 1921.
(28) W. E. Harper, *PDAO*, **6**, 209, 1935.
(29) Ref. 11, **10**, p. DO2.
(30) Ref. 11, **1**, p. 184.
(31) http://sb9.astro.ulb.ac.be/
(32) L. Häggkvist & T. Oja, *Ann. Rev. A&A*, **5**, 125, 1969.
(33) R. E. Schild, *AJ*, **78**, 37, 1973.
(34) C. R. Sturch & H. L. Helfer, *AJ*, **77**, 726, 1972.
(35) W. R. Hossack, *Commun. DDO*, no. 35 (*ApJ*, **119**, 613, 1954).
(36) W. R. Hossack, *JRAS Canada*, **47**, 195, 1953.
(37) W. I. Hartkopf & K. M. Yoss, *AJ*, **87**, 1679, 1982.
(38) A. Young & A. Koniges, *ApJ*, **211**, 836, 1977.
(39) F. van Leeuwen, *Hipparcos, the new reduction of the raw data* (Springer, Dordrecht), 2007.
(40) J. S. Plaskett *et al.*, *PDAO*, **1**, 163, 1921.
(41) E. C. Pickering, *Harvard Ann.*, **50**, 1910.
(42) W. I. Beavers & J. J. Eitter, *ApJS*, **62**, 147, 1986.
(43) [Announced by] J. R. de Medeiros & M. Mayor, *A&AS*, **139**, 433, 1999.
(44) J. R. de Medeiros, J. R. P. da Silva & M. R. G. Maia, *ApJ*, **578**, 943, 2002.
(45) J. M. Fletcher *et al.*, *PASP*, **94**, 1017, 1982.

REVIEWS

Jacobus Cornelius Kapteyn: Born Investigator of the Heavens, by P. C. Van der Kruit (Springer, Heidelberg), 2015. Pp. 698, 24 × 16 cm. Price £180/$279 (hardbound; ISBN 978 3 319 10875 9).

This is the definitive biography of J. C. Kapteyn (1851–1922) who was a leader of the international astronomical community at the turn of the 20th Century. It is extremely detailed, covering all aspects of Kapteyn's life, and is liberally illustrated, especially with portraits.

Kapteyn came from a long line of schoolmasters which can be traced back as far as the 18th Century. He studied at his father's own school and then at the University of Utrecht. He graduated *magna cum laude*, with a doctor's degree in applied mathematics; his thesis dealt with the vibrations of flat membranes. After his graduation in 1875, Kapteyn worked at Leiden Observatory until 1877 when he was appointed Professor of Astronomy in Groningen. There he discovered that he was expected to teach astronomy without any equipment, and he spent his early years there in vain attempts to fund an astronomical observatory. During that period he regularly visited Leiden, first, to determine stellar parallaxes with the meridian circle, and second, to determine absolute stellar declinations with the universal instrument. Because of his teaching duties he could make those visits only during university vacations, and the visits ceased after his marriage in 1879. His visits to Leiden showed that both methods could work, and this brought him into contact with David Gill (1843–1914), Her Majesty's Astronomer at the Cape of Good Hope, who had similar interests.

In 1882 Gill had photographed the great comet of that year with the newly developed dry-plate process. The large number of faint stars on the plates led to the idea of making a photographic *durchmusterung* of the southern sky, similar to existing ones made visually for the north. *Durchmusterung* was the accepted word for a list of all stars visible in a small telescope. The modern parlance is 'flux-limited sample'. He cannibalized an unused telescope mounting and with help from the British government he set up a dedicated astrograph. However, he had neither money nor resources to measure the photographic plates and derive the right ascensions and declinations of the stars recorded. He explained his difficulty to Kapteyn in the course of their correspondence; Kapteyn offered to complement Gill's work, if he could find the resources. He received grants from two private Dutch foundations and was allowed the use of two rooms by a fellow professor. He devised a measuring machine from old instruments intended for teaching (in effect an analogue computer) which ingeniously reduced the amount of subsequent calculation. Measuring extended from 1886 to 1892 and the results were published in the *Cape Annals* between 1896 and 1900. Over the years there have been unconfirmed rumours that Kapteyn employed convicts to measure plates. The evidence reviewed here shows that the rumours are almost certainly groundless.

