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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2011 May 13 at 16^h 00^m
in the Geological Society Lecture Theatre, Burlington House

R. L. Davies, *President*
in the Chair

The President. Our first speaker is William Chaplin from Birmingham, who will speak on 'Asteroseismology of solar-type stars and the NASA *Kepler* mission'.

Dr. W. Chaplin. It is currently a very exciting time for those of us whose research is focussed on the study of stars, thanks in large part to the wonderful photometric data being collected by the *Kepler* mission. Today, I am going to give a brief overview of the exciting new insights that are being made possible by *Kepler* in asteroseismology, the study of stars by observation of their natural, resonant oscillations.

Kepler was launched in 2009 to monitor — to exquisite levels of accuracy and precision — the photometric brightness of around 150 000 stars in the Cygnus region of the Orion arm of our Galaxy. The primary objective of the mission is to detect potentially habitable planets orbiting solar-type stars (*i.e.*, stars whose intrinsic properties are similar to those of the Sun). In addition to the exoplanet studies there are programmes dedicated to stellar physics, the largest being that conducted by the *Kepler* Asteroseismic Science Consortium (KASC). KASC is an international collaboration, led from Aarhus University (Denmark), comprised of 500 scientists. We already knew prior to *Kepler* that oscillations are a ubiquitous feature of stars right across the Hertzsprung–Russell (stellar classification) diagram. As such, KASC is divided into fourteen working groups, each group coordinating research on a different class of oscillating star. By far the largest group (currently comprising 170 scientists) is that dedicated to the asteroseismic study of solar-type stars, and it is led from Birmingham by me.

In today's talk, I report that the asteroseismic survey of solar-type stars that we undertook during the first few months of the *Kepler* Mission has met with unrivalled success, yielding clear detections of solar-like oscillations in more than 500 such stars. This represents a significant advance for the scientific investigation of solar-type stars, as we reported recently in *Science*.

The solar-type oscillations we have detected are the visible manifestations of standing waves in the stellar interiors. Stars like the Sun, whose outer layers are unstable to convection, display 'solar-like' oscillations excited by turbulence in the outermost layers of the convection zones. These oscillations are predominantly acoustic in nature, and probe all the way to the cores of the stars. The dominant periods are minutes in length and give rise to variations in stellar brightness at levels of typically just a few parts per million (hence the need for *Kepler*'s exquisite precision). Solar-type stars present a rich spectrum of overtones, and measurement of the frequencies provides stringent constraints on the fundamental stellar properties (*e.g.*, mass, radius, and age), which may be measured to levels that are hard to achieve by other means. Asteroseismology also offers the unique opportunity to map the internal structures and dynamics of these stars.

The study of stars is of central importance to astrophysics. The calibration of distances on extra-galactic scales, fixing the ages of the oldest stellar populations (which place very tight constraints on cosmologies), and tracing the chemical evolution of galaxies (including our own), are all vulnerable to uncertainties in stellar evolutionary modelling. Asteroseismology will reduce significantly these uncertainties, and therefore has the potential to re-set the foundations of these diverse and crucial areas of astrophysics. Of vital importance are methods we are developing to use asteroseismology to measure stellar masses and radii in a way that is independent of stellar-evolution theory.

Stars are sources of energy for giving conditions that can support life, and the exciting developments in exoplanet discoveries of course go hand-in-hand with detailed studies of the host stars. Asteroseismology can be used to constrain the sizes of planets discovered with the transit method by *Kepler* (from the accurate measurement of the radii of host stars), allowing the habitable zones to be fixed around the stars. Asteroseismology can also determine the ages and constrain the dynamical histories of these stellar systems, to unprecedented levels. Asteroseismic observations with *Kepler* will allow us to follow stellar cycles and to trace how levels of activity change as stars age, giving insights on planetary habitability and key information for understanding the variability shown by our own Sun (which has recently been in a quiescent state that is unique in the modern satellite era). We will soon be able to measure the depths of the convective envelopes in some of the *Kepler* targets, which is crucial information for dynamo theorists seeking to explain the visible manifestations of magnetic activity in solar-type stars. Interestingly, we can already see widespread evidence for magnetic activity suppressing the amplitudes of the solar-like oscillations of the more active stars in our *Kepler* ensemble.

Prior to *Kepler*, solar-like oscillations had been detected in around 20 stars. Our *Kepler* sample of over 500 solar-type stars represents a huge step forward. It will allow us to follow the evolution of solar-type stars (for example by selecting from the sample a sequence of stars of very similar mass and composition). The sample is sufficiently numerous to allow us to perform statistical studies, for example, tests of stellar-population models.

Previous studies of stellar populations have been hampered by not having mass estimates of individual stars. Precise estimates of masses of solar-type stars had been limited principally to stars in eclipsing binaries. The *Kepler* estimates add significantly to the total, and in numbers that are large enough to allow us to perform statistical population tests using direct mass estimates, which has not been possible before. We used asteroseismology to estimate the masses and radii of all 500 stars in which we detected solar-like oscillations: while the observed and modelled radii distributions were remarkably similar, we found notable differences in the masses. The next challenge for this strand of our work is to pin down the origins of these differences, *i.e.*, whether they lie in the physics of the stars or in assumptions used concerning the star-formation rates and histories in the synthesis modelling.

Exciting challenges lie ahead: we are just now starting to analyze multi-month *Kepler* datasets, and the near future offers the prospect of our being able to study multi-year datasets on the best asteroseismic targets. The future looks very bright for stellar physics.

Professor D. Lynden-Bell. Can you tell us about binary stars? Are there any in this set you've looked at, or not? Because they can be compared directly *vis-à-vis* masses, *etc*.

Dr. Chaplin. Yes. There is a *Kepler* working group, which is looking at oscillations in stars and binaries. There are a few examples of stars, of binaries which have red giants, which show solar-like oscillations, and there we can actually use the oscillations to get a measure of the properties of the stars and compare those with what we get from the binary orbits. The *Kepler* Science Team also has a list of solar-type binary stars which potentially can be studied using asteroseismology. So, one of the things that we want to do is to take stars that are oscillating in binaries and use the fundamental properties of the stars we can get from the binaries, which involves Kepler's laws — nothing at all to do with stellar-evolution theory — actually to validate some of the methods we're using in asteroseismology.

Professor M. Kendall. Are you seeing any evidence for mode splitting? I mean, something about 3-D structure, or non-1-D structure?

Dr. Chaplin. For the solar-type stars, the data are now extensive enough to see evidence for rotational splitting. For red giants, we've had the benefit that we don't need to rely on the high-cadence photometry; and so there we already have access to data covering more than a year, and evidence has been found for rotational splitting in those data as well. We hope that in some of the best stars, we'll be able to say something about the latitudinal dependence of the rotation, as for the Sun, where there is a very marked dependence in the outer envelope of rotation with latitude.

Dr. Lyndsay Fletcher. What does the discrepancy between the mass distributions observed and predicted imply for stellar structure?

Dr. Chaplin. We don't know yet; we're working on that. The first thing we need to do is to check that our estimates of mass are right, and it's not something wrong in what we've done: we don't think that's the case. Then one must address whether it's in the population synthesis, and there you have a whole variety of ingredients: you have to model the Galaxy, thick disc, thin disc, the initial mass function of stars, and the star-formation rate; then there are the ingredients that go into building new individual stars themselves. So watch this space.

Mr. M. Hepburn. Micro-hertz isn't a terribly familiar unit for most of us. Am I right in supposing that 2500 micro-hertz is an oscillation of a period of several minutes?

Dr. Chaplin. That is correct.

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

Our next speaker is Michelle Collins from Cambridge, and she is going to tell us about 'The thick disc in M31 — probing the evolutionary histories of spiral galaxies'.

Ms. Michelle Collins. How do large spiral galaxies like our own Milky Way and her neighbour, Andromeda, form and evolve over cosmic time? This is one of the great outstanding questions in astronomy today and while the broad-brush picture of this process is in place, the underlying details are still lacking.

One of our best hopes for gaining insight into this complex evolution lies in our own backyard. By pointing telescopes at the outer environs and inner-disc regions of our own Galaxy, and others nearby, we can accumulate detailed information for individual stars, including their positions, velocities, and chemistries. This is known as near-field cosmology, and such studies are being extensively carried out within our own Galaxy. However, using only the Milky Way as a basis for the evolution of all large spirals is potentially unwise. Thankfully, we have another spiral galaxy located only 785 kpc from us, the Andromeda galaxy, which we can also use in this way. With a similar total mass to the Milky Way, Andromeda has often been described as a sister to our own Galaxy, but there are several important differences between the two. One is simply a matter of observability. While the Milky Way offers a wide range of nearby stars for us to study in detail, we are unable to get an overarching view of its structure due to obscuration along various lines of sight by the Galaxy itself. With Andromeda, we are afforded a panoramic view of the complete system, from the bulge and stellar discs, into the farthest reaches of its stellar halo.

The Pan-Andromeda Archaeological Survey has been conducting an imaging campaign using the *Canada–France–Hawaii Telescope* to survey more than 400 square degrees of sky in the Andromeda–Triangulum region, mapping out the distribution of red-giant-branch (RGB) stars throughout the discs and haloes of those galaxies. The resulting maps have presented us with a beautiful image of galaxy formation in action. To complement this survey, we performed spectroscopic observations in the region with the *DEIMOS* Spectrograph mounted on *Keck II*. We now have over 110 fields of data giving us velocities and metallicities for approximately 18 000 RGB stars in the region. This has led to many exciting results, and today I am going to focus on the kinematic detection of a thick disc in Andromeda.

Thick stellar discs are of inherent interest as they are thought to probe the earliest epochs of disc formation in spiral galaxies. If we can understand their formation and evolution we can gain a better understanding of spiral systems. Our Galaxy has such a disc, and from observations of nearby edge-on spiral galaxies, it is thought that thick discs are a ubiquitous feature of disc galaxies. Until recently, we have been unable to detect such a disc in Andromeda. If such a component existed, it would provide us with another system where we can gather detailed information from the individual stellar constituents to see how they stack up against different proposed formation mechanisms.

Over the past couple of years, I have been leading a study to detect and analyze such a component in Andromeda using the products of our *DEIMOS* survey. By analyzing the velocities of stars in 21 fields in the south west of the galaxy, we looked for the kinematic signature of a thick disc, that is, stars rotating more slowly than the thin disc with a larger velocity dispersion (or velocity spread) than this thin component. To do this, we fit multiple Gaussians to our velocity information and determine how many components best fit the data. For our sample, this technique fixed on three components in the majority of our fields: a thin disc, a halo, and an intermediate population with a larger dispersion than the thin disc, rotating approximately 50 km s^{-1} slower than the thin disc.

We then observed how the velocity lag and velocity dispersion for the intermediate component evolved throughout the sample to see if it truly mimicked the behaviour of a thick disc. We found that the velocity lag and dispersion were roughly consistent throughout all our fields, and that the thick disc had a velocity dispersion of 50·4 km s^{-1} compared with 32·1 km s^{-1} for the thin disc. Both discs are therefore hotter than those of the Milky Way. We also inferred the scale heights of the discs in Andromeda using relationships between scale heights and scale lengths for thin and thick discs in external galaxies, giving us scale heights of 1·1 kpc for the thin disc and 2·8 kpc for the thick disc. These are roughly three times the corresponding Milky Way values.

We used our composite spectra to measure the metallicity of the thick disc and compare it to the thin disc and halo. We find that the thick disc is distinct from both these components, being more metal-poor than the thin disc and more metal-rich than the halo, as seen in the Galactic system. We combined this information with photometric metallicities to get a handle on the difference in ages and abundance of alpha elements (elements lighter than iron) between the two discs. This analysis implies that the thick disc is older than the thin disc by about 2 billion years, and contains more alpha elements, similar to what we see in the Galaxy.

Finally, we can compare our findings for the Andromeda thick disc to predictions from the four main formation models for such a component, which are: heating of an initial thin disc by minor mergers; heating by internal processes, such as interactions with spiral arms; the accretion of a satellite galaxy on a coplanar orbit; and a disc that forms thick *via* significant star formation occurring out of the plane. At present, our results are consistent with all of these mechanisms. However, as we include more data, we hope to improve on this, and begin to pin down the evolutionary route taken by the structure.

The President. Questions on the M31 thick disc?

Mr. Hepburn. You mentioned the dispersion in the speeds of these stars, at somewhere around 40–50 km s^{-1}: are they all moving at roughly the same velocity around the galactic centre, or is there some fall-off as you go further out? Does it stay the same as it does for a long way in our Galaxy, or does it vary?

Ms. Collins. There isn't a lot of variability as you go further out; it's fairly similar to what you see in the Milky Way.

Mr. Hepburn. And that's true even for the thick disc?

Ms. Collins. Yes, from what we can tell from the thick disc: the measurements are less secure, because you have fewer stars, but it seems to be very similar.

Mr. N. Calder. What's the effect of the thick disc in the story of dark matter? There's supposed to be an effect from dark matter on the velocities of the stars. Does the presence of a thick disc alter the details of that story?

Ms. Collins. Not as far as I know. As I said, in all the spiral galaxies where we've looked for a thick disc, we have found it. They seem to be able to be modelled with ΛCDM quite nicely, I think, but I am not sure about the details.

Rev. G. Barber. Connected to that, what's the total mass of the thick disc, and indeed in the extended disc, relative to the rest of the galaxy?

Ms. Collins. For the thick disc compared to the combined thin-disc–extended-disc — it's about 30%, I think. And, in terms of the total galaxy mass, I think the total mass estimate for M31 is around about 10^{12} solar masses, so there is significantly less — we're looking more at 10^{10} solar masses.

Rev. Barber. And the mass in the extended disc, where you have a much bigger volume — is some of the dark matter involved in these discs?

Ms. Collins. You mean dark matter within the disc?

Rev. Barber. Dark matter is just mass we don't know; so perhaps the extended disc, or the thick disc, is part of that mass?

Ms. Collins. It could be, but it's a small component of the mass because there are fewer stars within the thick disc; the mass of stars there is lower than the mass of stars in the thin disc.

Dr. S. Mitton. I was just going to comment that in 1886, at a Friday meeting of the Society, the amateur astronomer Isaac Roberts created an absolute sensation by displaying a black-and-white photograph of the Andromeda Nebula. [Most probably the meeting on 1886 January 8, reported in these pages in Vol. **9**, page 82. — Ed.] That photograph looks very similar to the black-and-white photograph we all are familiar with. It was the first photograph to show the disc structure and the spiral structure of the Andromeda Nebula, and it's across the courtyard, in the archives of the RAS.

The President. Thank you, Simon. I have a question: your alpha-element abundances are high, so doesn't that imply a rather compressed star-formation time-scale, and doesn't that limit some of your model possibilities?

Ms. Collins. You mean the alpha abundances of 0·2 and 0·4? They are high, but then I think you could probably tweak the values of the ages and get different values, so I think the constraints are not that tight at the moment. You can still tweak and say there's definitely an offset in alpha abundances between the two populations, but whether they are as high as 0·2 and 0·4 or lower, more like 0·0 and 0·2, say, is open.

The President. So can you can get away with a constant star-formation rate in both the disc of M31 and in this thick disc?

Ms. Collins. That, I'm not sure.

Professor P. Murdin. I'll just add a comment to Simon's remark. Isaac Roberts is buried in a cemetery in Birkenhead. He has a very large memorial which has got his astronomical pictures carved on it, and I think the Andromeda galaxy is one of them. That must be the only disc galaxy carved on a tombstone! [Laughter.]

Mr. M. F. Osmaston. Needless to say, I can't really explain it, but last year at the JENAM meeting in Lisbon, I had a poster which argued just exactly what you have been observing, namely that you expect the metallicity to go down as you go outwards.

The President. Let's thank Michelle again. [Applause.]

So we now move to the 2011 Harold Jeffreys Lecture, and before I introduce Lyndsay Fletcher, let me congratulate her on being elected Secretary of the Society earlier today. She is going to speak on 'The Sun at high energies'.

Dr. Fletcher. [It is expected that a summary of this talk will appear in a forthcoming issue of *Astronomy & Geophysics*.]

Dr. R. C. Smith. I am sure this is a very simple-minded question, but you showed an image of hard-X-ray and gamma-ray emission, which were just slightly in different places, and you were saying the X-rays come from electrons and the gamma-rays from protons. Is it so surprising that they're accelerated in opposite directions?

Dr. Fletcher. It's not necessarily that they are accelerated in opposite directions: they are spatially separated from one another. Whether or not they're moving in opposite directions really depends on the magnetic configuration in which this is taking place. I mean, if you thought it was taking place in a loop, with a field-aligned electric field, then you might expect all of the electrons to go in one direction, and all of the ions to go in another, for example. But it's a more complicated situation than that. It's probably not surprising that they are possibly accelerated in different locations, because the conditions, generally speaking, for accelerating electrons and ions are quite different. You can accelerate ions with a particular frequency of Alfvénic turbulence, for example, but you can't do the same with electrons, as they're not in resonance. So electron and ion acceleration may require different processes.

Professor Carole Jordan. You've mentioned the magnetic-field configuration quite widely; the *Michelson Doppler Imager* (*MDI*) on *SoHO* must surely be able to measure the strength of the magnetic field. What has been done on the distribution of the strength of the magnetic field, in the flaring regions and in the surrounding active regions? Of course that's crucial to the sort of geometry you were showing, and the cartoon you've shown is clearly the one you can rule out! As you've said, quite correctly, to make it work you require all the energy of everything in the corona. You could do the same with active regions: you could take the whole energy in an active region, if you know the magnetic field, the energy stored, and it won't work; the crucial thing is the emergence of new flux. However, do you have really nice observations of magnetic-field strength?

Dr. Fletcher. Let me address the question of what's known about the distribution of the magnetic field in the corona. Working this out is an amazingly difficult task. You do have the *MDI* distribution of the magnetic field in the photosphere which gives the line-of-sight field — in fact, what you need is the vector-field distribution, which is a noisy observable. From that, you have to do a calculation, a magnetic extrapolation, which is itself a difficult task: solving a set of non-linear vector equations. But ideas are converging about this, and when you do that, you find, at least in the active regions where this has been done, that the energy is stored low down: it's stored very close to the chromosphere, within maybe the first 12 000–15 000 km. It's not stored throughout the whole coronal volume; the currents are all low down, they are around the filaments in the active region.

Professor Jordan. It's just not stored at all. I mean people argued for 20–30 years about where the energy is stored. I think the whole point is that there is new flux emerging all the time. But to get the energies you need, and the fields you need, you have to have the emergence of a new bit of very strong flux, which is why they are in tiny regions.

Dr. Fletcher. Indeed, there is field emerging all the time, but very close to the magnetic dividing lines the field is very strong, you can calculate how much free energy is there. And there is enough accumulated over a couple of days to power a flare; but it comes from low down, which in fact is even more puzzling because then you really have to wonder where the electrons are coming from.

Mr. H. Regnart. I appreciate that your research is not related to fusion processes in the centre of the Sun, but is it possible that the work you and colleagues are doing on the behaviour of electric and magnetic fields might turn out to be of use to people who are trying to work on fusion using electromagnetic confinement?

Dr. Fletcher. Yes; I think the answer to that question is yes. There is a strong interaction between the two communities. Philippa Browning is one person, and there are others in Warwick, and I have myself worked with the Culham fusion labs. There are two aspects to this: first of all is understanding the stability of the confinement magnetic field, so this is really an MHD problem; the other thing is trying to understand what happens when that confinement breaks down, and when magnetic energy is released and accelerates particles in a disruption. The physics is probably very similar; the topology, in fact, is very different. But there is a cross-talk — probably not as strong as it should be — between the two communities.

Professor Murdin. I'm remembering my research-council heritage in putting the following observation to you, which I invite you to disagree with. You showed wonderful observational data sets, of marvellous complexity, and great beauty as well. And yet you had difficulty in identifying the basic physical principles that were going on; at least, perhaps you do understand the basic physical principles, but connecting it all together into a theory of these structures, and certainly, producing a theory of the whole of the Sun and the way these events propagate and carry on — that's really difficult, and you were frank about putting forward the limitations of the knowledge there. So what you need is to understand those phenomena by use of very intelligent people and possibly great computing power: large numbers of brains, big computers. And you *don't* need any further observational data! [Laughter.]

Dr. Fletcher. I have to answer this very, very carefully, don't I? [Laughter.] What I would like to say is I think that for some purposes we really have got more than we can deal with, but there are other things of which we're extremely ignorant. For example, what is happening in the microwave régime? Microwaves are very important because they tell us both about the electrons and about the magnetic field, simultaneously. That would be a wonderful resource to have, the proposed *Frequency Agile Solar Radiotelescope*. Also, what we don't have for flares, unbelievably, are visible-to-near-UV diagnostics of the chromosphere. We don't know what the plasma properties are in a solar flare nearly as well as we know what the plasma properties are in a stellar flare; in a stellar flare you have wonderfully resolved spectra where you see all the Balmer series, you can measure Stark broadening, you can do abundance analyses, yet we don't have that in the Sun.

When it comes to imaging, I agree with you, we're doing very well. The *Solar Dynamics Observatory* (*SDO*) is superb. I think it is approaching the resolution where you're really seeing the fundamental scales of the foot-points, the locations where the energy is being absorbed. And you're seeing multi-wavelength aspects of how the magnetic field is changing. But, really, our lack of optical and ultraviolet spectroscopy and radio-frequency observations is hampering us in trying to understand the whole process. So until we have a better understanding of existing data, I think it's probably time to hold fire on much more high-resolution whole-Sun images, but there are processes happening on time scales, and on spatial scales, which cannot be captured by the *SDO*. There are huge gaps in wavelength where we are just not probing, and that's really where I think we need to look next.

Professor Lynden-Bell. I want to ask if it is known whether most of the energy comes out mainly in ions, or in electrons?

Dr. Fletcher. We know that in some flares it's mostly ions, and in other flares it's mostly electrons. But, taken over the whole population of flares in which this has been measured, it's round about 50–50.

The President. Let's thank Lyndsay for a literally magnetic talk! [Applause.] So it just remains for me to remind you that there is a drinks reception in the library immediately following this meeting, and that we will meet again for an open meeting of the Society on Friday, October 14.

JOVICENTRICITY IN THE SOLAR SYSTEM: THE HISTORY OF A DISCOVERY

By Ivan Kotliarov
Higher School of Economics, St. Petersburg, Russia

I present an analysis of the history of discovery of the symmetry around Jupiter in the Solar System and a description of its mathematical formalization.

Introduction

The problem of hidden harmony of planetary distances is indeed one of the oldest and most tempting mysteries in astronomy. The search for this harmony goes back at least to the Pythagoreans. This search helped Johannes Kepler (after his failed attempt to establish a correspondence between planetary distances and Platonic solids) to discover his famous laws, and, indirectly, Newton's Law of Gravitation. But any field of science may become obsolete — as was the case of the problem of spacing of planets within the Solar System. The last important result in this field was the Titius–Bode Law that 'helped' to discover the asteroid belt, but is now considered by most astronomers as a simple mnemonic rule and an intellectual game with integer numbers (no more than an exercise in numerology). This perception of the previously famous law is due to its unclear physical nature (many astronomers believe that this law has no physical basis at all and is a mere coincidence) and big discrepancies between observed and calculated values of semi-major axes for outer planets and dwarf planets (Neptune, Pluto, and Eris). It is interesting to note that *Icarus*, a major journal in planetary science, has stopped accepting papers on the Titius–Bode Law.

However, studies in the field of the Titius–Bode Law still continue in two directions. Professional astronomers keep trying to find out if there are any physical explanations for the Titius–Bode Law. These studies include attempts at demonstration of the physical nature of this law[1–3], attempts at demonstration of its statistical significance[4,5], and attempts at demonstration that Titius–Bode-like laws do exist in different planetary and satellite systems[6,7] (obviously the list of references is far from being exhaustive). Results obtained in these studies are well represented in scholarly literature and are cited by specialists.

There are also attempts to find an 'improved' formula of planetary distances that could include all planets of the Solar System. Search for such a formula was relatively popular among professional astronomers, but now it is left to amateurs. Results obtained in this field are virtually unknown to specialists (some of these results are published in hard-to-find books and journals in Russian and are therefore hardly accessible to the international astronomical community) despite the fact that they are interesting for the history of astronomy and may be instructive for professionals. These results include the Jovicentricity (or Jovian symmetry) of the Solar System independently discovered by Kirill Butusov[8] and Sven-Ingmar Ragnarsson[9].

This paper is devoted to an analysis of formulae describing this Jovian symmetry. I will not discuss their physical nature; it is enough to mention that Jupiter contains approximately 71% of the total mass of our planetary system, and this gravitational domination is expected to produce orbital (near-) resonance that could be described by formulae similar to ones proposed by Butusov and Ragnarsson.

Butusov's Law

In 1973 Kirill Butusov, an astronomer from the USSR, proposed a new structural law[8]. This law is almost unknown outside Russia as all publications dedicated to it are in Russian.

According to this law (I will call it Butusov's Law), the product of semi-major axes of planets that are symmetrical around Jupiter (*i.e.*, Ceres and Saturn, Mars, Uranus, *etc.*) is equal to the square of the semi-major axis of Jupiter. It means that the distribution of planetary distances in the Solar System may be described by the following formula:

$$R_6^2 = R_{6+j} \times R_{6-j}, \tag{1}$$

where R_6 is Jupiter's orbital semi-major axis in AU; $j \in \{0, 1, 2, 3, 4, 5\}$; $6 \pm j$ is the number of a planet counting from the Sun*; $R_{6\pm j}$ is the semi-major axis of the $(6 \pm j)$ th planet (counting from the Sun), in AU.

For example, if $j = 2$, then

$$R_6^2 = R_4 \times R_8 \Rightarrow R_{Jupiter}^2 = R_{Mars} \times R_{Uranus} \,.$$

Butusov's Law simply says that there is a Jovian symmetry of planetary distances in the Solar System — and this was highlighted by Butusov himself. This 'Jovicentric' symmetry is the main discovery made by Butusov. His results are summed up in the Table I, which clearly shows that Butusov's Law is not absolutely precise. One may ask if the correspondence between real and calculated data is good enough to accept Butusov's formula as a 'law' of planetary spacing within the Solar System. It is difficult to give a precise answer because there is no definition of 'acceptable difference'. However, it can easily

* j is the number of a planet counting from Jupiter.

TABLE I

Butusov's Law

i	*Planet*	R_i, *AU*	j	R_{6-j}, *AU*	R_{6+j}, *AU*	$R_{6+j} \times R_{6-j}$	*d*, %
1	Mercury	0·39	5	0·39	67·67	26·39	−2·40
2	Venus	0·72	4	0·72	39·48	28·43	5·12
3	Earth	1·00	3	1·00	30·10	30·10	11·32
4	Mars	1·52	2	1·52	19·23	29·23	8·10
5	Ceres	2·77	1	2·77	9·58	26·54	−1·86
6	Jupiter	5·20	0	5·20	5·20	27·04	0·00
7	Saturn	9·58	1	2·77	9·58	26·54	−1·86
8	Uranus	19·23	2	1·52	19·23	29·23	8·10
9	Neptune	30·10	3	1·00	30·10	30·10	11·32
10	Pluto	39·48	4	0·72	39·48	28·43	5·12
11	Eris	67·67	5	0·39	67·67	26·39	−2·40

$$d = \frac{R_{6+j} \times R_{6-j} - R_6^2}{R_6^2} \times 100\%.$$

TABLE II

Comparison of deviations for Butusov's Law and the Titius–Bode Law

i	*Planet*	R_i, *AU*	$TB_i^{\dagger}$, *AU*	*D*, %	*d*, %
1	Mercury	0·39	0·4	−2·50	−2·40
2	Venus	0·72	0·7	2·85	5·12
3	Earth	1·00	1·00	0·00	11·32
4	Mars	1·52	1·6	−5·00	8·10
5	Ceres	2·77	2·8	−1·07	−1·86
6	Jupiter	5·20	5·20	0·00	0·00
7	Saturn	9·58	10·00	−4·2	−1·86
8	Uranus	19·23	19·6	−1·88	8·10
9	Neptune	30·10	39·8	−22·42	11·32
10	Pluto	39·48	77·2	−48·86	5·12
11	Eris	67·67	154·0	−56·06	−2·40

$$D = \frac{R_i - TB_i}{TB_i} \times 100\%.$$

† TB_i is the semi-major axis of the orbit of the *i*-th planet calculated on the basis of the Titius–Bode Law. *D* and *d* are the percentage differences between the calculations and measurements for Titius–Bode's and Butusov's Laws, respectively.

be seen that while for planets from Mercury to Uranus differences between real distances and distances calculated on the basis of Butusov's Law are as good as (or just a little worse than) the differences for the original Titius–Bode Law (the notable exception is Earth), Butusov's results for Neptune, Pluto, and Eris are substantially better (see Table II).

The difference between real and calculated distances in the case of Butusov's Law increases for $j = 1$–3 and decreases for $j = 4$–5 (see Table I). One might have expected that this difference would steadily increase (as with the increase of distance from Jupiter the gravitational influence of Jupiter should decrease and the correspondence between real and calculated distances should become worse), but that is not true.

Analysis of Butusov's Law leads to some interesting conclusions: (*a*) this law is also valid for Eris, which was discovered long after Butusov published his paper; that is, the formula (1) meets the main requirement for a law — it included not only the results known at the moment of its discovery, but was also able to predict new results (like the Titius–Bode Law 'predicted' Uranus and the asteroid belt). Of course, one should not think that the position of Eris within the Solar System is affected by the position of Mercury (or *vice versa*), rather one should admit that positions of both these planets are determined by the same effect (probably Jupiter's gravitational influence); (*b*) this law introduces natural limits for the number of planets in the Solar System: there may be no more than 11 planetary bodies as there is no additional planet between the Sun and Mercury that could allow existence of an additional planet beyond the orbit of Eris*; (*c*) Butusov's Law is not actually a law of planetary distances — in its

*But if a body corresponding to the definition of planet (or dwarf planet) is found beyond the orbit of Eris, Butusov's Law will require introducing fictitious 'planets' within the orbit of Mercury. This situation would be similar to the introduction of empty planetary orbits in exponential forms of the Titius–Bode Law and would strongly diminish the value of Butusov's Law.

present form it cannot be used to calculate the semi-major axis of a planet as a function of its number counting from the Sun. Therefore this law is actually a law of harmony in the structure of the Solar System; and (*d*) the most popular mathematical reformulation of the Titius–Bode Law is its representation in an exponential form:

$$R_i = Ak^i\,, \tag{2}$$

where A and k are constants.

It can easily be seen that the formula (2) has a close connection with the formula (1). Indeed, according to (2)

$$Ak^{6-j}\,Ak^{6+j} = A^2k^{12} = (Ak^6)^2\,. \tag{3}$$

It means that Butusov's Law includes the Titius–Bode Law as a particular case, but Butusov's Law is more general as the distribution of planetary distances in the Solar System is not exponential. In addition, the exponential form of the Titius–Bode Law does not give special status to Jupiter (contrarily to Butusov's Law) as an analogue of (3) could be written down for any l between 2 and 11:

$$Ak^{l-j}\,Ak^{l+j} = A^2k^{2l} = (Ak^l)^2\,.$$

The physical nature of Butusov's Law remains unclear (most probably its existence is due to orbital near-resonances produced by Jupiter thanks to its gravitational domination in the Solar System) but it shows an interesting regularity in the structure of the Solar System and should therefore be included in scholarly research. Kirill Butusov tried himself in his later works to find a physical explanation, but his hypotheses used non-traditional physics (mysterious "waves of pulsations" — a kind of acoustical effect created by planets in plasma that surrounds them) and were not accepted by the astronomical community in Russia[10].

Ragnarsson's Law

In 1995 Sven-Ingmar Ragnarsson, an astronomer from Sweden, published an article[9] with a new law of planetary distances. He also pointed out that there is Jovian symmetry in the Solar System and tried to reflect this symmetry in his formula introducing a parameter $m = i - 6$, which he labelled the Jovicentric planet number:

$$R_i = R_{Jupiter}\left[\left(\frac{5}{2}\right)^{\frac{2}{3}}|m|\right]^{\mathrm{sgn}(m)} \tag{4},$$

where $R_{Jupiter}$ is the semi-major axis of Jupiter's orbit in AU, and sgn(x) is a mathematical function:

$$\mathrm{sgn}(x) = \begin{cases} -1, & x < 0; \\ 0, & x = 0; \\ 1, & x > 0. \end{cases}$$

As $m < 0$ for planets within the orbit of Jupiter and $m > 0$ for planets beyond the orbit of Jupiter, then the formula (5) can be represented as

$$R_i = \begin{cases} \dfrac{R_{Jupiter}}{(5/2)^{2/3}|m|} & m < 0; \\ R_{Jupiter}\,(5/2)^{2/3}\,m & m > 0, \end{cases} \tag{5}$$

which clearly demonstrates that Ragnarsson's Law not only shows the existence of symmetry of planetary distances in the Solar System around Jupiter but also formally complies with Butusov's Law.

Interestingly enough, Ragnarsson states that "This symmetry [around Jupiter] seems not to have been widely observed in literature"; Butusov's paper is not listed in references to his paper. Unfortunately, all Butusov's publications were made in Russian and in collections of scientific papers with very limited circulation. The international scholastic community obviously did not have access to his works, and Ragnarsson made his discovery of Jovicentric symmetry independently.

Ragnarsson's law has several major advantages: (*a*) the factor $(5/2)^{2/3}$ is not a simple combination of integer numbers, but has a clear physical meaning. Ragnarsson decided to express distances in the Solar System in units of the Jovian orbital semi-major axis and observed that planetary distances were close to simple harmonic multiples of 1·842 (and their reciprocals). Mathematically $1{\cdot}842 = (5/2)^{2/3}$, which "is the ratio of distance for which the period of revolution is exactly 5/2 times that of Jupiter, as calculated from Kepler's 3rd Law, *i.e.*, one of the most important gravitational resonances"; (*b*) contrary to Butusov's Law, Ragnarsson's Law is indeed a law of planetary distances as it allows one to calculate the semi-major axis of a planet (formula (5) above); (*c*) also contrary to Butusov's Law, Ragnarsson's Law puts no limits on the number of planets in the Solar System; (*d*) Ragnarsson's idea of Jovicentric symmetry is expressed by a clear and unambiguous formula (4), showing that in fact *two different but symmetric* formulae (5) should be used for inner (with regard to Jupiter) and outer planets.

These advantages represent very important results obtained by Ragnarsson. His model is also interesting from the mathematical point of view as he used generalized functions to represent the Jovicentric symmetry. However, his formula has two main flaws: (*a*) Analysis of the formula (5) shows that, according to Ragnarsson, orbital semi-major axes of outer planets form an arithmetic progression with common difference $1{\cdot}842R_{Jupiter}$. While this is correct for Saturn, Uranus, Neptune, and Pluto (whose distances from the Sun do form an arithmetic progression), this is not true for Eris — and Ragnarsson's formula gives an incorrect value for the semi-major axis of Eris. In addition, as the formula (4) is symmetrical, incorrect results for Eris mean that results for Mercury will also be incorrect (see Table III)*. Therefore, despite the fact that Ragnarsson's Law ensures good correspondence with observed data for most planets, it is not able to include the 'limit' planets, Mercury and Eris (the same problem is typical for most versions of the Titius–Bode Law (see ref. 7)). It is an important problem that substantially diminishes the value of this law. (*b*) From the mathematical point of view this law (or, more precisely, the formula (4)) is also incorrect. Indeed, for Jupiter (where $m = 0$) the formula (5) can be rewritten as

$$R_{Jupiter} = R_{Jupiter}\,(0)^0.$$

*As the formula (4) is symmetrical, results will be either correct or incorrect for both planets with the same value of the Jovicentric planet number *m*. As Eris and Mercury share the same value of the Jovicentric planet number, distances calculated on the basis of Ragnarsson's Law are incorrect for both of them.

TABLE III

Ragnarsson's Law

i	*Planet*	R_i *(observed), AU*	m	R_m *(according to Ragnarsson's Law), AU*	D_R, %
1	Mercury	0·39	−5	0·56	−30·36
2	Venus	0·72	−4	0·71	1·41
3	Earth	1·00	−3	0·94	6·38
4	Mars	1·52	−2	1·41	7·80
5	Ceres	2·77	−1	2·82	−1·77
6	Jupiter	5·20	0	5·20†	0
7	Saturn	9·58	1	9·58	0
8	Uranus	19·23	2	19·16	0·37
9	Neptune	30·10	3	28·74	4·73
10	Pluto	39·48	4	38·31	3·05
11	Eris	67·67	5	47·89	41·30

$$D_R = \frac{R_i - R_m}{R_m} \times 100\%;$$

†Actually the semi-major axis of Jupiter cannot be calculated on the basis of Ragnarsson's Law (formula (5)) — see text.

The expression 0^0 has no mathematical meaning — while the proponent of this law probably supposed that it should be equal to 1 (as $k^0 = 1$ for all k *except* $k = 0$). This unfortunate mistake also diminishes the value of this law, particularly since owing to this mistake this law has no correct mathematical formulation for the central body of the postulated symmetry, Jupiter.

The physical explanation of this law (provided by Ragnarsson himself) is that Jupiter was the first planet to condense and accrete, so that it could influence the formation of other planets. Unfortunately, this explanation is not supported by calculations and despite being plausible still requires additional theoretical support.

Conclusions

An obvious question is which law (Titius–Bode, Butusov, or Ragnarsson) is the 'best' one. Unfortunately at the present stage, when physical reasons underlying these laws are unknown and no single formula provides an absolutely precise correspondence between observed and calculated distances, this question will remain unanswered.

In addition, the value of laws proposed by Butusov and Ragnarsson for scientific research is also unclear. While the Titius–Bode Law initiated the search for the missing planet between Earth and Mars and played a rôle in the discovery of Neptune, neither Ragnarsson's Law nor Butusov's Law have the same predictive potential (or nearly the same recognition that the Titius–Bode Law enjoyed in Olbers' and Le Verrier's era) — Ragnarsson's predictions beyond Pluto are incorrect and Butusov's Law puts a limit on the number of planets: no planets beyond Eris are possible. These laws just describe (with more or less success) the situation with existing planets.

However, in my opinion, the value of these laws resides in a clear indication of Jovian symmetry in the Solar System and of the rôle of Jupiter in the spacing of planetary orbits.