Every field was photographed twice, or more if one of the plates was defective. Two plates were held by the plate-holder of the measuring machine with a slight offset; the measurer could immediately identify plate defects or suspected variable stars or stars of significant proper motion. Doubtful cases were referred back to the Cape for confirmation. Kapteyn discovered several variable stars and a star of extremely large proper motion. The last had already been recorded in the *Cordoba Zone Catalogue* with a position confirming Kapteyn's proper motion. This star was also referred to the Cape where two visual observations were made by the Cape astronomer Robert Innes (1861–1933), who later became head of the Union Observatory, Johannesburg. Kapteyn published a note in the *Astronomische Nachrichten* announcing the discovery of a star with the largest known proper motion, listing the five observations and crediting Innes for the last two. The Editors of *The Observatory* published an editorial in which they suggested that Innes was a co-discoverer and that it was wrong for Kapteyn to claim it for his own. Gill replied in a letter to the Editors that all the work was shared collectively between the Cape and Groningen and that all were happy with what had happened. That did not discourage the Editors from a further editorial pedantically justifying the earlier one. Nowadays the star is universally known as Kapteyn's Star.

Throughout his career Kapteyn was interested in the structure of the sidereal universe. William Herschel (1738–1822) had derived a model and also the apex of the Sun's way through space. The number of parallaxes available to Kapteyn was very small and he realized that it must be substantially increased. Anders Donner (1854–1938), director of the Helsingfors Observatory, offered to provide some plates for Kapteyn to measure for trigonometric parallax. At that time no-one was sure of the metrological properties of photographic plates; during processing the emulsion swelled and it was not clear that the developed grains would sink back into the same place when the plate was dried. Kapteyn aimed to avoid this by exposing the same plate three times, each slightly displaced; two at the extremes of the parallactic ellipse and a repeat after a year under the same conditions as the first. This enabled both the parallax and the proper motion to be measured. In practice it was difficult to achieve similar images for the three exposures because of differences in transparency and seeing. The measurement and reduction took longer than expected. Only a handful of the measured parallaxes were significantly positive and an embarrassingly large number were negative.

This turned Kapteyn's attention from trigonometric to secular parallaxes, which are based on the Sun's motion through the average of all the stars with measured proper motion. The secular method has a baseline much longer than the trigonometric method but can be used only for substantial samples of stars. There were 3268 accurately measured proper motions available to Kapteyn but they were afflicted by the uncertainty of the constant of precession. He derived the solar apex and with the 51 radial velocities then known, the solar motion was 17 km s^{-1}. When he examined the proper motions of the individual stars he expected to find random motions superposed on the reflected solar motion. He plotted polar histograms of the direction of proper motion and found them quite different from his expectation. The explanation was that the stars had two preferential directions of motion, both different from the Sun. The motions were merely preferential and individual stars still had their individual peculiar motions. Kapteyn termed them "star streams", which he announced at the St. Louis World's Fair in 1904 where he was an invited speaker.

Kapteyn realized that he was dealing with small samples of bright stars and that working towards fainter stars over the whole sky would require a prohibitive amount of effort. He proposed his 'Plan of the Selected Areas' consisting of 206 fields spread evenly on a grid of 15° over the whole celestial sphere. The fields were 80′ square at high Galactic latitudes reducing to 20′ in the Galactic plane. Edward Pickering (1846–1919), director of the Harvard Observatory, provided plates which were measured at Groningen to form the 'Durchmusterung of the Selected Areas'. He also suggested 46 additional areas to cover particularly interesting features of the Galaxy. The plan was for each participating observatory to make such observations as its equipment was best suited to. Kapteyn's skill as a networker resulted in several observatory directors agreeing to participate, but worthwhile contributions were slow to emerge. A case in point was the Radcliffe Observatory in Oxford which agreed to determine proper motions for the selected areas in the Northern Hemisphere. The first plates were taken in 1909 May but the complete set of first-epoch plates required nine years. It required fourteen more years to take the second-epoch plates and measure and reduce them. Twenty-four years of work were required for 450 hours of exposure. It is no wonder that it moved to South Africa as soon as it had the chance.