This short analysis of laws proposed by Butusov and Ragnarsson shows that even in such a peripheral field of astronomy as analysis of the structure of the Solar System — a field connected mostly with the Titius–Bode Law and having a somewhat ambiguous reputation — interesting discoveries are still possible. And the main discovery made independently by Butusov and Ragnarsson is the symmetry of the Solar System around Jupiter — a symmetry which may (but obviously not necessarily *will*) be important for understanding the origin of planets in our system. On the other hand, while this supposed symmetry is not provided with a solid theoretical foundation on the basis of modern methods of celestial mechanics, it must be considered as a coincidence.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 221: HD 109803, HD 109954, HD 110195, AND HD 117078

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The four objects discussed here are all to be found within about 10° of the North Galactic Pole. All are multiple systems in which two (only) of the components are late-type stars that give measurable signatures in radial-velocity traces; the undetected components are most likely to be main-sequence stars of considerably lower luminosities than the observed components.

HD 109803 appears at first sight to be a very unequal double-lined binary which has a circular orbit with a period of 6·74 days and a very low inclination that causes the velocity amplitudes to be so small as to create observational difficulty by the permanent mutual blending of the spectra. The γ-velocity of the pair varies in a period of about 8 years in an orbit of small amplitude and rather high eccentricity.

HD 109954 exhibits two components differing by about three-quarters of a magnitude. The brighter one is a single-lined binary with a 65-day period, which the other does not share. Instead, the γ-velocity of the 65-day orbit varies in anti-phase with the velocity of the fainter star in an orbit 100 times as long. The two orbits are quite eccentric and are of remarkably similar form and even amplitude. On occasion the radial-velocity signatures of the two components are completely separated, giving accurate information on their profiles and thereby facilitating their measurement at the (more usual) blended phases. The parallax shows the stars to be above the main sequence, but their evolutionary status is enigmatic.

An orbit with a period close to 18 days was published for the nearly-equal binary system HD 110195 by the writer some 25 years ago, but there was one (temporally isolated) observation that was so discordant that it could be understood only in terms of higher multiplicity. The matter was not pursued at the time, but the publication comparatively recently of a lot of additional radial velocities of the system has re-opened the issue. Observations in the last three seasons have established the current phase in a very eccentric orbit in which the γ-velocity of the 18-day pair varies opposite an unseen tertiary star in a quite eccentric orbit that has a period of about 22 years.

HD 117078 is a double-lined system with a Δm a little more than one magnitude. The velocities of the observed components vary in totally independent ways — one in an almost-circular 5·84-day orbit and the other in a very eccentric one with a period of 204 days. Their γ-velocities vary slowly in anti-phase with one another, so the two observed stars must be the primaries of two single-lined binaries that themselves constitute a binary system, whose period promises to be some 40 years and whose rather eccentric orbit, though far from having been seen round a complete cycle, is determined in a preliminary fashion by a rather makeshift expedient. The outer system has recently been resolved by optical interferometry.

Introduction

This is another paper that (like nos. 203, 206, 212, 215, and 218 in this series, to recall only those published since no. 200) presents orbits for stars whose spectroscopic-binary nature was discovered in the course of the photometric and radial-velocity survey undertaken in 1967–97 by Yoss & Griffin[1] of all the late-type stars near the North Galactic Pole (NGP) ($b > 75°$). Readers are invited to view as acceptable the use here of the expression 'spectroscopic binary' in a relaxed manner, to embrace also systems of higher multiplicity.

Particularly in view of the scarcity of astrophysical data concerning most NGP stars, exacerbated by the fact that the survey paper which contains most of such information as *is* available is not retrieved in their *Simbad* bibliographies, it seems useful to present the survey results here in Table I.

TABLE I

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

Star	*V*	*(B – V)*	*Type*	M_V	*z*
	m	*m*		*m*	*pc*
HD 109803	9·38	0·57	G0 V	+4·4	98
HD 109954	8·47	0·86	G8 IV	+2·4	163
HD 110195	10·13	0·74	G9 V	+5·7	77
HD 117078	9·59	0·57	G0 V	+4·4	107

The spectral types listed are not genuine classifications from spectra but are inferences from *DDO*-style photometry[2]; in the cases of double-lined binary systems, such as are treated here, they will inevitably be luminosity-weighted averages of the types of the components. Moreover, the absolute magnitudes determined in the survey pre-suppose that the objects being dealt with are single stars. Not only are the luminosities of the objects in Table I increased by the contributions of the secondary stars, but the luminosity estimates themselves do not refer strictly to the primaries but to a weighted average of the components, *i.e.*, they are too low. The situation is exactly analogous to that pertaining to Strömgren photometry, discussed at some length in Paper 185, where[3] it was shown that the correction for duplicity to the photometrically estimated absolute magnitude remains near $-0^{m}\cdot7$ for Δm values up to a whole magnitude, which covers all the stars discussed here apart from HD 109803. All the tabulated distances accordingly need revision by a factor of about 1·4, or somewhat less in the case of HD 109803. For HD 109954 (much the brightest of the stars in Table I and the only one with an *Hipparcos* parallax) that revision would bring the distance up to about 230 pc, corresponding to a parallax of about 4·3 arc-ms, just one standard deviation away from the *Hipparcos* value. This small discussion is not to be read as any particular indictment of photometrically estimated spectral types or distances, since exactly the same arguments would apply equally to direct classifications and to absolute magnitudes based upon them.

Observationally, four different cases of triple stellar systems may be distinguished. In 'Case 1' only one spectrum is observable (it is necessarily that of one of the stars in the short-period 'inner' orbit, otherwise the system just looks like a single-lined binary); the single spectrum exhibits velocity variations with two different periods. Then there are two cases in which two spectra are visible. In 'Case 2' they are those of the two stars in the inner orbit, whose γ-velocity changes in a longer period, like those of HD 109803 and HD 110195 treated below; in 'Case 3' one star in the inner orbit and the singleton in the outer orbit are seen, as happens with HD 109954. In 'Case 4' all three stars are seen in the spectrum. Not only are quite different orbit-solving programs needed for the four cases, but each also requires a special section to deal with objects which have circular or near-circular inner orbits (operating by the Sterne[4] principle instead of the more general Lehmann-Filhés[5] method, which breaks down near zero eccentricity). The writer was fortunate in having the assistance some years ago of Mr. A. P. Cornell, who was largely responsible for the technical side of developing the programs and completed the 'Case 3' one, which was duly utilized forthwith in a couple of demonstration papers[6,7]. Serviceable versions of the others have been completed as need for them has respectively arisen.

HD 109803

This is the most northerly of the four stars and is in the constellation Canes Venatici, about 5° south-preceding Cor Caroli (α CVn). Malmquist[8] first obtained photographic photometry for it and gave it an identification of 34° 303, and much later[9] classified it by low-dispersion objective-prism spectroscopy as type G5, thereby agreeing with the *HD* type. Upgren[10] classified it as G0, also from an objective-prism plate. Magnitudes of $V = 9^{m}\cdot387$, $(B-V) = 0^{m}\cdot570$, $(U-B) = 0^{m}\cdot076$ have been published by Oja[11], with a claim to high precision; they are seen to agree well with the survey[1] magnitudes (as far as they go) given in Table I above.

The first radial-velocity measurement of HD 109803 was made with the original Cambridge spectrometer[12], on which the now universally adopted method of cross-correlation was first developed, in 1982. The radial-velocity trace showed a dip that was quite shallow owing to the rather early type of the star, so the object was more easily observed with the OHP *Coravel*[13] when opportunity arose to use that instrument. Single observations were also made with the ESO and DAO[14] spectrometers. The variability of the star's velocity soon came to light, but the difficulty of obtaining good velocities for both components is well illustrated by the fact that it was not until the twentieth observation was made, on 1992 April 26, that the trace was recognized as double-lined. Assiduous efforts in successive OHP observing runs established the 6·74-day double-lined orbit, but at the same time demonstrated that there were changes in the γ-velocity of that orbit. It took much longer to document *them*, since the 'outer' orbit has a period of over 8 years. No sign has been seen of the object that must circulate in that orbit opposite the 6-day binary.

In recent years the system has been seen round a complete cycle of the outer orbit with the Cambridge *Coravel*. The observations have generally been scheduled at phases near the nodes of the inner orbit, when it may be hoped that the velocities of both components can be determined. An unusually well-integrated trace taken almost exactly at a node is reproduced here as Fig. 1 and shows absolutely the most favourable resolution that can ever be obtained of the blend. Many of the traces obtained away from the node, or with less generous *S*/*N* ratio, have not been reduced with all the dip parameters 'free' but have had a primary:secondary ratio of 5:1 (representing the conclusion from the best independently reduced traces) imposed, and the projected rotational velocity of the secondary (at least) has been fixed at zero.

There are 49 Cambridge traces that have all been reduced as double-lined. There are also 50 traces from the OHP *Coravel*, seven from the original spectrometer at Cambridge, and one each from ESO and the DAO. They were taken at random times and therefore refer to all phases of the inner orbit, so many of them are much more closely blended even than the trace in Fig. 1. Of them all, only seven, all from OHP, have been reduced as double-lined. All the measurements are listed in Table II. (In that and subsequent analogous tables, the civil dates given are the actual times of observation, but the Julian dates have been corrected for light-time to the barycentre of the system. It might also be mentioned that apparent slight inaccuracies in the summation and differencing of velocities arise merely from rounding errors.)

The 56 double-lined results form the principal basis of the orbital solution, which was run with a program that optimizes the elements of both the outer and inner orbits simultaneously. There are ten elements to be solved for in this case, where the inner orbit is held circular. The OHP observations have as usual been adjusted by +0·8 km s^{-1} and the Cambridge ones by –0·4 km s^{-1} from

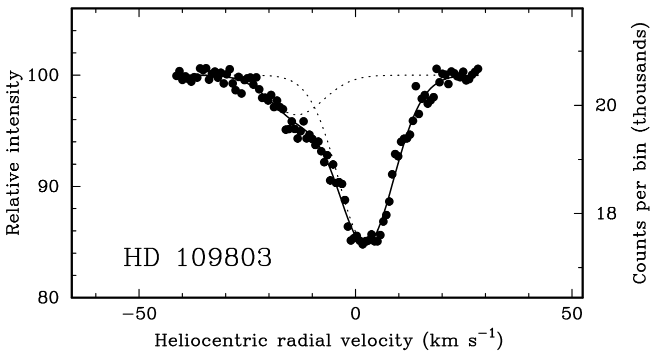

FIG. 1

Radial-velocity trace of HD 109803, obtained with the Cambridge *Coravel* on 2003 April 21. The trace was taken right at a node of the (circular) inner orbit (phase ·008) and shows the best separation that can ever be seen of the blended dips.

the 'as reduced' values. The two sources have been given equal weight in the solution, but the measurements of the weak secondary have needed to be down-weighted by a factor of 25 in comparison with the primary. Before a solution is presented on that basis of little more than half of the total set of observations, it is worth considering what if anything can be done with all the others, which were reduced as single-lined. Many among the OHP traces would in principle support double-lined reductions, but there is now no way in which the writer can obtain fresh reductions of OHP data, which are stored in a file to which he has no access.

Owing to the large disparity in the signatures of the two components, the velocities given by single-lined reductions are not very far from the true velocities of the primary star, but are in all cases 'dragged' towards the γ-velocity by blending with the secondary. Such a situation is already familiar, and previous efforts at salvaging blended traces, *e.g.*, refs. 6, 15, and 16, have used the 'dragging function' approach. In the present instance that is easily done, because quite a satisfactory orbit can be derived for the 6-day orbit from the available double-lined reductions; then the single-lined ones can be entered in the solution with zero weight and their residuals read off directly and plotted against the velocity separation of the components. It is clear intuitively that at very small velocity separations the blend appears to be at the weighted mean velocity of the components, so in the case of HD 109803, with its adopted 5:1 ratio of components, the initial gradient of the dragging function ought to be 1 in 6; we know from previous experience that it very soon falls below the initial value, reaches a maximum about when the velocity separation equals the half-widths of the dips, and then declines again, to zero when the dips are practically separate. In actual fact, in the present case it was found to reach 0·5 km s^{-1} at a velocity separation of about 3·5 km s^{-1} (at the expected initial slope it would be at 3), it reaches 1 km s^{-1} at a separation of 8 km s^{-1}, and is just about at a maximum of 1·3 km s^{-1} at the nearly-16-km s^{-1} maximum separation of the components.

Since the argument of the dragging function is velocity separation — tantamount to orbital phase — its application makes further use of the orbit already obtained. The result of the application is of course a lot of extra,

TABLE II

Radial-velocity observations of HD 109803

Except as noted, the sources of the observations are as follows:
1987–1998 — OHP Coravel (weight 1); 2000–2011 — Cambridge Coravel (weight 1)

Where there is no measurement of the secondary, the measurement was of the blend and has been 'corrected' to the primary and weighted ¼ (see text)

Date	*MJD*	*Velocity* A km s^{-1}	*Velocity* B km s^{-1}	*Outer orbit* Phase	*Outer orbit* Velocity km s^{-1}	*Inner orbit* Phase	*Inner orbit* Vel (A) km s^{-1}	*Inner orbit* Vel (B) km s^{-1}	(O–C) A km s^{-1}	(O–C) B km s^{-1}
1982 Mar. 8·08*	45036·08	−1·8	—	0·566	−6·8	0·918	+5·5	—	−0·5	—
1987 Mar. 2·94	46856·94	+0·2	—	1·166	−4·1	270·945	+6·0	—	−1·7	—
1988 Mar. 14·12	47234·12	−0·7	—	1·290	−5·2	326·879	+4·6	—	−0·1	—
1989 Mar. 28·97	47613·97	−4·9	—	1·415	−6·0	383·209	+1·6	—	−0·5	—
Apr. 29·99	645·99	−0·1	—	·425	−6·0	387·958	+6·1	—	−0·2	—
May 30·94*	676·94	−10·1	—	·436	−6·1	392·547	−6·1	—	+2·1	—
1990 Jan. 27·12	47918·12	−8·3	—	1·515	−6·5	428·313	−2·5	—	+0·7	—
Feb. 12·32†	934·32	−8·1	—	·520	−6·6	430·716	−1·4	—	−0·2	—
Mar. 26·99*	976·99	−0·9	—	·534	−6·6	437·044	+6·1	—	−0·4	—
Apr. 30·90*	48011·90	−5·6	—	·546	−6·7	442·221	+1·2	—	−0·1	—
1991 Jan. 29·12	48285·12	−7·1	—	1·636	−7·2	482·738	−0·5	—	+0·5	—
May 9·94*	385·94	−10·5	—	·669	−7·3	497·690	−2·4	—	−0·8	—
24·96*	400·96	−4·5	—	·674	−7·3	499·917	+5·5	—	−2·7	—
June 12·97*	419·97	−8·9	—	·680	−7·4	502·736	−0·6	—	−1·0	—
Dec. 17·15	607·15	−13·5	—	·742	−7·7	530·494	−6·4	—	+0·5	—
1992 Jan. 14·12	48635·12	−11·6	—	1·751	−7·7	534·642	−4·0	—	+0·1	—
19·23	640·23	−13·2	—	·753	−7·7	535·400	−5·1	—	−0·3	—
Feb. 27·47‡	679·47	−6·5	—	·766	−7·8	541·219	+1·2	—	+0·1	—
Apr. 21·98	733·98	−9·5	—	·783	−7·9	549·303	−2·1	—	+0·4	—
26·96	738·96	−1·9	—	·785	−7·9	550·041	+6·1	—	−0·2	—
29·04	741·04	−11·2	—	·786	−7·9	·350	−3·7	—	+0·4	—
29·95	741·95	−14·6	—	·786	−7·9	·485	−6·3	—	−0·4	—
May 1·03	743·03	−11·3	—	·786	−7·9	·645	−3·9	—	+0·5	—
June 24·93	797·93	−6·9	—	·805	−7·9	558·787	+1·4	—	−0·4	—
26·90	799·90	−2·5	—	·805	−7·9	559·079	+5·6	—	−0·2	—
Aug. 12·86	846·86	−2·5	—	·821	−8·0	566·043	+6·1	—	−0·6	—
15·83	849·83	−14·4	+1·4	·822	−8·0	·483	−6·3	+9·0	−0·1	+0·4
16·84	850·84	−12·2	—	·822	−8·0	·633	−4·3	—	+0·1	—
Dec. 18·21	974·21	−2·5	—	·863	−8·1	584·928	+5·7	—	−0·1	—
19·19	975·19	−2·7	—	·863	−8·1	585·074	+5·7	—	−0·3	—
20·17	976·17	−7·0	—	·863	−8·1	·219	+1·2	—	−0·1	—
21·17	977·17	−12·4	—	·864	−8·1	·367	−4·3	—	0·0	—
22·17	978·17	−14·2	+2·4	·864	−8·1	·516	−6·3	+9·0	+0·2	+1·5
1993 Feb. 12·15	49030·15	−7·2	—	1·881	−8·1	593·224	+1·0	—	−0·1	—
13·04	031·04	−12·4	—	·881	−8·1	·356	−3·9	—	−0·4	—
14·12	032·12	−14·9	+0·4	·882	−8·1	·516	−6·3	+9·0	−0·5	−0·5
15·10	033·10	−11·8	—	·882	−8·1	·662	−3·4	—	−0·3	—
Mar. 18·07	064·07	−8·7	—	·892	−8·1	598·254	−0·2	—	−0·5	—
23·13	069·13	−1·8	—	·894	−8·1	599·005	+6·4	—	−0·1	—
July 8·86	176·86	−1·3	—	·929	−7·7	614·981	+6·3	—	+0·1	—
11·86	179·86	−13·2	+2·2	·930	−7·7	615·426	−5·7	+8·1	+0·2	+1·8
Dec. 25·23	346·23	−1·1	—	·985	−5·6	640·098	+5·2	—	−0·7	—
27·19	348·19	−10·8	—	·986	−5·6	·388	−4·9	—	−0·4	—
28·16	349·16	−11·5	+4·0	·986	−5·5	·532	−6·2	+8·9	+0·3	+0·6

TABLE II (*continued*)

Date	*MJD*	*Velocity* *A* *km s^−1*	*Velocity* *B* *km s^−1*	*Outer orbit* *Phase*	*Outer orbit* *Velocity* *km s^−1*	*Inner orbit* *Phase*	*Inner orbit* *Vel (A)* *km s^−1*	*Inner orbit* *Vel (B)* *km s^−1*	*(O−C)* *A* *km s^−1*	*(O−C)* *B* *km s^−1*
1994 Feb. 18·05	49401·05	−3·6	—	2·003	−4·5	648·227	+0·9	—	0·0	—
21·09	404·09	−7·5	—	·004	−4·5	·678	−2·8	—	−0·3	—
May 3·02	475·02	−1·9	—	·027	−3·4	659·197	+2·1	—	−0·5	—
Dec. 12·20	698·20	−5·6	—	·101	−3·3	692·293	−1·7	—	−0·5	—
29·22	715·22	−1·2	—	·106	−3·4	694·817	+2·6	—	−0·4	—
1995 Jan. 2·24	49719·24	−9·5	—	2·108	−3·4	695·413	−5·4	—	−0·6	—
3·17	720·17	−10·1	—	·108	−3·4	·551	−6·0	—	−0·6	—
4·21	721·21	−5·9	—	·108	−3·4	·706	−1·8	—	−0·7	—
5·17	722·17	−0·1	—	·109	−3·4	·848	+3·7	—	−0·3	—
May 31·02	868·02	−10·2	+7·9	·157	−4·0	717·477	−6·3	+9·0	+0·1	+2·9
June 6·92	874·92	−10·2	+7·7	·159	−4·0	718·500	−6·4	+9·1	+0·2	+2·6
1996 Mar. 29·06	50171·06	−11·1	—	2·256	−4·9	762·416	−5·5	—	−0·7	—
Apr. 25·00	198·00	−10·7	—	·265	−5·0	766·411	−5·4	—	−0·3	—
1997 Mar. 29·08§	50536·07	−11·9	+0·7	2·377	−5·8	816·547	−6·1	+8·7	−0·1	−2·2
Apr. 8·02§	546·01	+1·1	−11·2	·380	−5·8	818·021	+6·3	−9·0	+0·6	+3·6
14·95§	552·94	+0·4	−14·0	·382	−5·8	819·049	+6·1	−8·7	+0·1	+0·5
17·95§	555·94	−11·5	+3·6	·383	−5·8	·494	−6·4	+9·1	+0·7	+0·3
1998 May 1·97	50934·97	−8·6	—	2·508	−6·5	875·701	−1·9	—	−0·2	—
July 14·87	51008·87	−9·9	—	·532	−6·6	886·660	−3·4	—	+0·1	—
2000 Feb. 11·14	51585·14	−3·1	−13·5	2·722	−7·6	972·119	+4·7	−6·7	−0·2	+0·7
14·19	588·19	−13·4	−3·2	·723	−7·6	·572	−5·7	+8·2	−0·1	−3·8
Apr. 10·92	644·92	−1·3	−14·6	·742	−7·7	980·984	+6·3	−9·0	0·0	+2·1
23·98	657·98	−2·0	−12·9	·746	−7·7	982·921	+5·6	−8·0	+0·1	+2·8
30·01	664·01	−5·1	−10·0	·748	−7·7	983·815	+2·5	−3·6	+0·1	+1·3
2001 Mar. 11·07	51979·07	−14·2	+2·4	2·851	−8·1	1030·538	−6·2	+8·8	+0·1	+1·7
May 12·94	52041·94	−3·2	−14·2	·872	−8·1	1039·861	+4·1	−5·9	+0·8	−0·2
2002 Apr. 6·04	52370·04	−12·1	+4·1	2·980	−5·9	1088·518	−6·3	+9·0	+0·1	+1·0
May 22·94	416·94	−11·3	+0·9	·996	−5·0	1095·473	−6·3	+9·0	−0·1	−3·1
2003 Jan. 11·21	52650·21	+2·9	−11·5	3·072	−3·1	1130·066	+5·8	−8·3	+0·2	−0·1
28·16	667·16	−8·7	+6·7	·078	−3·1	1132·579	−5·6	+8·0	0·0	+1·8
Feb. 21·09	691·09	+1·2	−11·9	·086	−3·2	1136·128	+4·4	−6·3	0·0	−2·4
Mar. 16·10	714·10	−9·5	+5·2	·093	−3·3	1139·540	−6·2	+8·8	−0·1	−0·3
20·05	718·05	+1·8	−8·8	·095	−3·3	1140·126	+4·5	−6·4	+0·6	+0·9
Apr. 7·98	736·98	+2·7	−12·1	·101	−3·3	1142·933	+5·8	−8·3	+0·2	−0·5
8·95	737·95	+3·2	−11·6	·101	−3·3	1143·077	+5·6	−8·0	+0·9	−0·2
15·99	744·99	+0·9	−13·4	·104	−3·4	1144·121	+4·6	−6·6	−0·3	−3·4
17·95	746·95	−9·2	+3·4	·104	−3·4	·412	−5·4	+7·7	−0·4	−0·9
18·94	747·94	−9·6	+5·9	·105	−3·4	·558	−5·9	+8·5	−0·3	+0·8
21·97	750·97	+3·2	−12·6	·106	−3·4	1145·008	+6·4	−9·1	+0·2	−0·1
29·05	758·05	+3·1	−12·5	·108	−3·4	1146·058	+5·9	−8·5	+0·6	−0·6
2004 Jan. 17·23	53021·23	+0·8	−11·8	3·194	−4·4	1185·086	+5·5	−7·8	−0·3	+0·4
Apr. 7·07	102·07	+1·3	−12·2	·221	−4·6	1197·074	+5·7	−8·1	+0·2	+0·5
June 5·94	161·94	−0·1	−13·8	·241	−4·8	1205·953	+6·1	−8·7	−1·4	−0·3
2005 Jan. 22·22	53392·22	−0·3	−10·3	3·317	−5·4	1240·102	+5·1	−7·3	0·0	+2·3
Apr. 19·05	479·05	+0·8	−12·3	·345	−5·6	1252·979	+6·3	−9·0	+0·1	+2·3
June 8·93	529·93	−11·5	+5·0	·362	−5·7	1260·524	−6·3	+9·0	+0·5	+1·7
2006 Feb. 16·13	53782·12	−0·8	−15·4	3·445	−6·2	1297·924	+5·7	−8·1	−0·3	−1·2
Mar. 2·12	796·11	−0·1	−13·3	·450	−6·2	1299·999	+6·4	−9·1	−0·3	+2·0

TABLE II (*concluded*)

Date	*MJD*	*Velocity* *A* *km s⁻¹*	*Velocity* *B* *km s⁻¹*	*Outer orbit* *Phase*	*Outer orbit* *Velocity* *km s⁻¹*	*Inner orbit* *Phase*	*Inner orbit* *Vel (A)* *km s⁻¹*	*Inner orbit* *Vel (B)* *km s⁻¹*	*(O–C)* *A* *km s⁻¹*	*(O–C)* *B* *km s⁻¹*
2007 Feb. 6·16	54137·16	−11·9	+3·3	3·562	−6·8	1350·574	−5·7	+8·1	+0·6	+2·0
May 7·96	227·96	−1·1	−16·8	·592	−6·9	1364·040	+6·2	−8·8	−0·3	−1·1
2008 Feb. 25·15	54521·15	−13·9	+2·9	3·688	−7·4	1407·519	−6·3	+9·0	−0·2	+1·3
July 8·93	655·93	−14·2	−1·4	·732	−7·6	1427·506	−6·4	+9·1	−0·2	−2·9
2009 Jan. 6·28	54837·28	−13·1	+1·6	3·792	−7·9	1454·400	−5·1	+7·3	−0·1	+2·1
Mar. 29·07	919·07	−14·4	+0·7	·819	−8·0	1466·529	−6·3	+8·9	−0·1	−0·2
May 24·97	975·97	−2·2	−18·4	·838	−8·1	1474·967	+6·2	−8·9	−0·4	−1·4
2010 Mar. 5·04	55260·04	−2·5	−14·1	3·931	−7·7	1517·094	+5·3	−7·5	−0·1	+1·1
Apr. 17·01	303·01	−13·6	+1·1	·945	−7·4	1523·466	−6·2	+8·9	0·0	−0·4
20·97	306·97	−0·9	−13·7	·947	−7·3	1524·054	+6·0	−8·6	+0·4	+2·2
May 17·98	333·98	−1·5	−16·8	·956	−7·1	1528·059	+5·9	−8·5	−0·4	−1·3
July 6·95	383·95	−12·4	+4·2	·972	−6·3	1535·469	−6·2	+8·9	+0·2	+1·6
9·93	386·93	−1·2	−13·4	·973	−6·3	·911	+5·4	−7·7	−0·3	+0·6
Dec. 12·25	542·25	+2·8	−14·2	4·024	−3·5	1558·945	+6·0	−8·5	+0·4	−2·1
2011 Apr. 9·06	55660·06	−8·9	+2·0	4·063	−3·1	1576·415	−5·5	+7·8	−0·4	−2·8
May 9·96	690·96	+3·5	−13·6	·073	−3·1	1580·998	+6·4	−9·1	+0·2	−1·4

* Observed with original spectrometer; wt. 0.
† Observed with ESO *Coravel*; weight ¼.
‡ Observed with DAO spectrometer; weight ¼.
§ Observed with Cambridge *Coravel*; weight 1.

somewhat oblique, determinations of the primary velocity, which are open to the objection that they depend too much on the orbit already determined without them and that they cannot therefore be used in good conscience to support that orbit — that could be seen as an effort to 'haul oneself up by one's bootlaces'! The writer, while sensitive to that criticism, considers that it would be possible to dull its edge to some extent by pleading in mitigation that (*a*) the corrections applied to the measured velocities are at most about ⅕ of the velocity amplitude and are quite accurately determined, so their own uncertainties are negligible, and (*b*) as far as the inner orbit is concerned, the salvaged observations are much better distributed in phase than those reduced as double-lined.

The orbits (both inner and outer) derived when the salvaged observations are included in the solution are very similar (within one standard deviation in all elements) to those obtained when they are omitted, but the standard deviations themselves are modestly reduced. If there were an argument as to the acceptability of the extra observations, those facts might be deployed on either side, depending on one's point of view! The writer, sitting on the fence and not coming down too definitely on either side of it, finds that the 'proper' weighting of the extra data, which would bring their weighted r.m.s. residuals into line with those of the double-lined observations, would be ½, instead of which he has elected to weight them ¼. (The observations made with the original Cambridge spectrometer are much worse and have been zero-weighted.)

Whatever objections may be levelled at the use of the extra observations to bolster the 6-day orbit from which the dragging corrections were determined, they can hardly apply with the same force to the outer orbit, in which the phases in the 6-day orbit are completely 'scrambled', so any errors that might depend systematically upon those phases would merely contribute slightly to the *random* errors. It is instructive to see the improvement that is made to the

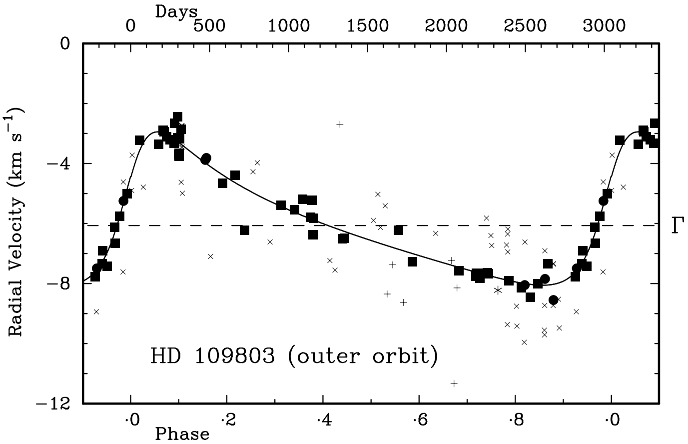

FIG. 2a

The observed radial velocities of the primary component of HD 109803, as a function of phase in the 3000-day outer orbit. Squares plot the radial velocities obtained with the Cambridge *Coravel*, circles those from the OHP one. The small crosses denote observations made at OHP but reduced as single-lined; the resulting velocities are therefore heavily weighted towards the primary, but are 'dragged' some way towards the γ-velocity of the inner binary by blending with the small secondary dip. The effect runs systematically with phase in the inner orbit, but is unrelated to phase in the outer orbit, where it simply produces a wide scatter around the velocity curve. There are also a few plusses, which denote analogous single-lined observations that were obtained with the original Cambridge instrument.

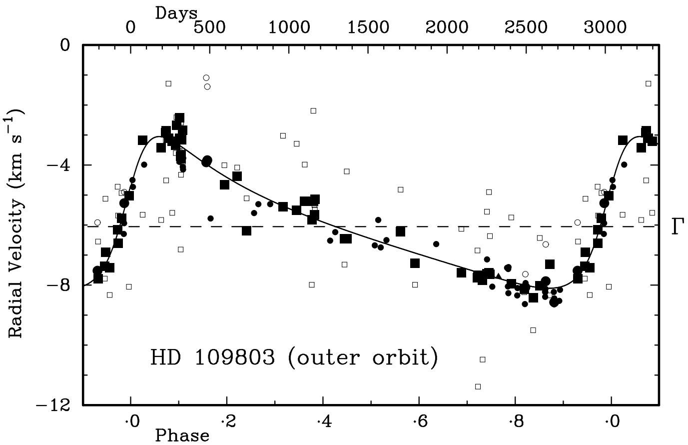

FIG. 2b

Basically the same as Fig. 2a, but here the single-lined OHP measurements have been (*a*) corrected for blending by the 'dragging function' procedure, (*b*) incorporated in the orbital solution with low weight (¼), and (*c*) added to the plot as small filled circles. Also added to the plot are points (open symbols corresponding to the filled ones of the primary) representing the secondary component of the inner binary, whose measurements are inevitably very ragged.

appearance of the outer orbit by the application of the dragging corrections. That comparison is made in Fig. 2. Fig. 2a plots the solution of the outer orbit on the basis of the double-lined measurements alone, with the single-lined ones additionally plotted 'raw'; in Fig. 2b the dragging corrections have been applied (and the orbit has been re-determined as noted in the last paragraph, although the changes between the two diagrams are hardly detectable). Fig. 2b, only, plots the observations of the secondary star, which form a very diffuse cloud of points owing to their large errors, and would tend to confuse Fig. 2a which already has one rather diffuse cloud. Fig. 3 illustrates the inner orbit; it includes the salvaged single-lined data. The final orbital elements are as follows:

Element		*Outer Orbit*	*Inner Orbit*
P	(days)	3038 ± 8	6·74326 ± 0·00004★
T, T_0	(MJD)	52430 ± 13	52164·253 ± 0·016
Γ†	(km s^{-1})	−6·05 ± 0·05	
K_1	(km s^{-1})	2·53 ± 0·07	6·36 ± 0·05
K_2	(km s^{-1})		9·09 ± 0·25
q	$(= m_1/m_2)$		1·43 ± 0·04
e		0·538 ± 0·024	0
ω	(degrees)	290 ± 3	undefined
$a_1 \sin i$	(Gm)	89·0 ± 2·9	0·590 ± 0·005
$a_2 \sin i$	(Gm)		0·843 ± 0·024
$f(m_1)$	($M_\odot$)	0·00305 ± 0·00030	0·000180 ± 0·000004
$f(m_2)$	($M_\odot$)		0·00053 ± 0·00004
$m_1 \sin^3 i$	($M_\odot$)		0·00152 ± 0·00010
$m_2 \sin^3 i$	($M_\odot$)		0·00106 ± 0·00004

R.m.s. residual (unit weight) = 0·33 km s^{-1}

★The true period (in the rest-frame of the system) is 6·74339 ± 0·00004 days; the difference from the observed period is 3·0 standard deviations.

†The capital Γ denotes the 'grand' γ-velocity of the whole system, as distinct from the γ-velocity of the inner sub-system, which varies with time.

If the imposition of circularity on the inner orbit is relaxed, the orbit is found to show an eccentricity of 0·015 ± 0·011, with ω = 100° ± 42°. The sum of the squares of the weighted deviations is reduced only from 17·27 to 17·03 (km s^{-1})^2, a reduction that Bassett's[17] second test shows to be nowhere near significant.

The short period of the inner orbit and the lack of any measurable projected rotational velocities practically guarantees that the two observed components of the HD 109803 system are main-sequence stars. The fivefold disparity in their dips in radial-velocity traces is arithmetically equivalent to a difference of $1^{\mathrm{m}}\cdot74$ in stellar-magnitude terms, and multiplication of that number by the empirical factor of 1·15 adopted by Griffin & Suchkov[18] as applicable to F/G dwarfs, to obtain the equivalent difference in V magnitudes, leads to a ΔV very close to two magnitudes. To obtain agreement with the colour indices, the spectral types to be assigned to the components are F8 and K0. Table III demonstrates how accurately such a pair reconstructs the observed colour indices of HD 109803; with masses close to 1·2 and 0·8 $M_\odot$ it also agrees nicely with the observed mass ratio. The integrated absolute magnitude of the system is seen to be $3^{\mathrm{m}}\cdot83$; it leads to a distance modulus of $5^{\mathrm{m}}\cdot55$ and thus to a distance of about 129 pc or a parallax of $0''\cdot078$.

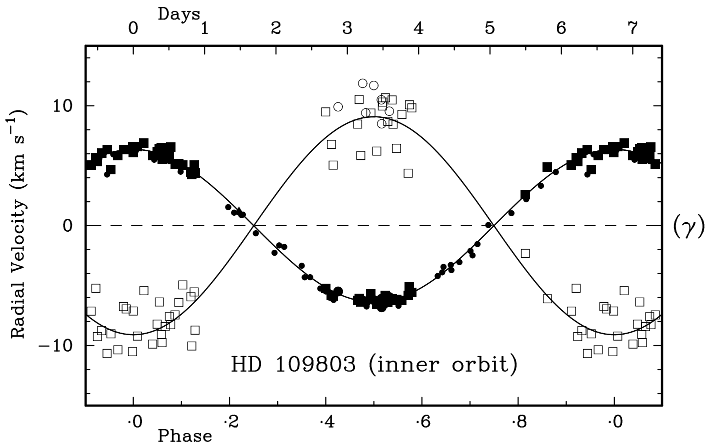

FIG. 3

Plot illustrating the 6·74-day inner orbit of HD 109803. The coding of the symbols is the same as in Fig. 2b.

TABLE III

Photometric model (absolute magnitudes, colour indices) for HD 109803

Star		M_V	$(B-V)$	$(U-B)$	M_B	M_U
		m	*m*	*m*	*m*	*m*
Model	F8V	4·0	0·52	0·04	4·52	4·56
	K0V	5·9	0·89	0·47	6·79	7·26
	F8V+K0V	3·83	0·56	0·08	4·39	4·47
HD 109803 (observed)		9·38*	0·57	0·08		

**Apparent* magnitude. The implied distance modulus is $5^{\mathrm{m}}{\cdot}55$, $\pi_{sp} = 0''{\cdot}078$.

The orbital elements determine the masses only as their product with the factor $\sin^3 i$, which by comparison with the actual masses suggested by the spectral types is seen to be about 0·0013, so $\sin i$ is close to $\sqrt[3]{0{\cdot}0013}$, or nearly 0·11, and the orbital inclination is therefore very close to 6¼°, with an uncertainty unlikely to be more than a fraction of a degree. The likelihood of such a low inclination is very small: it implies that the pole of the orbit is directed little more than 6° away from the line of sight from the star to the Earth, and the proportion of the sky that is so close to a pole is only about 0·6%. The low inclination excuses the stars from showing any appreciable projected rotational velocities; if their rotations are synchronized with the orbit, the equatorial rotational velocity of the primary must be about 9 km s^{-1}, but its projection on the line of sight would be only 1 km s^{-1}, and that of the secondary would be even less.

The mass function of the outer orbit, too, is very small, and is less informative than that of the inner orbit because we know the mass of only one component — the primary, which is of course the observed 6-day system whose total mass is surely very close to 2 $M_{\odot}$. The mass function then shows the *minimum* mass of the secondary to be 0·26 $M_{\odot}$, corresponding to that of a main-sequence star near type M4; it could be more massive than that, of course, to any extent, although in practice the third star could scarcely be as bright as the observed secondary without seriously confusing the radial velocities by blending with the dips given by the recognized components.