George Ellery Hale (1868–1938) founded the Yerkes Observatory and the Mount Wilson Solar Observatory. Later he went on to include stellar astronomy with the construction of the 60-inch reflector. He was intent on staffing his observatory with the best stellar astronomers and offered the position of Research Associate to Kapteyn, who agreed to spend a few months every year at Mount Wilson while retaining his post and duties in Groningen. Kapteyn first visited in 1908 but the telescope was delayed; his time there was not wasted because he found the friendly atmosphere enabled him to pursue his own theoretical problems without interruption. Hale was working on the observational programme for the 60-inch, which was based on the plan of the selected areas. On completion the telescope was to have Newtonian and coudé foci, and two Cassegrain foci. Because the primary had no central hole they were 'broken' Cassegrains fed from a flat in the middle of the tube. One was *f*/15, intended for on-axis instruments like spectrographs, and the other *f*/20 for off-axis imaging. In those days there were no correcting lenses for large reflectors and the only remedy for asymmetric off-axis aberrations like coma was to use a large *f*-ratio. Unless the images are effectively circular, precise astrometry is impossible. The spectrograph was producing radial velocities for fainter stars which Kapteyn required for his investigations. Kapteyn's time at Mount Wilson was spent in collaborating with the permanent staff who were working on the plan, especially the star counts. The greatest difficulty was to measure magnitudes on a strictly logarithmic scale, when the blackening on the photograph plate was highly non-linear with respect to intensity.

When Kapteyn visited again, Mrs. Kapteyn accompanied him. The only accommodation available on the mountain top was strictly for men only in an aptly named 'monastery'. The couple lived in a small tent; there is a photograph of the couple outside their tent, with Kapteyn in his shirtsleeves, incongruously wearing a black bowler (derby), wing collar, and tie. The tent was later replaced by the 'Kapteyn Cottage'. The observatory's men-only policy persisted into the late 20th Century; eventually female graduate students were allowed to live in the cottage while observing.

Kapteyn visited Mount Wilson annually until the outbreak of war in 1914. The sea and rail trip took more than a fortnight each way. The Kapteyns visited their son, a mining engineer in Colorado, on most occasions and usually went through Chicago, Washington, and London to see old friends and expand their rapidly growing circle of new friends. Kapteyn frequently took students with him to California. Some of them made their careers in America while others returned to Europe, a pattern which has persisted to this day.

At the end of World War I the International Astronomical Union was set up. There was a general recognition that international co-operation was the best insurance against further conflicts. However, the hatred inflamed by the war had not yet died down and many astronomers on the winning side wished to exclude astronomers from the losing side. The Netherlands had remained neutral throughout the war and Kapteyn, who had friends on both sides, was strongly against the policy. Fortunately, common sense soon prevailed and the IAU became truly international a few years later.

This lengthy and detailed work will be an invaluable reference for historians of science. Further details can be found at www.rug.nl/JCKapteyn. — DEREK JONES.

Post-Planck Cosmology, edited by C. Deffayet *et al.* (Oxford University Press), 2015. Pp. 545, 25 × 17·5 cm. Price £42·50 (hardbound; ISBN 978 0 19 872885 6).

The full title is *École de Physique des Houches, Session C, 8 July – 2 August 2013, Post-Planck Cosmology*. Session C did not follow sessions A and B, but rather sessions XCVIII and XCIX; these are the proceedings of the 100th Les Houches summer school (the first was in 1951). There are twelve chapters by fourteen authors (one chapter has three, though only one is also a lecturer). Eighteen lecturers (and four seminar speakers, one of whom is an author of one of the chapters) are listed (as well as fifty-three students). Strangely, only the list of students is strictly alphabetical; the other lists are only almost so — too much so to be due to chance. Strange spellings in the French front matter ("Marie Curi", "physicque" —though these are sometimes spelled correctly) add to the surreal impression. Fortunately, though, this is confined to the front matter; the book itself, though not perfect, has been well edited. Despite having many authors, the appearance is quite uniform, with the exception of citations and references, where the style varies from chapter to chapter.