HD 109954

This star is to be found about 4° south-following the centre of the Coma Cluster and 2½° north of the 5^{m} object 26 Com, for which an orbit was given in the first paper[19] in a different series from the present one, in the *Journal of Astrophysics & Astronomy*. HD 109954 is not itself a member of the Coma Cluster, although it does feature in an astrometric catalogue[20] by Abad & Vicente of that vicinity. Photometry by Häggkvist & Oja[21] yielded the values $V = 8^{m}{\cdot}42$, $(B-V) = 0^{m}{\cdot}86$, $(U-B) = 0^{m}{\cdot}57$; the V magnitude is in none too good agreement with the $8^{m}{\cdot}47$ listed in Table I. Hansen & Radford[22] obtained narrow-band filter photometry in the *Copenhagen* system[23], which although not primarily directed towards obtaining the ordinary V magnitude does allow a transformation that estimates that quantity: they obtained $8^{m}{\cdot}49$. They also found a *res*(k) value of $0^{m}{\cdot}083$; a *res*(k) of more than $0^{m}{\cdot}04$ is considered to flag up a significant anomaly in the distribution of flux with wavelength, usually to be interpreted in terms of the light originating in two distinct sources with different temperatures. It seems possible that such an anomaly could vitiate to some extent the transformation of narrow-band measurements to the V magnitude, since the *Copenhagen*[23] and *DDO*[1] results agree with one another but not with the directly-observed broad-band V.

Radial velocities have been published for HD 109954 by Woolley *et al.*[24] and by Sandage & Fouts[25], but in both cases the admitted uncertainties are about 5 km s^{-1} and the double-lined nature of the object was not recognized, so those results cannot contribute to the present study of the star. The first Cambridge observation was made in 1971, but it was not until 1988, when the third measurement was made (still with the original spectrometer[12]), that the discordant velocity demonstrated the binary nature of the object and the asymmetry of the trace also showed that it was double-lined. It was some years later, in 1992, that traces made with the OHP *Coravel* showed two dips that were completely resolved from one another. It transpired that the primary dip moved with a 65-day period that the weaker one did not share; instead, the γ-velocity of the primary changed slowly, in anti-phase with a slow change in the secondary's velocity. It is only at a nodal phase in the outer orbit that the two dips ever become completely separated; recently that phase has recurred, after an interval of 18 years that represents the period of the outer orbit. Fig. 4 shows a trace made with the Cambridge *Coravel* at such a time. In a tabulation of the properties of a lot of late-type giant stars, Famaey *et al.*[26] have noted HD 109954 as a spectroscopic binary and listed a γ-velocity of $-10{\cdot}00 \pm 0{\cdot}30$ km s^{-1} for it; their information was lifted from the data base kept in Geneva of OHP radial velocities, which must include (and in the case of HD 109954 probably consists exclusively of) those that are shown in Table IV below as originating from OHP.

Since 1990 HD 109954 has been kept under tolerably systematic observation by the writer. Altogether 108 radial-velocity measurements have been made — 13 with the original Cambridge spectrometer, 39 at OHP, and 52 with the present Cambridge *Coravel*, plus two each at ESO and the DAO. Unfortunately they are a mixed bag as far as utility for determining the orbits is concerned. Only the OHP and Cambridge *Coravel* observations have been reduced as double-lined;

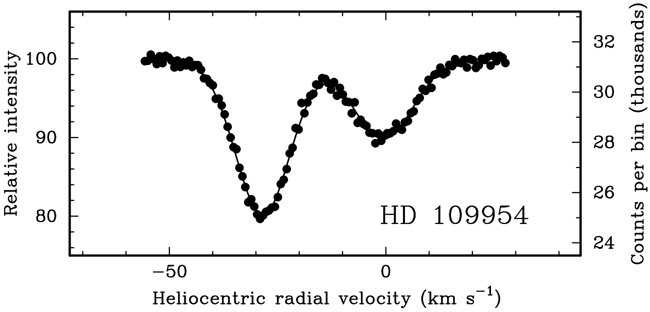

FIG. 4

Radial-velocity trace of HD 109954, obtained with the Cambridge *Coravel* on 2010 May 6, when the nodal passage in the outer orbit (which has a larger amplitude than the inner one despite its 100-fold longer period) permitted the two dips to be seen separately. Most of the time they are blended together.

since the two dips are almost always blended together, single-lined reductions are in most cases not merely unhelpful but can be downright misleading. In the case of HD 109954 blends cannot usefully even be plotted, as they *can* for a normal double-lined binary, in an orbit diagram, where they typically form a sequence that follows broadly the variation of the primary component but with a muted amplitude: in the present instance, the blending has capricious effects and does not necessarily act even qualitatively in the direction of the γ-velocity. Thus we are obliged to omit altogether from consideration the measurements that have been reduced only as single-lined.

Then the reduction of the OHP observations leaves a lot to be desired. Most of them (32) have been reduced as double-lined, but for each of them it has been done independently. Whereas if the dip parameters (depth and width) had been averaged from the best-integrated and best-resolved traces and those averages were imposed on the rest, a good data set would have been obtained, as it stands the set is unsatisfactory. The dip profiles were approximated by two Gaussians, but the computer was not told that they could never be narrower than the instrumental profile. Some of the widths attributed by the computer are far too narrow (down almost to ⅔ of the true minimum width in the worst case) and the corresponding dips are consequently placed much too near the edge of the blend that they are supposed to be modelling. In other cases (and some of the same ones) the relative sizes of the two dips are far from the factor of two to which the best-resolved ones adhere very closely. It has therefore been necessary to reject those observations that attribute to the model dips parameters that are excessively far removed from those that we know to be correct. The rejections have been objectively made by reference to dip parameters being out of acceptable range and not because the writer did not like the velocity residuals. Even those observations that remain are more or less subject to errors that would be readily avoided in a new reduction, which could be made if only there were a way of getting it done. The observations themselves are good and could support an improvement on the orbits that can be offered here.

After the rejections noted in the previous paragraph had been made, there remained 25 pairs of OHP velocities that could be included in the determination

TABLE IV

Radial-velocity observations of HD 109954

The sources of the observations are as follows:
1991–1996 — Haute-Provence Coravel (wt. 1/10); 2000–2011 — Cambridge Coravel (wt. 1)

Date	*MJD*	*Velocity* B km s^{-1}	*Velocity* Aa km s^{-1}	*Phase (outer)*	*(O – C)* B km s^{-1}	*Phase (inner)*	*Computed vel.* outer km s^{-1}	*Computed vel.* inner km s^{-1}	*(O – C)* Aa km s^{-1}
1991 Dec. 17·19	48607·19	0·0	−18·1	0·033	−0·9	0·487	−19·9	+1·6	+0·2
1992 Jan. 18·16	48639·16	+0·2	−22·5	0·038	−1·0	0·980	−20·1	−2·8	+0·4
21·16	642·16	−0·2	−27·5	·039	−1·4	1·027	−20·1	−7·6	+0·2
Apr. 23·93	735·93	+2·1	−18·7	·053	+0·5	2·473	−20·5	+1·4	+0·4
27·95	739·95	+2·1	−17·9	·053	+0·5	·535	−20·5	+2·1	+0·4
June 19·95	792·95	0·0	−20·9	·061	−1·7	3·353	−20·5	−0·4	0·0
24·93	797·93	+0·2	−19·9	·062	−1·5	·430	−20·5	+0·8	−0·2
Aug. 13·84	847·84	+0·7	−23·8	·069	−1·0	4·200	−20·5	−3·6	+0·3
16·82	850·82	+0·1	−22·9	·070	−1·6	·246	−20·5	−2·4	0·0
Dec. 17·19	973·19	−0·3	−25·4	·088	−1·6	6·134	−20·2	−5·6	+0·4
18·22	974·22	0·0	−25·8	·088	−1·3	·150	−20·2	−5·1	−0·5
19·19	975·19	−0·1	−25·0	·088	−1·4	·165	−20·2	−4·6	−0·2
20·18	976·18	−1·2	−24·5	·088	−2·5	·180	−20·2	−4·1	−0·2
22·18	978·18	−0·5	−23·9	·089	−1·8	·211	−20·2	−3·3	−0·5
1993 Feb. 13·07	49031·07	−0·2	−27·2	0·097	−1·2	7·027	−19·9	−7·6	+0·4
18·11	036·11	0·0	−26·4	·097	−1·0	·104	−19·9	−6·7	+0·2
Mar. 18·08	064·08	−1·0	−18·1	·102	−1·8	·536	−19·8	+2·2	−0·5
25·01	071·01	0·0	−17·4	·103	−0·8	·643	−19·8	+3·3	−0·9
July 7·86	175·86	−0·5	−21·4	·118	−0·6	9·260	−19·2	−2·1	−0·1
1994 Jan. 1·23	49353·23	−1·6	−23·1	0·145	−0·4	11·997	−18·1	−5·0	0·0
Aug. 2·87	566·87	−4·0	−19·2	·177	−1·1	15·293	−16·8	−1·4	−1·0
1995 June 4·94	49872·94	−6·4	−22·5	0·222	−1·3	20·015	−15·0	−6·8	−0·7
5·95	873·95	−4·6	−22·3	·223	+0·5	·030	−15·0	−7·8	+0·4
6·93	874·93	−5·3	−23·2	·223	−0·2	·045	−15·0	−8·1	−0·1
1996 Apr. 24·98	50197·98	−7·2	−21·3	0·271	−0·1	25·029	−13·3	−7·7	−0·3
2000 Jan. 10·24	51553·24	−14·1	−6·2	0·473	−0·6	45·937	−8·1	+1·8	+0·1
20·07	563·07	−14·6	−15·0	·475	−1·1	46·088	−8·1	−7·2	+0·3
Feb. 11·15	585·15	−14·1	−6·7	·478	−0·5	·429	−8·0	+0·8	+0·5
Mar. 4·09	607·09	−12·8	−3·6	·481	+0·9	·768	−7·9	+4·3	0·0
Apr. 10·93	644·93	−13·8	−8·2	·487	0·0	47·351	−7·8	−0·4	0·0
17·94	651·94	−13·7	−6·7	·488	+0·1	·459	−7·8	+1·2	−0·1
23·99	657·99	−13·7	−5·5	·489	+0·1	·553	−7·8	+2·3	−0·1
30·00	664·00	−13·7	−4·3	·490	+0·2	·646	−7·8	+3·3	+0·2
May 13·94	677·94	−15·3	−3·4	·492	−1·4	·861	−7·7	+4·3	0·0
2001 May 12·98	52041·98	−14·7	−5·3	0·546	+0·4	53·477	−6·8	+1·4	0·0
31·91	060·91	−15·3	−2·0	·549	−0·1	·769	−6·7	+4·3	+0·4
2002 Feb. 14·12	52319·12	−15·3	−2·1	0·588	+0·6	57·752	−6·1	+4·2	−0·2
Mar. 29·02	362·02	−15·3	−5·2	·594	+0·7	58·414	−6·0	+0·6	+0·2
Apr. 23·98	387·98	−15·5	−1·4	·598	+0·6	·814	−5·9	+4·5	+0·1
2003 Jan. 11·23	52650·23	−17·3	−1·3	0·637	−0·5	62·860	−5·4	+4·3	−0·2
Feb. 21·07	691·07	−16·7	−3·6	·643	+0·2	63·490	−5·3	+1·6	+0·1
Mar. 15·07	713·07	−17·1	−1·5	·647	−0·2	·829	−5·3	+4·5	−0·7
20·08	718·08	−16·9	−1·9	·647	0·0	·907	−5·3	+3·3	0·0
23·09	721·09	−17·6	−5·0	·648	−0·7	·953	−5·3	+0·4	−0·1
23·97	721·97	−16·5	−6·2	·648	+0·4	·967	−5·3	−1·1	+0·2
Apr. 16·97	745·97	−15·9	−5·9	·651	+1·1	64·337	−5·2	−0·6	−0·1
May 13·97	772·97	−16·9	−1·1	·655	+0·1	·753	−5·2	+4·2	−0·2
17·99	776·99	−17·3	−0·9	·656	−0·3	·815	−5·2	+4·5	−0·2

TABLE IV (*concluded*)

Date	*MJD*	*Velocity* B km s⁻¹	*Velocity* Aa km s⁻¹	*Phase (outer)*	*(O–C)* B km s⁻¹	*Phase (inner)*	*Computed vel. outer* km s⁻¹	*Computed vel. inner* km s⁻¹	*(O–C)* Aa km s⁻¹
2004 Mar. 30·06	53094·06	−17·8	−0·9	0·703	−0·2	69·707	−4·7	+3·9	−0·1
Apr. 5·04	100·04	−17·8	−0·3	·704	−0·2	·799	−4·7	+4·5	−0·1
June 4·98	160·98	−18·9	−0·8	·713	−1·2	70·739	−4·6	+4·1	−0·3
2005 May 4·99	53494·99	−17·4	−0·4	0·763	+0·6	75·892	−4·4	+3·8	+0·2
June 27·92	548·92	−18·1	0·0	·771	−0·1	76·724	−4·4	+4·0	+0·4
2006 Mar. 1·10	53795·10	−18·1	−2·5	0·808	−0·3	80·522	−4·6	+2·0	+0·1
May 10·99	865·99	−18·5	−1·6	·819	−0·9	81·615	−4·7	+3·0	+0·1
21·96	876·96	−17·7	−0·3	·820	−0·1	·784	−4·7	+4·4	0·0
2007 Feb. 3·17	54134·17	−17·7	−1·4	0·859	−1·1	85·752	−5·5	+4·2	−0·1
Mar. 27·04	186·04	−17·8	−3·8	·867	−1·5	86·553	−5·8	+2·3	−0·4
Apr. 12·03	202·03	−17·1	−1·7	·869	−0·9	·799	−5·9	+4·5	−0·3
2008 Feb. 16·15	54512·15	−13·0	−5·6	0·915	+0·1	91·583	−8·4	+2·7	+0·1
Mar. 5·17	530·17	−11·8	−3·9	·918	+1·1	·861	−8·6	+4·3	+0·4
May 2·96	588·96	−11·4	−4·4	·927	+0·6	92·768	−9·3	+4·3	+0·6
18·97	604·97	−11·1	−16·9	·929	+0·6	93·015	−9·5	−6·9	−0·5
2009 Jan. 14·20	54845·20	−7·3	−9·5	0·965	−0·2	96·721	−13·3	+4·0	−0·2
Feb. 4·23	866·23	−4·6	−21·0	·968	+2·0	97·046	−13·7	−8·1	+0·8
Apr. 9·04	930·04	−4·8	−22·7	·978	+0·4	98·030	−14·8	−7·8	−0·1
June 18·93	55000·93	−2·2	−22·1	·988	+1·5	99·124	−16·1	−6·0	−0·1
2010 Apr. 4·98	55290·98	+1·5	−16·7	1·032	+0·7	103·599	−19·8	+2·9	+0·3
May 6·95	322·95	+0·7	−27·1	·036	−0·4	104·092	−20·0	−7·1	0·0
11·93	327·93	+1·1	−24·7	·037	0·0	·169	−20·1	−4·5	−0·2
June 3·96	350·96	+1·1	−17·9	·041	−0·2	·524	−20·2	+2·0	+0·3
July 5·92	382·92	+0·7	−27·4	·045	−0·8	105·017	−20·3	−7·0	−0·1
9·92	386·92	+1·2	−28·0	·046	−0·3	·079	−20·3	−7·5	−0·1
2011 Jan. 19·24	55580·24	+1·9	−28·6	1·075	+0·3	108·061	−20·4	−8·0	−0·2
Apr. 7·11	658·11	+1·4	−22·6	·086	+0·1	109·263	−20·2	−2·0	−0·3
May 24·02	705·02	+0·8	−23·6	·093	−0·3	·986	−20·0	−3·6	0·0
24·92	705·92	+0·4	−25·3	·094	−0·7	110·000	−20·0	−5·4	+0·1

of the orbits. Those, only, are listed in Table IV, with the 52 pairs obtained with the Cambridge *Coravel*. It seems undesirable to publish the OHP velocities that are manifestly wrong; and there is little purpose in listing observations that have been reduced as single-lined and cannot contribute to the orbits nor even can sensibly be plotted on the graphs.

Some explanation may be called for regarding the presentation of the observational results on HD 109954 and the fitting of them by the orbital solutions. Owing to the logical need to present the outer orbit before the inner one because the motion in the former determines the instantaneous γ-velocity of the latter, Table IV gives the radial velocities, phases, and residuals of the component that is here called B before the corresponding quantities for A, the star that is observationally the primary. The computed velocity of A (only) has contributions, which are separately specified in the 'Computed velocity' columns, from both orbits.

For the solution of the orbits the writer would feel more confident if he could fall back on the Cambridge velocities alone, but that is impractical because they do not cover a complete cycle of the 18-year outer orbit, and it would be unrealistic for him to think of watching the system until the next nodal passage, in 2029. A formal equalization of the variances for the two data sources and the two components points to a weighting of Cambridge 1, OHP ½, and to primary 1, secondary ⅐. Concern over possible biasses in the remaining OHP velocities has induced the author arbitrarily to down-weight them to ⅒. Comparison of the resulting orbital elements with those that are obtained with the formally correct weighting, and with those obtained before any rejections were made at all, shows that the elements do not actually differ much between the various solutions, except that for the period and periastron epoch in the outer orbit the standard errors are naturally increased by the down-weighting of the OHP data upon which the timing of the earlier (1992) nodal passage depends. Perhaps a bit perversely, that could be seen as an actual advantage, softening what could well prove in the future to be claims of unrealistic precisions for those elements. The finally adopted elements are presented here, and are illustrated in Figs. 5 and 6.

Element		*Outer Orbit*	*Inner Orbit*
P	(days)	6696 ± 34	64·8210 ± 0·0020
T	(MJD)	48384 ± 37	51038·80 ± 0·15
Γ	(km s^{-1})	−10·52 ± 0·04	
K_1	(km s^{-1})	8·06 ± 0·06	6·28 ± 0·07
K_2	(km s^{-1})	9·85 ± 0·12	
q	$(= m_1/m_2)$	1·221 ± 0·018	
e		0·400 ± 0·005	0·511 ± 0·007
ω	(degrees)	127·3 ± 1·0	124·2 ± 1·1
$a_1 \sin i$	(Gm)	681 ± 7	4·81 ± 0·06
$a_2 \sin i$	(Gm)	831 ± 11	
$f(m_1)$	$(M_\odot)$	0·281 ± 0·007	0·00106 ± 0·00004
$f(m_2)$	$(M_\odot)$	0·511 ± 0·019	
$m_1 \sin^3 i$	$(M_\odot)$	1·691 ± 0·036	
$m_2 \sin^3 i$	$(M_\odot)$	1·385 ± 0·027	

R.m.s. residual (unit weight) = 0·23 km s^{-1}

The *Hipparcos* parallax, both in the original publication[27] and in the recent revision[28], gives the absolute magnitude of HD 109954 as close to $+1^{m}$, though with a 1-σ uncertainty of about $0^{m}\!\cdot\!6$. The disparity of a factor of two between the dip signatures of the components, which in the absence of any clue as to their colour difference we must take as their luminosity difference, equivalent to three-quarters of a magnitude, makes their individual absolute magnitudes about $+1^{m}\!\cdot\!4$ and $+2^{m}\!\cdot\!2$. The former might well be a 'clump' giant, and the latter may be at an earlier stage of its evolution, still low down on the giant branch of the H–R diagram. Since they are both evolving at the same time, they could be expected to have rather similar masses, so most of the difference of $0{\cdot}3/\sin^3 i\, M_\odot$ may plausibly be assigned to the unseen companion in the 65-day orbit with the more luminous star. The $\sin^3 i$ factor is not likely to be less than about ⅔, otherwise the masses of the two observed components would become excessive, so the unseen star probably has a mass well under 0·5 $M_\odot$ and may be expected to be an early-M dwarf, with a luminosity only of the order of a thousandth of that of the other two stars.

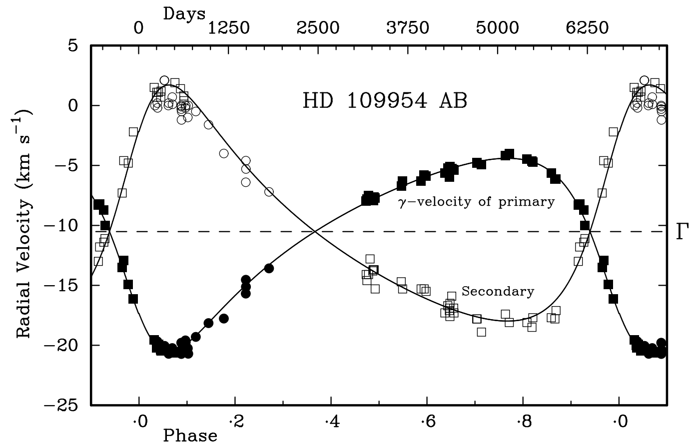

FIG. 5

Radial velocities of HD 109954 plotted against phase in the outer orbit, with the velocity curves corresponding to the adopted orbital elements drawn through them. As for HD 109803, primary observations are denoted by filled symbols, secondary ones by open symbols; squares represent Cambridge, circles OHP.

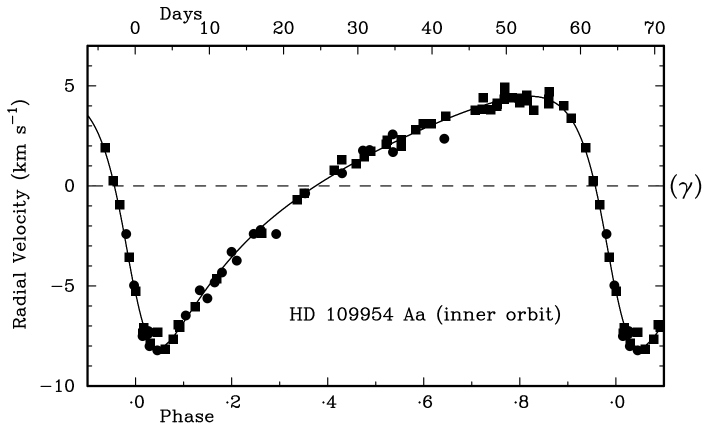

FIG. 6

The inner orbit of the primary component of HD 109954. The coding of the points is the same as in Fig. 5

HD 110195

HD 110195 is to be found about 3° following γ Com. The writer has occasion to take a second bite at this particular cherry, having already given[29] an orbit for the system some 25 years ago. He showed then that it is a nearly-equal double-lined binary consisting of two late-G dwarfs differing in brightness by about $0^{m}\cdot15$ and circulating in an orbit with a period of just under 18 days. It is a particularly faint object for a *Henry Draper Catalogue* star, having a *V* magnitude slightly fainter than 10 and seeming to be variable over a small range, probably owing to star-spots. Not only is it faint, but the radial-velocity dips are shallow, their depths being halved because there are two of them in each trace. Because it was a difficult object to measure with the original radial-velocity spectrometer[12], the orbit relied to an unusual extent on observations obtained with the spectrometer made and operated by Griffin & Gunn[30] at the coudé focus of the Palomar 200-inch telescope, for which of course it was an easy object. An example of a Palomar trace was included in the previous paper[29].

A troublesome feature of the original investigation was that the first Palomar observation, which was an isolated one (made in 1978 whereas all the others were obtained in 1981–85) gave altogether unacceptable residuals of about +3 km s^{-1} for both components. The paper frankly discusses those residuals, as follows.

> "They are far too large to be understood as random errors, and adjacent observations of other stars confirm their systematic correctness. The question arises as to how they are to be regarded. To accept the corresponding observation fully would be to imply that HD 110195 has a variable γ-velocity and is therefore a triple system; the author is not prepared to do that on the basis of one observation, although it is true that that is the only way in which the 1978 observation can be reconciled with the others. To reject the observation without any better reason than discomfiture over its residuals would be too arbitrary. … The possibility of variation in the γ-velocity, like that of photometric variability, is one that warrants investigation in the future."

The prospect of a campaign that would inevitably take many years and might well be inconclusive even then, on a particularly faint and difficult star, did not commend itself too strongly to the author, who neglected to undertake it. As luck would have it, however, HD 110195 was observed by others at OHP, and in late 2008 Mermilliod, Grenon & Mayor published a paper[31] referring to a lot of radial velocities measured for stars in the vicinity of the Coma Cluster; among them was HD 110195, for which they determined an orbit, though without mentioning that one[29] already existed. The underlying radial-velocity measurements were to be made available through the Centre de Données Stellaires, although that was not so at the time and does not appear to be so even now. However, Dr. Mermilliod was kind enough to provide his observations of HD 110195 to the present writer directly, and it was immediately evident that they supported the idea (which had not occurred to the OHP authors[31]) that the system is triple. A moral is not far to seek in that narrative of events, to the effect that honesty is the best policy: by drawing attention to a 'bad' observation (rather than sweeping it under a carpet), one may with better face capitalize on it when — albeit after a long time — the observation is seen not only to be 'good' after all but to represent in retrospect something of a discovery! There is a fairly accurate parallel with "The stone which the builders rejected …"[32].

Even when the Mermilliod observations were brought into the discussion, considerable uncertainty remained in the period of the outer orbit. There existed a huge (16-year) time gap since the observational coverage ended, so there seemed to be a good possibility of determining the orbital period by making some fresh observations to establish the current phase. The star was therefore restored to the Cambridge observing programme and has been measured occasionally in the last three seasons. The seasonal means show a gentle rise in velocity which definitely places the present phase on the rising, as opposed to the descending, branch of the orbit, and defines the phase (and thereby the orbital period) tolerably well, although those quantities are slightly at the mercy of the correctness of the assigned zero-point differences between the various instruments used in the observations.

The available radial velocities are listed in Table V. Those from OHP (supplied in this instance by Mermilliod) have as usual been increased by 0·8 km s^{-1}. On the basis of previous experience of the colour dependence of the zero-point of the Cambridge *Coravel*, the recent measurements have been corrected by –0·5 km s^{-1}; the velocities that featured in the 1985 orbit paper[29] have been transcribed *verbatim* into Table V. New weighting has, however, been adopted: the early Palomar velocities are here accorded unit weight, the same as the OHP and Cambridge data, while the DAO and Cambridge observations from the 1985 paper have been weighted ½ and ⅒ respectively. Measurements of the secondary have had their weights multiplied by 0·7. On that basis the two orbits with their total of twelve elements have been solved with the same program as was used for HD 109803, with the results shown in Figs. 7 and 8. The elements are as follows:

Element		*Outer Orbit*	*Inner Orbit*
P	(days)	7960 ± 210	17·778584 ± 0·000010
T	(MJD)	52700 ± 250	48740·836 ± 0·005
Γ	(km s^{-1})	−14·54 ± 0·09	
K_1	(km s^{-1})	2·02 ± 0·12	55·49 ± 0·12
K_2	(km s^{-1})		56·39 ± 0·20
q	($= m_1/m_2$)		1·016 ± 0·004
e		0·56 ± 0·05	0·5688 ± 0·0011
ω	(degrees)	124 ± 6	59·80 ± 0·20
$a_1 \sin i$	(Gm)	183 ± 14	11·159 ± 0·027
$a_2 \sin i$	(Gm)		11·340 ± 0·041
$f(m_1)$	($M_\odot$)	0·0038 ± 0·0008	0·1756 ± 0·0013
$f(m_2)$	($M_\odot$)		0·1842 ± 0·0020
$m_1 \sin^3 i$	($M_\odot$)		0·725 ± 0·006
$m_2 \sin^3 i$	($M_\odot$)		0·714 ± 0·005

R.m.s. residual (unit weight) = 0·54 km s^{-1}

*The true period (in the rest-frame of the system) is 17·779445 ± 0·000012 days; the difference from the observed period is 73 standard deviations.

The three sets of elements for the inner orbit (from refs. 29 and 31, and here) are assembled in Table VI and are seen to be in good agreement with one another. Since the new orbit incorporates the observations that went into both of the previous ones, and more besides, it is not surprising that it is formally more precise than its predecessors. The present work breaks entirely new ground, however, in the definite recognition that the HD 110195 system is really triple, and in providing what is thought to be a reasonably close approximation to the 22-year outer orbit. The seeming anomaly of the initial Palomar observation, already suspected in 1985 as indicating higher multiplicity, is now seen to be entirely explained as arising from a real change in the γ-velocity of the inner binary, and is fully supported by the almost-contemporaneous OHP measurements that have only recently come to light.

The observed pair of stars, which are clearly main-sequence objects, are nearly equal both in mass and in their signatures in radial-velocity traces; having a combined $(B - V)$ of $0^{\mathrm{m}}{\cdot}74$, they could be estimated to have masses close to 0·9 $M_\odot$. The orbital elements give their masses as $0{\cdot}72/\sin^3 i$ $M_\odot$, from which we deduce that $\sin^3 i \sim 0{\cdot}8$, or $i \sim \arcsin\sqrt[3]{0{\cdot}8}$ — about 70°, though with no great precision owing to the high sensitivity of i to $\sin i$ when $\sin i$ approaches

TABLE V

Radial-velocity observations of HD 110195

Except as noted, the sources of the observations are as follows:
1979–1993 — OHP Coravel (weight 1); 2009–2011 — Cambridge Coravel (weight 1)

Date	*MJD*	*Velocity*		*Outer orbit*		*Inner orbit*			*(O–C)*	
		A km s⁻¹	*B* km s⁻¹	*Phase*	*Velocity* km s⁻¹	*Phase*	*Vel (A)* km s⁻¹	*Vel (B)* km s⁻¹	*A* km s⁻¹	*B* km s⁻¹
1978 May 23·28★	43651·28	+2·7	−29·2	0·864	−13·2	0·725	+16·0	−16·2	−0·1	+0·2
1979 Feb. 4·08	43908·08	−53·6	—	0·896	−13·3	15·170	−39·2	—	−1·1	—
18·12	922·12	+57·6	−86·5	·898	−13·3	·959	+71·3	−72·5	−0·3	−0·7
19·17	923·17	+3·9	−30·1	·898	−13·3	16·018	+17·5	−17·8	−0·3	+1·0
1980 Feb. 13·13	44282·13	−50·6	+24·5	0·943	−13·9	36·209	−37·2	+37·8	+0·5	+0·7
1981 Jan. 30·18	44634·18	+13·9	−44·4	0·987	−15·7	56·011	+28·4	−28·8	+1·2	+0·1
May 4·93†	728·93	−43·4:	—	·999	−16·3	61·340	−26·9	—	−0·2	—
17·35★	741·35	−24·8::	−12·8‡	1·001	−16·3	62·039	−7·5	+7·7	−0·6	−1·5
19·20★	743·20	−56·6	+22·4	·001	−16·3	·143	−39·6	+40·4	−0·6	−1·5
Dec. 4·55★	942·55	−42·6	+10·6	·026	−17·1	73·356	−25·5	+25·9	0·0	+1·7
6·55★	944·55	−32·7	−2·0	·026	−17·1	·469	−15·1	+15·4	−0·5	−0·3
1982 Mar. 4·08†	45032·08	−39·2:	+5·8	1·037	−17·2	78·392	−22·3	+22·7	+0·3	+0·3
13·05†	041·05	+38·2	—	·038	−17·2	·896	+55·9	—	−0·5	—
16·09†	044·09	−45·9:	+10·0	·039	−17·2	79·067	−27·6	+28·0	−1·1	−0·8
Nov. 23·57★	296·57	−49·7	+16·4	·070	−17·0	93·269	−32·9	+33·4	+0·1	0·0
24·57★	297·57	−45·5	+5·5	·071	−17·0	·380	−23·4	+23·8	+0·3	−1·3
26·56★	299·56	−34·5	+2·2	·071	−17·0	·437	−18·2	+18·4	+0·6	+0·7
1983 Feb. 3·49§	45368·49	−46·6	+14·0	1·079	−16·9	97·314	−29·1	+29·6	−0·6	+1·3
4·49§	369·49	−42·1	+8·6	·080	−16·9	·371	−24·2	+24·6	−1·0	+0·8
15·38§	380·38	+44·9	−81·1	·081	−16·8	·983	+62·1	−63·1	−0·4	−1·1
23·11†	388·11	−36·5	+5·5	·082	−16·8	98·418	−20·0	+20·3	+0·3	+2·1
May 15·92†	469·92	−0·9	−35·4	·092	−16·7	103·020	+16·0	−16·3	−0·2	−2·4
June 6·92†	491·92	−52·4	+18·8	·095	−16·7	104·257	−33·8	+34·3	−1·9	+1·1
18·92†	503·92	+52·6	−80·4	·096	−16·7	·932	+67·2	−68·3	+2·1	+4·5
19·92†	504·92	+41·4	−77·3	·097	−16·6	·988	+57·6	−58·6	+0·4	−2·1
1984 Apr. 17·01†	45807·02	+47·2	−82·7	1·134	−16·2	121·980	+64·3	−65·4	−0·9	−1·1
20·99†	811·00	−55·2	+23·1	·135	−16·2	122·204	−37·5	+38·1	−1·5	+1·2
Nov. 29·57★	46033·58	+0·7	−31·6	·163	−15·9	134·724	+15·7	−15·9	+0·9	+0·2
30·53★	034·54	+8·7	−42·0	·163	−15·9	·778	+25·2	−25·7	−0·6	−0·5
Dec. 2·53★	036·54	+38·2	−71·3	·163	−15·9	·890	+53·9	−54·7	+0·2	−0·7
21·24†	055·25	+51·2	−84·3	·166	−15·9	135·942	+69·7	−70·9	−2·7	+2·4
1985 Feb. 17·44§	46113·45	−50·7	+21·9	1·173	−15·8	139·216	−36·7	+37·3	+1·8	+0·4
1986 Jan. 18·19	46448·20	−28·6	−3·0	1·215	−15·5	158·045	−13·1	+13·3	0·0	−0·8
18·20	448·21	−29·9	−1·4	·215	−15·5	·046	−13·6	+13·8	−0·9	+0·2
21·22	451·23	−52·8	+21·1	·215	−15·5	·215	−36·8	+37·4	−0·6	−0·8
1987 Jan. 25·17	46820·18	+54·3	−86·7	1·262	−15·1	178·968	+70·0	−71·1	−0·6	−0·5
28·18	823·19	−55·2	+24·7	·262	−15·1	179·137	−39·6	+40·2	−0·5	−0·3
Apr. 27·98	912·99	−53·2	+23·2	·273	−15·1	184·188	−38·4	+39·0	+0·2	−0·8
1988 Feb. 10·13	47201·14	−36·1	+7·5	1·310	−14·9	200·396	−22·0	+22·3	+0·8	+0·1
15·14	206·15	−5·1	−23·3	·310	−14·9	·678	+8·8	−8·9	+1·0	+0·5
16·07	207·08	+1·6	−32·3	·310	−14·9	·730	+16·7	−17·0	−0·3	−0·5
17·11	208·12	+12·2	−43·1	·310	−14·9	·789	+27·5	−27·9	−0·4	−0·4
Mar. 24·01	244·02	+17·2	−47·1	·315	−14·8	202·808	+31·6	−32·1	+0·5	−0·1

TABLE V (*concluded*)

Date	MJD	Velocity A km s⁻¹	Velocity B km s⁻¹	Outer orbit Phase	Outer orbit Velocity km s⁻¹	Inner orbit Phase	Inner orbit Vel (A) km s⁻¹	Inner orbit Vel (B) km s⁻¹	(O–C) A km s⁻¹	(O–C) B km s⁻¹
1989 Feb. 9·20	47566·21	+52·3	−82·3	1·355	−14·6	220·930	+66·7	−67·8	+0·2	+0·1
Dec. 28·20	888·21	−24·2	−3·9	·396	−14·4	239·042	−10·4	+10·6	+0·7	0·0
29·22	889·23	−50·7	+22·9	·396	−14·4	·099	−36·7	+37·3	+0·5	0·0
30·22	890·23	−53·4	+25·8	·396	−14·4	·156	−39·5	+40·2	+0·6	+0·1
31·22	891·23	−50·7	+23·3	·396	−14·4	·212	−37·0	+37·6	+0·8	+0·2
1990 Jan. 1·23	47892·24	−46·9	+18·6	1·396	−14·4	239·269	−32·9	+33·4	+0·5	−0·3
1991 Feb. 12·17	48299·18	−52·9	+26·1	1·447	−14·2	262·158	−39·5	+40·1	+0·8	+0·2
Dec. 30·21	620·22	−50·9	+22·9	·488	−14·1	280·216	−36·8	+37·4	−0·1	−0·4
31·22	621·23	−47·2	+17·9	·488	−14·1	·272	−32·6	+33·1	−0·5	−1·1
1992 Jan. 1·21	48622·22	−42·4	+14·3	1·488	−14·1	280·328	−28·0	+28·4	−0·4	0·0
2·22	623·23	−37·0	+9·4	·488	−14·1	·385	−23·0	+23·3	+0·1	+0·1
3·23	624·24	−31·7	+2·7	·488	−14·1	·442	−17·7	+18·0	+0·2	−1·2
1993 Jan. 20·21	49007·22	+47·9	−76·1	1·536	−13·9	301·983	+61·9	−62·9	−0·1	+0·7
24·19	011·20	−51·2	+24·3	·537	−13·9	302·207	−37·3	+37·9	0·0	+0·3
2009 Jan. 14·23	54845·24	−39·9	+10·3	2·269	−15·1	630·357	−25·4	+25·8	+0·6	−0·5
Feb. 7·21	869·22	−2·8	−28·6	·272	−15·1	631·706	+12·9	−13·1	−0·6	−0·4
Apr. 21·96	942·97	+27·7	−59·0	·282	−15·0	635·854	+43·1	−43·8	−0·4	−0·2
29·97	950·98	−45·2	+15·7	·283	−15·0	636·305	−29·9	+30·4	−0·3	+0·3
May 17·98	968·99	−43·9	+14·1	·285	−15·0	637·318	−28·8	+29·3	−0·1	−0·2
28·92	979·93	+53·1	−83·5	·286	−15·0	·933	+67·5	−68·6	+0·6	+0·1
29·94	980·95	+40·3	−71·4	·286	−15·0	·990	+55·3	−56·2	0·0	−0·2
31·92	982·93	−52·8	+21·7	·287	−15·0	638·102	−37·1	+37·7	−0·7	−1·0
2010 Mar. 5·06	55260·07	−3·8	−25·9	2·321	−14·8	653·690	+10·6	−10·7	+0·4	−0·4
Apr. 18·02	304·03	−53·5	+24·9	·327	−14·8	656·163	−39·4	+40·0	+0·6	−0·3
May 11·94	327·95	−26·2	−3·5	·330	−14·8	657·508	−11·2	+11·4	−0·3	−0·1
July 4·95	381·96	−21·3	−6·9	·337	−14·7	660·546	−7·2	+7·3	+0·6	+0·5
2011 Apr. 7·05	55658·06	−45·5	+17·5	2·371	−14·6	676·076	−31·1	+31·6	+0·1	+0·5
May 11·04	692·05	+43·9	−72·8	·376	−14·5	677·988	+57·7	−58·7	+0·7	+0·4
24·99	706·00	+10·2	−39·9	·377	−14·5	678·773	+24·3	−24·7	+0·4	−0·7

* Observed with 200-inch telescope, wt. 1.
† Observed with original spectrometer, wt. 1/10.
‡ Zero-weighted observation.
§ Observed with DAO spectrometer, wt. ½.

unity. The mass function in the outer orbit, interpreted in the light of our understanding of the inner system as having a combined mass close to 1·8 $M_\odot$, requires the unseen tertiary star to have a mass not less than about 0·25 $M_\odot$; it is probably another main-sequence star, of late-K or early-M type, but it cannot be later than about M4.