The chapters are arranged alphabetically by author (except that that by Renata Kallosh is last — perhaps because she was a 'seminar speaker' rather than a 'lecturer') and vary in length (from 16 to 86 pages) and level (from slightly technical to very technical). Seven could be described as "a lot of theory" and five as "somewhat less theory"; the five chapters in which observations play a substantial role also include information on numerical simulations, semi-analytic models, and so on. All in all, the topics are those which would be covered at a typical modern cosmology conference, though of course the Les Houches lectures, lasting almost a month, provide much more detail. There is also a considerable variation in style. For example, Andreas Albrecht's chapter on 'Cosmic inflation' notes that good reviews of the topic (by himself and by others) are available, so his contribution mentions the few things he finds most important (including his own 'de Sitter equilibrium cosmology'). Clive Burgess' contribution is similar to talks I have heard by him at the last two conferences I have attended, so I'm sure that he has spoken on this topic many times; his chapter is thus a presentation of his own current work (on a possible extra-dimensional solution to the cosmological-constant problem), rather than a review. About half of the chapters are typical reviews; the two longest (by Francis Bernardeau on 'The *Planck* mission' and Andrei Linde on 'Inflationary cosmology after *Planck*') are both thorough and readable. The shorter reviews provide good potted introductions to their topics, and the fact that most chapters have very many references (Burgess' has nine pages of references for a text just four times as long) makes this book useful for someone wanting to learn about a topic in depth.

Apart from the review of the *Planck* mission itself, "post-*Planck* cosmology" is not immediately obvious in most of the chapters; however, it is always present, since a common theme is that observations now provide very strict constraints on theory. *Planck* might even be the last word on this in some sense with regard to the CMB; for other areas of astronomy, upcoming satellites such as *Euclid* and ground-based instruments such as the *SKA* and the *LSST* will play a similar role. Cosmology has been data-driven for the past twenty years or so, but will probably become even more so in the next twenty. As a result, I think that the 'a lot of theory' chapters would be much the same if written twenty years from now, while the others will of course have to take into account vast amounts of new data.

The figures are all in black and white, though the captions often say "For the figure in color, please see the online version of the lectures." The location of these, however, is not mentioned in the book; the URL is https://sites.google.com/site/cosmologyleshouches2013/lecture-notes. (This page has links to an on-line 'dropbox', where the actual content is, and appears to be accessible without registration.) Some of them are slides, others are more similar to the proceedings (but with colour figures). Formats vary: 'raw' PowerPoint, PDF (some produced from PowerPoint, some from LaTeX, and so on, but also some scans of handwritten transparencies), and even a Mathematica 'notebook'. There is a huge amount of information available here, including material from some of the lecturers and seminar speakers who aren't represented in the proceedings. (Most of those who are represented have also provided on-line material.)

I didn't notice any factual mistakes, but that would be a surprise indeed from authors writing about their own work. There is no index, but that is not really a problem for a book like this. Notes are fortunately provided as footnotes, rather than being collected at the end of each chapter or at the end of the book. The overall production is good. I recommend the book for those who want a detailed overview of current topics in cosmology. — PHILLIP HELBIG.

The Singular Universe and the Reality of Time, by R. M. Unger & L. Smolin (Cambridge University Press), 2015. Pp. 543, 23·5 × 16 cm. Price £19·99/$29·99 (hardbound; ISBN 978 1 107 07406 4).

According to philosopher Unger and theoretical physicist Smolin, time is real and multiverses are not, at least the sorts promulgated by other cosmologists. All the rest, to quote Hillel*, is commentary. The commentary is a sort of 'duograph': 347 pages from Unger, 150 from Smolin, and a final 20 addressing eleven issues on which they differ. The differences are said "not (to) be compatible with adherence to the same directions of thought", yet minor when compared to what unites them and puts them in opposition both to Everett's many-world interpretation of quantum mechanics and to string theory.

Their starting points are reasonably clear (that is, consisting of words I think I know, in more or less that order). Thus Unger's "first cosmological fallacy" begins by saying "There is one real universe. Time is real, and nothing

*The folklore says that Rabbi Hillel responded to a request to explain the law while standing on one foot with a form of the Golden Rule, "That which is hateful to you, do not do unto another. The rest is commentary"; Bernard Shaw's version was "Do not do unto others as you would have others do unto you; their tastes may be different".

lies beyond its reach. Mathematics has the one real, time-soaked world as its subject matter and inspiration ... ." And Smolin's cosmological crisis consists at least partly of the long-standing failure to unify or at least reconcile General Relativity and quantum mechanics. Our inability to calculate properties of forces, particles, and the Universe is part of the deal.