HD 117078

HD 117078 is near the eastern border of Coma, some 5° preceding 6 Boo, a star whose orbit was given by the writer[33] some 25 years ago. (Long afterwards he worried that the determination of that orbit, whose amplitude was little more than 1 km s^{-1}, was almost too ambitious for the original radial-velocity

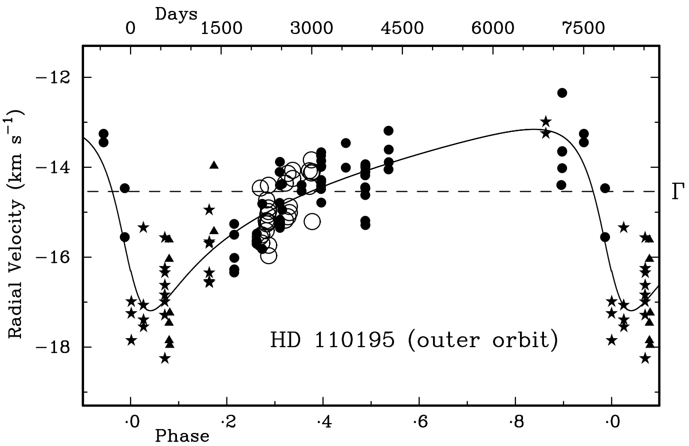

FIG. 7

Illustration of the outer orbit of HD 110195. The two observable components of this triple system are in an 18-day inner binary and are of fairly similar brightness. To avoid too great a proliferation of different symbols, velocities of both components use the same symbols. The original investigation[29] of the inner orbit, which in the absence of a sufficiently long time base was assumed to be the only one, with its γ-velocity constant, depended on the points that are plotted here between phases ·0 and ·2; star-symbols denote data obtained with the spectrometer[30] at the coudé focus of the Palomar 200-inch telescope, triangles plot those obtained at the DAO 48-inch. Additional data that were taken with the original Cambridge spectrometer and contributed to the original orbit have not been plotted because their scatter is bad and tends to make the diagram confusing. There was one temporally outlying Palomar observation, seen here at phase ·86, which gave unacceptable residuals for both components of the binary. It is now seen to be at exactly the opposite node of the outer orbit from the bulk of the early observations, and its residuals are reduced practically to nothing. The filled circles are observations made with the OHP *Coravel* and referred to in a 2008 paper[31] by Mermilliod, Grenon & Mayor; Dr. Mermilliod kindly supplied the data for use here. They were obtained in 1979–81 and 1986–93, so they started shortly after the Palomar observation that seemed discordant, missed the minimum-velocity node in the outer orbit, but documented part of the rising branch. Nearly a whole cycle of the outer orbit has elapsed since the last OHP observation. New measurements made at Cambridge in the last three seasons are plotted as large open circles and clearly identify the current phase in the velocity curve.

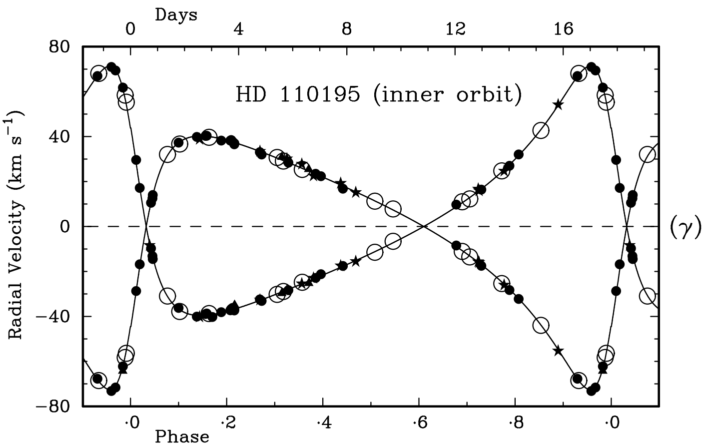

FIG. 8

The inner orbit of HD 110195. The coding of the points is the same as in Fig. 7.

TABLE VI

Comparison of orbital elements for inner orbit of HD 110195

Orbit	*P* days	*K* km s⁻¹ *A*	*B*	*e*	*ω* deg.	*a sin i* Gm *A*	*B*	*m sin³ i* $M_\odot$ *A*	*B*
Camb	17·7786	55·8	55·5	0·563	60·4	11·28	11·22	0·72	0·72
1985	5	·5	·8	5	·8	·10	·17	4	2
OHP	17·77851	55·31	56·43	0·569	59·8	11·12			
2008	4	·25	·27	2	·4	7			
Camb	17·778584	55·49	56·39	0·5688	59·80	11·159	11·34	0·725	0·714
2011	10	·12	·20	11	·20	27	4	6	5

Note: There are two lines for each orbit, the second one giving the standard deviations, with the digits aligned with those of the elements immediately above them. The three successive orbits are those in reference 29, reference 31, and the one given here in this paper.

spectrometer[12], so he restored 6 Boo to the observing programme and obtained a virtually identical result[34] with the *Coravel*!) If reliance were to be placed wholly on the *Simbad* bibliography it would appear that there is no paper giving any magnitude or classification for HD 117078 (though there is of course a spectral type (G5) in the *Henry Draper Catalogue*). Table I above, however, shows that the Yoss–Griffin survey[1] measured the star's magnitude and $(B-V)$ colour index as $9^{m}\cdot59$ and $0^{m}\cdot57$, respectively (there are tolerably accordant values from *Tycho*, too), as well as suggesting a spectral type of G0 V and an absolute magnitude of $4^{m}\cdot4$ from narrow-band photometry. The one and only paper that *Simbad* finds for HD 117078 is that by Tokovinin, Mason & Hartkopf[35]. Those authors reported its resolution by speckle interferometry on the *SOAR* telescope at Cerro Pachon in Chile (near CTIO) in 2009, with an angular separation of 99·6 ± 5 arc-ms and a magnitude difference (qualified by a colon after it) of $1^{m}\cdot9$. A note reads, "This is a newly resolved quadruple, with two SBs orbiting each other with 36-yr period (MSC).". However, in actual fact the *MSC* (*Multiple Star Catalogue*)[36] has no entry for HD 117078; instead, the information in the Tokovinin *et al.* paper must have been quoted (without acknowledgement) from the present writer's contribution at a conference[37].

The principal feature in radial-velocity traces of HD 117078 is a broadened dip, shallow, with a central depression of only about 10%, that gives a velocity varying in a quasi-sinusoidal manner in a period a little under six days. With fortunate timing, careful attention, and longer integrations, another, much *more* exiguous, dip can be observed, one that is only about 4% deep and that moves on a much more leisurely time-scale. Its period of 204 days was first identified by the late Dr. A. Duquennoy, whose untimely demise, shortly after he made that discovery, was chronicled[38] in this *Magazine*. He had in fact agreed to join the present writer in completing a thorough study of the object. Inasmuch as the two observed components of HD 117078 showed variations in different periods, they evidently belonged to separate single-lined binary systems. From the fact that HD 117078 *looks* like a single object in a telescope, it was immediately obvious that the two binaries must constitute a quadruple system, somewhat after the fashion of ε Lyrae and of HD 51565[39]. In due course it became apparent that the binaries were not very distant from one another, because the γ-velocities of both systems proved to vary slowly in opposite senses, so the outer orbit promised to have a period of the order of decades rather than centuries or millennia. Since in a sense each of the observed stars is the primary component in its own sub-system, in what follows the two stars, and their corresponding short-period orbits where appropriate, will be identified as A and B rather than as primary and secondary.

It is only when A is near a node of its orbit that the radial-velocity trace shows the dips tolerably well resolved. For that reason, the observations of the 6-day A orbit are unusually non-uniformly distributed in phase, being concentrated near the nodes. The best separation of the dips occurs when B (which has a smaller velocity amplitude, as befits its much longer period) is near the abrupt minimum in its very eccentric orbit. A trace obtained at a favourable, though not quite an optimal, time is shown here as Fig. 9.

The observational material consists of 69 observations from OHP, 38 from the Cambridge *Coravel*, and three from ESO, which have all been reduced as double-lined apart from five which were deliberately centred so as to show only B, A being beyond the end of the scan. There is in addition a number of measurements of blends, which cannot be used in the solution of the orbits, or even usefully be plotted, for the reason (explained, however opaquely, in the section on HD 109954 above) that in a multiple system the blends do not fall between the velocity curves for the respective components in the way that they do for an ordinary binary system. There is also one A measurement, made by the writer at the DAO, which is useable because it is now found that B must have been well resolved from it at the time; and there is an initial observation with the original Cambridge spectrometer, a marginal result on a blend. As usual, the OHP and ESO measurements have been adjusted by +0·8 km s^{-1}; the Cambridge velocities have been accorded an empirical adjustment of –0·8 km s^{-1} to bring them into systematic agreement with the OHP ones. The orbital solutions exhibit some unusually bad residuals from both of the principal sources; some of the OHP residuals stem from observations which were attributed dip profiles well removed from the mean values, but others with reasonable dip parameters are just as bad. There are certain Cambridge measurements that give extraordinary residuals of A, but no mistake in reduction has been identified. In preference to trying to tinker with the data set to put a better face on it, the decision has been made to accept the data as they stand (apart from one particularly heinous Cambridge observation) and to let matters take

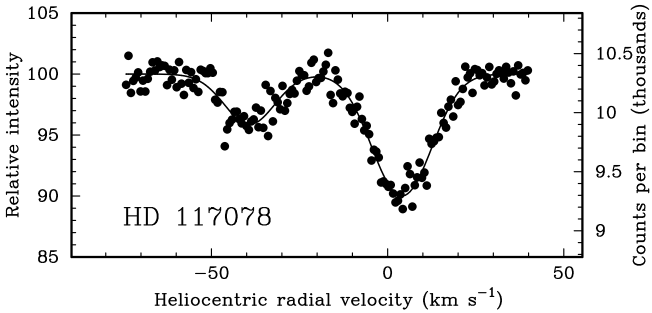

FIG. 9

Radial-velocity trace of HD 117078, obtained with the Cambridge *Coravel* on 2010 April 13, when the two stars were near opposite nodes in their respective short-period orbits and their dips were thereby separated.

their course. That has involved down-weighting the Cambridge data by a factor of three in the case of A, although the measurements of B *from the same traces* give residuals that are somewhat *smaller* than OHP ones (but not by so much as strongly to call for extra weight). The only possible reason that comes to mind for the raggedness of the A observations is that the star observed, having a short period and considerable rotational velocity, is subject to star-spots that were particularly virulent during the era of the Cambridge observations and caused significant asymmetries in the flux contributions from the approaching and receding limbs of the stellar disc. That suggestion might yet be supported if a photometric campaign, such as has never been made on the system so far, were to demonstrate considerable variability, but since such a campaign could not be retrospective it could not positively rule out the suggestion even if the result proved not to support it.

The useable radial velocities are set out in Tables VII and VIII for components A and B respectively. It is a bit unsatisfactory thus to list them in separate tables, because normally (and preferably) the twin velocities derived from each observation would be printed in adjacent columns; then a sufficiently interested reader might, for example, notice whether a bad residual is associated with a possibly over-ambitious effort to split a close blend to obtain the separate velocities. But such a table that would be ideal scientifically would be unacceptable typographically, being too wide for the page.

Solution of the orbits of HD 117078 presents a number of problems, not the least of which is that the observations do not cover a complete cycle of the outer orbit. The available coverage is 'only' about 9000 days (24 years), and really needs to be extended for another similar interval — not a practical proposition for the present author, who would be a centenarian by then. No doubt the proper way of solving the orbits would be by means of a program that deals with all three of them — the long-period outer orbit and the short-period orbits of the individual components — simultaneously. In the author's experience there would be no call apart from the present one for such a program, and the effort of writing it hardly seems warranted for this one system. Especially since no great accuracy can be expected for the long-period orbit which has not been seen round, the oblique approach adopted here may be almost as serviceable [soft-speak for 'not much worse'!]. The principle is to treat each of the short-period systems separately, solving the outer and inner orbits simultaneously; then to look at the two resulting versions of the outer orbit and to align them (in part subjectively, but particularly with an eye to equalizing their γ-velocities); and finally to re-compute the inner orbits, holding the elements of the outer orbit fixed at the chosen values. It may be mentioned that the elements of the inner and outer orbits are almost de-coupled from one another, so those of the inner ones are not substantially changed by modest adjustments to the elements imposed for the outer one.

An obvious initial effort is to attempt to solve the two orbits of each of the separate binaries with all the elements left 'free', *i.e.*, as disposable parameters in the solution. The A sub-system yields a solution in which the outer orbit is attributed a period of 11054 ± 1846 days; the solution for B (whose radial velocities are much more ragged) cannot be persuaded to converge at all, but runs away to longer and longer periods and eventually diverges. Solutions can readily be run for both components, however, if the period of the outer orbit is fixed. For both systems the resulting γ-velocities change monotonically, in opposite senses, with the period that is imposed, in the manner illustrated in the informal table on p. 381.

TABLE VII

Radial-velocity observations of HD 117078 A

Except as otherwise noted, the sources of the observations are as follows:
1987–1998 — Haute-Provence Coravel (weight 1); 2000–2011 — Cambridge Coravel (weight ⅓)

Date	MJD	Velocity km s⁻¹	Phase Outer	Phase Inner	Computed vel. Outer km s⁻¹	Computed vel. Inner km s⁻¹	(O – C) km s⁻¹
1987 Mar. 1·073	46855·106	−25·8:	0·612	0·313	−19·8	−5·9	−0·1
1988 Feb. 1·530*	47192·563	−52·9	0·635	58·118	−19·5	−32·5	−0·8
Mar. 11·177	231·210	+0·1:	·637	64·738	−19·5	+20·6	−1·0
12·068	232·101	−29·5:	·637	·891	−19·5	−9·1	−0·9
13·018	233·051	−51·6	·638	65·053	−19·5	−32·0	−0·1
13·967	234·000	−43·2	·638	·216	−19·5	−23·3	−0·4
15·001	235·034	−9·6	·638	·393	−19·5	+9·9	0·0
16·933	236·966	+2·6	·638	·724	−19·5	+22·7	−0·6
1989 Feb. 24·272†	47581·306	+6·3	0·661	124·707	−19·2	+24·9	+0·6
Mar. 27·063	612·097	−44·3	·663	129·982	−19·2	−25·1	−0·1
27·979	613·013	−51·5	·663	130·139	−19·2	−31·5	−0·8
28·949	613·983	−26·3:	·663	·305	−19·2	−7·6	+0·5
30·028	615·062	+6·4	·663	·490	−19·2	+25·4	+0·2
Apr. 27·915	643·949	−1·7	·665	135·438	−19·2	+17·9	−0·5
29·964	645·998	−7·8	·665	·789	−19·2	+11·7	−0·3
May 1·069	647·103	−43·8	·665	·978	−19·2	−24·6	−0·1
1·966	648·000	−50·4	·665	136·132	−19·2	−31·9	+0·7
2·996	649·030	−25·8	·665	·308	−19·1	−6·9	+0·3
1990 Jan. 27·069	47918·103	−8·9	0·683	182·399	−18·9	+11·1	−1·0
31·184	922·218	−51·6	·683	183·104	−18·9	−32·9	+0·2
Feb. 12·394†	934·428	−45·7:	·684	185·195	−18·9	−26·1	−0·7
15·319†	937·353	+7·9	·684	·696	−18·9	+26·3	+0·5
1991 Jan. 26·133	48282·166	−0·8	0·707	244·761	−18·6	+16·7	+1·0
27·140	283·173	−35·1	·708	244·934	−18·6	−17·4	+0·8
28·172	284·205	−51·6	·708	245·110	−18·6	−32·8	−0·3
29·203	285·236	−29·1	·708	·287	−18·6	−11·1	+0·5
30·129	286·162	+0·7	·708	·446	−18·5	+19·2	+0·1
31·055	287·088	+13·7	·708	·604	−18·5	+32·2	+0·1
Feb. 1·152	288·185	−7·9	·708	·792	−18·5	+11·0	−0·4
2·167	289·200	−41·5	·708	·966	−18·5	−22·8	−0·2
3·202	290·235	−49·8	·708	246·143	−18·5	−31·2	0·0
6·197	293·230	+11·3	·708	·656	−18·5	+30·1	−0·2
Dec. 17·201	607·234	+0·4	·729	300·443	−18·2	+18·8	−0·2
1992 Jan. 14·177	48635·210	−38·6	0·731	305·236	−18·2	−20·3	−0·1
19·212	640·245	−50·7	·731	306·098	−18·2	−33·0	+0·4
Apr. 22·122	734·154	−46·2	·738	322·184	−18·1	−27·5	−0·7
23·074	735·106	−17·4	·738	·347	−18·1	+0·9	−0·3
24·090	736·122	+9·2	·738	·521	−18·1	+28·8	−1·5
27·038	739·070	−48·1	·738	323·026	−18·1	−30·1	+0·1
30·005	742·037	+12·9	·738	·535	−18·1	+29·9	+1·1
June 24·952	797·984	−50·7	·742	333·118	−18·0	−32·5	−0·2
27·948	800·980	+13·6	·742	·631	−18·0	+31·5	+0·1
Dec. 19·217	975·248	+6·6	·754	363·482	−17·8	+24·5	−0·1
1993 Feb. 11·175	49029·206	+4·8	0·757	372·725	−17·7	+22·5	0·0
13·138	031·169	−49·9	·757	373·061	−17·7	−32·4	+0·2
19·066	037·097	−50·5	·758	374·077	−17·7	−32·8	+0·1
Mar. 20·103	066·134	−49·0	·760	379·051	−17·7	−31·8	+0·5
Apr. 23·048	100·079	−21·8	·762	384·865	−17·7	−3·9	−0·2

TABLE VII (*continued*)