The most important, testable, point on which they agree is that "the laws of nature evolve, and they do so through mechanisms that can be discovered and probed experimentally because they concern the past", although no very explicit examples are given in the closing thoughts from Smolin. In between come phrases like "retrospective teleology" (applied to the anthropic principle; Unger is against it) and "every instance of a qualia occurs at a unique moment in time" (Smolin is in favour of this; qualia must be both singular and plural; and they are roughly what were called "sense impressions" back in introductory psychology in the 1960s).

You will meet in these pages many of the sages you expect, including Wigner on the "unreasonable [but not infinite, say the authors] effectiveness of mathematics"; Kant, Lorentz, Einstein, and others on space–time (where the latter might create the former, but not conversely); Gödel and Turing on various kinds of incompleteness; and Heraclitus and Bergson on change. Marx is less obvious! I would have liked a glossary (the diagonal argument? the Planck mass?), some micro-biographies (who was Charles Sanders Pierce?), and a slightly better index (find Heraclitus by looking up Bergson!).

Who is the intended readership? It is not, say the authors, necessary to be either a philosopher or a scientist to appreciate their arguments. One must, however, be brave. If you can read Pierre Teilhard de Chardin, you can probably read Unger & Smolin, and be surprised at how such very different sorts of ideas can be expressed in rather similar phrases. I had meant to compare their comments on the arrow of time with ones included in a talk at the April meeting of the American Physical Society, but the sketchy index prevents relocating the remark.

Conflict of interest? For once, mine is actually a review copy sent by the publisher, though it does not carry the customary red warning against resale. The price, however, is modest given the length and the presence of a few equations (which take $c = 1$) so I am not tempted, the more so as I seem to have written all over the margins and fly leaves. Indeed, I'll probably have another go at understanding more of the authors' words in their order sometime in the future. — VIRGINIA TRIMBLE.

Statistical Methods for Astronomical Data Analysis, by A. K. Chattopadhyay & T. Chattopadhyay (Springer, Heidelberg), 2014. Pp. 349, 24 × 16 cm. Price £76·50/$109 (hardbound; ISBN 978 1 4939 1507 1).

Astrostatistics is a topical field, in which there are relatively few textbooks. This book covers statistical methods from a frequentist perspective, with examples drawn from astronomy. It is an ambitious project, which has been only partially successful. The book starts with a courageous attempt to cover almost all of astronomy in 80 pages, with mathematical results. I am not sure how useful that is — unless one knows the material beforehand, it is really impossible to get any depth of understanding from such a short section. In places there is some formal development of the mathematics, but mostly concepts are introduced more informally as reasonable suppositions, and there are places where some more justification would be helpful, to build insight. An example of this is in linear regression, where least-squares minimization is advocated, without any discussion of when this would be appropriate and when it might not. A general comment is that it needs a bit more detail — topics are often introduced but then not elaborated to the depth needed to give full understanding. The links to astronomical data-sets are useful, though — more so than the pages of printouts of numbers that are surely not going to be used in that form. — ALAN HEAVENS.

Explore the Cosmos like Neil deGrasse Tyson, by C. A. P. Saucier (Prometheus, Amherst), 2015. Pp. 120, 20·5 × 13·5 cm. Price $14·99 (about £10) (paperback; ISBN 978 1 63388 015 3).

This book has been written with the intention of inspiring children (recommended ages 8–12) to develop an interest in space science. Has it hit its target?

For a relatively slim book, a wide range of historical and contemporary ideas are covered, interwoven with a potted biography of Neil deGrasse Tyson. This is an interesting approach as it personalizes the scientific concepts and helps make the study of space science more relevant to the reader. The language level, with short sentences, has been well tailored to the target age group and scientific explanations are clear if somewhat simplified at times. However, this structure does disrupt the flow of the narrative in places, giving it a rather random feel. Perhaps this is not such a problem for 8–12 year olds as it is for this somewhat older teacher? Neil deGrasse Tyson himself is clearly a well-known 'space celebrity' in the USA where he appears on TV, writes popular science books on astronomy, lectures, and runs the Hayden Planetarium at the American Museum of Natural History — a sort of American Brian Cox — and I understand that he does have an international profile courtesy of social media. This means that the book has a very American bias in its storyline and the examples it draws on for inspiration. For that reason, much of the relevance may be lost for a readership of British children.

There are relevant, coloured illustrations throughout the book, but they are not referred to in the text and so depend on their own descriptions. There is a bibliography including many internet references and a clear glossary of terms. The information is up to date to 2014, though even then some points have been superseded.

Overall, the book contains good introductions to many areas in astronomy and astrophysics and looks forward to future developments, but is not necessarily a very coherent read. — DEBRA HOLTEN.

Physics of the Plasma Universe, 2nd Edition, by A. L. Peratt (Springer, Heidelberg), 2015. Pp. 406, 24 × 16 cm. Price £117/$179 (hardbound; ISBN 978 1 4614 7818 8).

Anyone who has spent time engaged in research or in teaching in the field of plasma physics will know that it contains a wide variety of elements, both in terms of the physical theories that support it and the phenomena to which it can be applied. Consequently, textbooks on the subject come in a wide variety too. At one end of the spectrum, there are those that develop the fundamental kinetic or fluid theories systematically and progressively through several chapters; at the other end there are those that focus on phenomena, usually with a particular theme as a focus. This book is very much in the latter category, although as its title suggests, it is aimed at many different aspects of plasma in the Universe, ranging from laboratory-based experiments, through the terrestrial aerospace environment, to stellar and galactic plasmas. This book is encyclopaedic in its coverage, in terms of the sheer variety and breadth of plasma phenomena it deals with. It begins with a survey of the plasma universe and then there follow chapters on field-aligned currents, magnetic fields, electric fields, plasma structuring, radiation emission, and so on. Each chapter includes many examples of those themes from a number of different parts of the plasma universe.

I suspect that this book is going to please some, but frustrate others. Those looking for a systematic development of plasma physics applied in a progressive way are likely to be disappointed. On the other hand, those looking for an eventful up-to-date guided tour through some of the key sites of the plasma universe may well find what they are looking for here. However, the reader should be prepared for a somewhat quixotic adventure. The author appears intent on covering just about everything plasma, as his title suggests, and succeeds, after a fashion, in touching on nearly everything one could think of in the plasma universe. It mixes the elementary with the esoteric, the bread and butter with the exotic. Two final points: the inconsistencies in mathematical notation from one page to another and the quality of some of the graphics leave a lot to be desired. — Terry Robinson.

Solar Cosmic Rays: Fundamentals and Applications, 2nd Edition, by L. Miroshnichenko (Springer, Heidelberg), 2015. Pp. 521, 24 × 16 cm. Price £153/$229 (hardbound; ISBN 978 3 317 09428 1).

The second edition of this textbook was written over a six-month period during 2013 and 2014, and follows the first edition which appeared in 2001. Leonty Miroshnichenko has worked on solar cosmic rays for many years and is a prolific publisher, though until recently mainly in Russian journals. The book covers observational data in some detail but with extensive chapters on the acceleration mechanisms of cosmic-ray particles and their interaction in the solar atmosphere and interplanetary medium from their origin. Some space is devoted to what was regarded as the 'solar-flare myth' (*i.e.*, that solar flares are incidental to particle acceleration but coronal mass ejections are the key) which might intrigue younger readers. There is a surprising lack of mathematical detail in the description of the acceleration mechanisms, which is certainly a point in its favour as this is a textbook described (by its author in the introduction) as aimed at graduate lecture courses and possibly to postgraduates also who are not necessarily specializing in this field.

Looking in more detail, however, one can see many drawbacks which detract from its worth. First, the text is in dire need of more rigorous proof-reading — the English is stilted, with missing definite and indefinite articles, misprints abound, and explanations of many basic concepts are of poor quality. Thus, it is hard to see what an undergraduate would make of the few-sentence-long description of frozen-in charge states in the interplanetary medium on p. 42, or the definition of X-ray events that are impulsive (often less than an hour) or gradual (many hours) on p. 34, but with second thoughts a couple of pages further on when impulsive flares now have durations of minutes and gradual events with durations of hours and days. Perhaps the worst aspect of the book, certainly the one that is most immediately obvious, is the dreadful quality of some of the illustrations. Most appear to be copied from already-published papers, and badly so. Some of the line drawings are crucial to understanding concepts, yet are so badly drawn that it is difficult to make out what is being emphasized. Among the small number of coloured illustrations, the *LASCO* coronagraph (on the *SOHO* spacecraft) image on p. 86 is barely recognizable as such. Like many specialized fields, acronyms are very common and there is a list to guide the reader in the Appendix (though not fully comprehensive). A glossary might have helped students of the subject, but the only advice offered is the one of Bruzek & Durrant dated 1977.

For the price the book is hardly good value and is in desperate need of a reprinting with the many deficiencies removed. — KEN PHILLIPS.

Nature's Third Cycle: A Story of Sunspots, by Arnab Rai Choudhuri (Oxford University Press), 2015. Pp. 281, 22·5 × 14·5 cm. Price £22 (hardbound; ISBN 978 0 19 967475 6).

Here's a splendid book on sunspots, the 11-year solar cycle, and the solar dynamo, written by a leading expert, Arnab Rai Choudhuri, a theoretical astrophysicist at the Indian Institute of Science in Bangalore. Many of his published research papers are on the generation of solar magnetic fields by the dynamo process and the formation of sunspots. His graduate textbook on astrophysical fluids and plasmas (CUP, 1998) is widely adopted. *Nature's Third Cycle* is probably unique as a non-technical presentation of the physics of the solar cycle. The narrative style is that of a personal journey of discovery, which works very well in this case. There is much personal detail that enriches his story of a life in science, with the author showing how he took part in some of the remarkable advances made in the subject over the past three decades. Choudhuri is full of praise for his thesis supervisor, Eugene Parker, who inspired his career. He acknowledges "Having the good fortune of doing my PhD under arguably the greatest theoretical scientist in our field." The author's account of the historical development of sunspot science and solar-dynamo theory is first rate in terms of accuracy as well as the precision of the language used to describe difficult concepts verbally. Readers of this book will learn all about the thrill of doing research in astrophysics, and they will learn much about the operation of the solar dynamo and the 11-year cycle. Choudhuri is strong in his explanations of the basic physics behind the observed phenomena. This is not a mass-market popular-science book, but it is an instructive source for beginning undergraduates thinking of specializing in astrophysics for the latter years of their degree, to whom I recommend it without reservation. — SIMON MITTON.

Eclipses, Transits, and Comets of the Nineteenth Century, by S. Cottam & W. Orchiston (Springer, Heidelberg), 2015. Pp. 336, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 08340 7).

When I got this book, I thought it was simply a review of eclipses, transits, and comets in the 19th Century with a bias towards those events seen from the United States. Having read the book, I am pleased to say I was wrong. The book is so much more than that. It is about how astronomy caught the imagination of what was a predominantly agrarian nation at the beginning the 19th Century and set it on the road to becoming the major scientific power that it is today.

The book covers the growth of professional astronomy and the role of amateur astronomers in the popularization of astronomy in the United States. During the period 1850–1910, the United States witnessed seven total eclipses of the Sun, two transits of Venus, and seventeen comets, spurring a huge demand for information from the general public that was satisfied both by charismatic professional astronomers as well as amateur astronomers who provided newspaper articles, books, booklets, and observing nights to introduce people to the night sky. Professional astronomers from the United States also took part in many of the great observing expeditions, including those for the century's two transits of Venus.

The book is in seven chapters covering astronomy until the 1860s including the Leonid meteor storm of 1833, the scientific context of the first half of the 19th Century, the solar eclipses of 1868, 1869, and 1878, the transits of Venus in 1874 and 1882, and a comprehensive section on the role of professionals and amateurs, their writings, telescopes, observatories, as well as the growth of astronomical societies. Astronomy in the United States has come a long way from the short-lived *Sidereal Messenger* of 1846 and the first appearance of the *Astronomical Journal* in 1849.

I am not a fan of dry historical texts but this was a fascinating read as well as a very informative review. At £90, it is not cheap, but it is an excellent survey of American astronomical activity in the 19th Century. The book is very well illustrated, mainly by portraits of the key players of the period, and comes with a reference list covering nearly 40 pages. If you are interested in the development of science and how the astronomical awareness of a continent developed from something "primarily for the vain art of astrology and for almanac production" to having the huge number of amateur astronomers it has today then this book is for you. — STEVE BELL.

Here and There

GRAVE ERROR

… Rosse's picture [of M 51] was a sensation when first exhibited in 1845; contemporary reports included William Herschel … experiencing "a delight he could not express" when encountering it. — *The Times*, 2015 April 1, p. 56.

MORE KNOWLEDGE AND LESS EDUCATION

… the promotion of 'literacy' in science, technology, education, and mathematics (STEM). — *The Observatory*, **135**, 112, 2015.