Date	*MJD*	*Velocity*	*Phase*		*Computed vel.*		*(O – C)*
			Outer	*Inner*	*Outer*	*Inner*	
		km s⁻¹			*km s⁻¹*	*km s⁻¹*	*km s⁻¹*
1993 July 8·960	49176·990	−49·8	0·767	398·040	−17·6	−31·2	−1·1
11·954	179·984	+14·3	·767	·553	−17·6	+31·0	+0·8
Dec. 28·173	349·202	+13·0	·779	427·539	−17·3	+30·2	+0·2
1994 Jan. 25·357	49377·386	−12·0	0·780	432·367	−17·3	+4·8	+0·5
26·386	378·415	+13·6	·781	·543	−17·3	+30·4	+0·5
27·371	379·400	+6·8	·781	·712	−17·3	+24·4	−0·3
Feb. 16·183	399·212	−49·9	·782	436·105	−17·3	−32·9	+0·3
Apr. 30·064	472·093	+15·2	·787	448·589	−17·2	+32·2	+0·2
Aug. 7·857	571·885	+9·7	·793	465·683	−17·1	+27·7	−1·0
Dec. 11·240	697·267	−46·4	·802	487·160	−16·9	−29·9	+0·4
1995 June 6·999	49875·025	+15·6	0·814	517·609	−16·6	+32·1	+0·1
1996 Mar. 30·109	50172·132	+10·7	0·833	568·502	−16·2	+26·9	0·0
1997 Apr. 25·973	50563·992	+15·9	0·860	635·626	−15·6	+31·7	−0·2
May 1·086[‡]	569·105	+10·0	·860	636·502	−15·6	+26·8	−1·2
6·972[‡]	574·991	+11·5	·860	637·510	−15·6	+27·7	−0·6
12·965[‡]	580·984	+14·3	·861	638·536	−15·6	+30·0	−0·1
Dec. 21·187	803·203	+16·7	·876	676·601	−15·3	+32·2	−0·2
22·204	804·220	−0·8	·876	·776	−15·3	+14·1	+0·4
1998 Apr. 29·059	50932·073	+13·3	0·884	698·676	−15·1	+28·4	0·0
July 14·905	51008·918	−13·3	·889	711·839	−15·0	+1·5	+0·2
25·880	019·893	+8·0	·890	713·719	−15·0	+23·3	−0·4
27·884	021·897	−47·3	·890	714·063	−15·0	−32·4	+0·1
30·855	024·868	+16·9	·890	·572	−15·0	+31·8	+0·1
2000 Mar. 22·110	51625·114	−1·9	0·930	817·391	−14·3	+9·5	+3·0
25·097	628·101	−25·2	·931	·902	−14·3	−11·4	+0·5
26·005	629·009	−47·3	·931	818·058	−14·3	−32·2	−0·8
Apr. 10·090	644·094	+16·6	·932	820·642	−14·3	+31·0	−0·1
2001 Mar. 3·170	51971·169	+14·6	0·953	876·668	−14·2	+29·2	−0·3
9·085	977·084	+12·1	·954	877·681	−14·2	+27·9	−1·6
12·186	980·185	−37·6	·954	878·212	−14·2	−23·9	+0·5
Apr. 29·061	52028·059	−1·5	·957	886·413	−14·3	+13·6	−0·9
May 12·000	040·998	+19·1	·958	888·629	−14·3	+31·6	+1·8
2002 May 30·993[§]	52424·985	+0·3	·984	954·404	−14·7	+12·0	+3·0
June 1·997	426·989	+4·7	·984	·747	−14·7	+19·1	+0·3
10·980	435·972	−24·9	·984	956·286	−14·7	−11·2	+1·0
23·938	448·929	+12·7	·985	958·506	−14·7	+27·2	+0·2
2003 May 20·048	52779·035	−47·2	1·007	1015·051	−15·5	−31·9	+0·2
June 12·966	802·953	−46·5	·009	1019·148	−15·6	−30·9	0·0
2004 Mar. 1·173	53065·157	−48·8	1·026	1064·062	−16·4	−32·4	0·0
May 24·951	149·934	+15·7	·032	1078·584	−16·7	+32·1	+0·2
June 16·937	172·920	+12·5	·034	1082·521	−16·7	+28·8	+0·4
2005 Jan. 23·263	53393·244	−33·1	1·048	1120·262	−17·5	−15·7	+0·1
Mar. 25·115	454·096	+8·6	·052	1130·685	−17·7	+27·5	−1·2
June 8·961	529·942	+10·2	·057	1143·677	−17·9	+28·3	−0·2
2006 Apr. 5·037	53830·017	−51·3	1·077	1195·079	−18·8	−32·9	+0·4
26·016	850·996	+10·5	·079	1198·672	−18·8	+28·8	+0·6

TABLE VII (*concluded*)

Date	MJD	Velocity km s⁻¹	Phase Outer	Phase Inner	Computed vel. Outer km s⁻¹	Computed vel. Inner km s⁻¹	(O – C) km s⁻¹
2006 May 12·001	53866·981	−6·2	1·080	1201·411	−18·9	+13·2	−0·5
21·998	876·978	−51·8	·080	1203·123	−18·9	−32·3	−0·5
2007 Apr. 11·046	54201·026	+13·1	1·102	1258·631	−19·7	+31·5	+1·3
June 1·975	252·955	+8·2	·106	1267·526	−19·8	+29·2	−1·1
2008 July 3·946	54650·928	+6·9	1·132	1335·696	−20·5	+26·3	+1·2
2009 Apr. 29·997	54950·980	−54·0	1·152	1387·094	−20·9	−33·0	−0·1
May 28·953	979·937	−52·0	·154	1392·054	−21·0	−32·0	+1·0
2010 Apr. 13·045	55299·031	+3·4	1·175	1446·713	−21·3	+24·2	+0·5
17·037	303·023	−9·9	·176	1447·397	−21·3	+10·7	+0·7
18·047	304·033	+10·3	·176	·570	−21·3	+31·8	−0·2
2011 May 20·926	55701·915	+1·6	1·202	1515·725	−21·5	+22·5	+0·6
24·954	705·943	−7·7	·202	1516·415	−21·5	+14·0	−0·2

* Observed with DAO 48-inch telescope; wt. ⅓.
† Observed with ESO *Coravel*; weight 1.
‡ Observed with Cambridge *Coravel*; weight ⅓.
§ Rejected observation.

TABLE VIII

Radial-velocity observations of HD 117078 B

Except as otherwise noted, the sources of the observations are as follows:
1987–1998 — Haute-Provence Coravel; 2000–2011 — Cambridge Coravel

All velocities (except one rejected) weighted equally in the orbit

Date	MJD	Velocity km s⁻¹	Phase Outer	Phase Inner	Computed vel. Outer km s⁻¹	Computed vel. Inner km s⁻¹	(O – C) km s⁻¹
1987 Mar. 1·07	46855·04	−16·4	0·612	0·651	−18·4	+0·2	+1·8
1988 Mar. 11·18	47231·14	−18·3	0·637	2·492	−18·7	+3·1	−2·7
12·07	232·03	−14·3	·637	·497	−18·7	+3·1	+1·4
13·02	232·98	−16·5	·638	·501	−18·7	+3·0	−0·7
13·97	233·93	−18·0	·638	·506	−18·7	+2·9	−2·2
15·00	234·96	−17·0	·638	·511	−18·7	+2·8	−1·1
16·93	236·90	−17·4	·638	·520	−18·8	+2·7	−1·3
1989 Feb. 24·27*	47581·23	−12·4	0·661	4·206	−19·1	+7·1	−0·4
Mar. 27·06	612·03	−14·1	·663	·357	−19·1	+5·2	−0·2
27·98	612·94	−14·5	·663	·361	−19·1	+5·1	−0·5
28·95	613·91	−16·8	·663	·366	−19·1	+5·0	−2·7
30·03	614·99	−13·7	·663	·371	−19·1	+4·9	+0·5
Apr. 27·92	643·88	−19·2	·665	·513	−19·1	+2·8	−2·9
29·96	645·93	−16·9	·665	·523	−19·1	+2·6	−0·4
May 1·07	647·03	−16·4	·665	·528	−19·1	+2·5	+0·2
1·97	647·93	−17·0	·665	·533	−19·1	+2·5	−0·3
3·00	648·96	−17·6	·665	·538	−19·1	+2·4	−0·8
1990 Jan. 27·07	47918·03	−26·6	0·683	5·855	−19·4	−7·0	−0·2
31·18	922·15	−27·0	·683	·875	−19·4	−8·4	+0·8
Feb. 12·39*	934·36	−34·8	·684	·935	−19·4	−14·8	−0·6
15·32*	937·28	−39·0	·684	·949	−19·4	−17·3	−2·3

TABLE VIII (*continued*)

Date	*MJD*	*Velocity* *km s⁻¹*	*Phase* *Outer*	*Phase* *Inner*	*Computed vel.* *Outer* *km s⁻¹*	*Computed vel.* *Inner* *km s⁻¹*	*(O – C)* *km s⁻¹*
1991 Jan. 26·13	48282·10	−16·8	0·707	7·638	−19·8	+0·5	+2·5
27·14	283·10	−18·6	·708	·642	−19·8	+0·4	+0·8
28·17	284·13	−19·5	·708	·648	−19·8	+0·3	0·0
29·20	285·16	−18·2	·708	·653	−19·8	+0·1	+1·4
30·13	286·09	−19·1	·708	·657	−19·8	0·0	+0·6
31·06	287·02	−20·2	·708	·662	−19·8	−0·1	−0·3
Feb. 1·15	288·11	−23·3	·708	·667	−19·8	−0·2	−3·3
2·17	289·13	−20·2	·708	·672	−19·8	−0·3	−0·1
3·20	290·16	−18·9	·708	·677	−19·8	−0·4	+1·3
6·20	293·16	−18·4	·708	·692	−19·8	−0·8	+2·2
Dec. 17·20	607·16	−14·6	·729	9·229	−20·2	+6·8	−1·3
1992 Jan. 14·18	48635·14	−15·6	0·731	9·366	−20·2	+5·0	−0·4
19·21	640·18	−14·9	·731	·391	−20·2	+4·7	+0·6
Apr. 22·12	734·09	−28·6	·738	·850	−20·3	−6·7	−1·5
23·07	735·04	−28·5	·738	·855	−20·3	−7·0	−1·2
24·09	736·05	−29·0	·738	·860	−20·3	−7·3	−1·3
27·04	739·00	−28·7	·738	·874	−20·3	−8·3	−0·1
30·00	741·97	−28·1	·738	·889	−20·3	−9·5	+1·7
June 24·95	797·92	−13·5	·742	10·163	−20·4	+7·5	−0·6
27·95	800·91	−12·9	·742	·178	−20·4	+7·4	+0·1
Dec. 19·22	975·18	−20·0	·754	11·031	−20·6	+0·4	+0·3
1993 Feb. 11·18	49029·14	−14·0	0·757	11·295	−20·7	+6·0	+0·7
13·14	031·10	−15·1	·757	·305	−20·7	+5·9	−0·3
19·07	037·03	−15·7	·758	·334	−20·7	+5·5	−0·5
Mar. 20·10	066·07	−17·1	·760	·476	−20·7	+3·4	+0·2
Apr. 23·05	100·01	−19·7	·762	·642	−20·8	+0·4	+0·7
July 8·96	176·93	−29·0	·767	12·018	−20·9	−5·5	−2·6
11·95	179·92	−19·5	·767	·033	−20·9	+1·2	+0·2
Dec. 28·17	349·14	−28·9	·779	·862	−21·1	−7·4	−0·3
1994 Jan. 3·22	49355·19	−31·5	0·779	12·891	−21·1	−9·7	−0·7
25·36	377·33	−39·2	·780	·000	−21·2	−19·3	+1·3
27·37	379·34	−31·5	·781	·009	−21·2	−11·9	+1·6
Feb. 16·18	399·15	−11·9	·782	·106	−21·2	+7·6	+1·7
Apr. 30·06	472·03	−17·1	·787	·463	−21·3	+3·6	+0·6
Aug. 1·90	565·87	−35·1	·793	·923	−21·4	−13·0	−0·6
7·86	571·83	−42·6	·793	·952	−21·4	−17·8	−3·4
Dec. 11·24	697·21	−20·2	·802	14·566	−21·6	+1·9	−0·5
1995 June 7·00	49874·97	−17·7	0·814	15·436	−21·9	+4·0	+0·2
1996 Mar. 30·11	50172·08	−31·8	0·833	16·891	−22·4	−9·6	+0·2
1997 Apr. 25·97	50563·95	−28·1	0·860	18·809	−23·0	−4·6	−0·5
May 1·09†	569·07	−29·0	·860	·834	−23·0	−5·8	−0·1
6·97†	574·95	−31·0	·860	·863	−23·0	−7·5	−0·4
12·96†	580·94	−34·9	·861	·892	−23·0	−9·8	−2·1
Dec. 21·19	803·17	−46·1	·876	19·980	−23·4	−23·5	+0·8
22·21	804·19	−47·1	·876	·985	−23·4	−23·7	+0·1
1998 Apr. 29·06	50932·04	−22·5	0·884	20·611	−23·6	+1·0	+0·1
July 7·89	51001·87	−40·8	·889	·953	−23·7	−18·0	+0·9
8·90	002·88	−40·4	·889	·958	−23·7	−19·0	+2·4
12·90	006·88	−47·9	·889	·978	−23·7	−23·1	−1·1
14·91	008·89	−48·1	·889	·987	−23·7	−23·6	−0·7
24·87	018·86	−23·2	·890	21·036	−23·7	+2·1	−1·5

TABLE VIII (*concluded*)

Date	*MJD*	*Velocity* km s^{-1}	*Phase* *Outer*	*Inner*	*Computed vel.* *Outer* km s^{-1}	*Inner* km s^{-1}	*(O – C)* km s^{-1}
1998 July 25·88	51019·87	−21·0	0·890	·041	−23·7	+3·2	−0·5
27·88	021·87	−19·2	·890	·051	−23·8	+4·9	−0·4
30·86	024·85	−18·5	·890	·066	−23·8	+6·3	−1·1
2000 Mar. 22·11	51625·10	−43·0	0·930	24·004	−24·5	−16·0	−2·6
25·10	628·09	−29·8	·931	·019	−24·5	−5·2	−0·1
26·00	628·99	−27·4	·931	·023	−24·5	−2·8	−0·2
Apr. 10·09	644·09	−15·8	·932	·097	−24·5	+7·5	+1·2
2001 Mar. 3·17	51971·17	−25·0	0·953	25·699	−24·6	−1·0	+0·5
9·08	977·08	−25·9	·954	·728	−24·6	−1·7	+0·4
12·19	980·19	−23·8	·954	·743	−24·6	−2·2	+3·0
Apr. 29·06	52028·06	−46·5	·957	·977	−24·5	−23·0	+1·1
May 4·89	033·89	−40·8	·958	26·006	−24·5	−14·9	−1·4
12·00	041·00	−19·8	·958	·040	−24·5	+3·1	+1·6
2002 May 30·99‡	52425·00	−32·3	0·984	27·920	−24·1	−12·7	+4·5
June 2·00	427·01	−37·7	·984	·930	−24·1	−14·1	+0·5
18·93	443·94	−33·8	·985	28·013	−24·0	−9·1	−0·7
23·94	448·95	−21·8	·985	·038	−24·0	+2·4	−0·2
2003 May 20·05	52779·06	−21·8	1·007	29·654	−23·2	+0·1	+1·3
June 12·97	802·98	−24·1	·009	·771	−23·1	−3·1	+2·1
2004 Mar. 1·17	53065·19	−15·9	1·026	31·055	−22·2	+5·4	+0·9
May 24·95	149·97	−18·4	·032	·470	−21·9	+3·5	0·0
June 16·94	172·96	−21·2	·034	·582	−21·8	+1·6	−1·0
2005 Jan. 23·26	53393·28	−19·7	1·048	32·661	−21·0	0·0	+1·3
Mar. 25·11	454·13	−39·4	·052	·959	−20·8	−19·2	+0·6
June 8·96	529·98	−14·3	·057	33·330	−20·5	+5·5	+0·7
2006 Apr. 5·04	53830·06	−23·9	1·077	34·799	−19·5	−4·2	−0·2
26·02	851·04	−30·5	·079	·902	−19·5	−10·7	−0·3
May 12·00	867·02	−43·6	·080	·980	−19·4	−23·5	−0·7
22·00	877·02	−17·0	·080	35·029	−19·4	−0·2	+2·5
2007 Apr. 11·05	54201·07	−16·1	1·102	36·616	−18·5	+0·9	+1·5
June 1·97	252·99	−24·5	·106	·870	−18·4	−8·0	+1·9
2008 July 3·95	54650·97	−21·9	1·132	38·818	−17·6	−5·1	+0·8
2009 Apr. 30·00	54951·02	−11·0	1·152	40·287	−17·2	+6·1	+0·1
May 28·95	979·97	−13·0	·154	·429	−17·1	+4·1	0·0
2010 Apr. 13·04	55299·06	−40·2	1·175	41·991	−16·8	−23·0	−0·4
17·04	303·06	−25·6	·176	42·011	−16·8	−10·8	+2·0
18·05	304·07	−24·3	·176	·016	−16·8	−7·2	−0·3
2011 May 20·93	55701·94	−36·3	1·202	43·964	−16·5	−20·3	+0·5
24·95	705·96	−38·9	·202	·983	−16·5	−23·7	+1·3

* Observed with ESO *Coravel*.
† Observed with Cambridge *Coravel*.
‡ Rejected observation.

Period (days)		11000	13000	15000	17000	19000
γ-velocity (km s^{-1})	A	−18·13	−18·60	−19·01	−19·37	−19·68
	B	−19·98	−19·45	−19·02	−18·65	−18·32

The sum of the squares of the residuals increases just marginally in the same sense as the period for A and in the reverse sense for B, as would be expected from what has been said concerning the efforts to solve the orbits with the period 'free'. The variations are not statistically significant and do not constrain the choice of period within the range just tabulated. It is seen that the γ-velocities practically coincide for the period of 15 000 days, which makes that an attractive choice for adoption as the period. The standard deviation of the γ-velocities of the two stars, for that period, are 0·13 and 0·24 km s^{-1}, so the s.d. of their difference is 0·27 km s^{-1} or about one-third of the discrepancy between the two γ-velocities at periods differing from 15 000 by ±2000 days. It is readily acknowledged, however, that it might be optimistic to deduce from that that the 1-σ uncertainty of the outer period is only about 700 days.

Once the outer period is fixed, the elements of both orbits are readily calculated for both observed components. The next problem is that the elements of the outer orbit, calculated independently for each of the sub-systems, are inevitably not identical, although they do bear a family resemblance to one another. It is necessary — if a specific solution is to be given — to adopt one that does justice to both of them to the extent that that is possible. The two solutions are in fact encouragingly similar, but that of A is naturally better determined than that of B, whose underlying radial velocities are so much worse owing to its niggardly 'dip' in the observed traces. A comparison of the elements of the outer orbit follows, with the last column showing the corresponding quantities that are adopted here as the result of a subjective weighted-averaging process.

Element	*A*	*B*	*Adopted mean*
P (days)	15000 (fixed)	15000 (fixed)	15000
T (MJD)	52555 ± 173	52895 ± 278	52670
Γ (km s^{-1})	−19·01 ± 0·13	−19·02 ± 0·24	−19·01
K (km s^{-1})	3·80 ± 0·17	4·20 ± 0·29	
e	0·376 ± 0·034	0·371 ± 0·056	0·374
ω (degrees)	37 ± 5	56* ± 9	42

*Inverted from the computed value of 236°, for comparability with ω_A

The values of the velocity amplitudes, K, are of course independent of one another since they compare the total masses of the independent sub-systems A and B, and give a value, of sorts, for the relative masses of those sub-systems. It appears that A is the more massive by about 10%, but the difference from unity is little more than one standard deviation. All the same, A is evidently the brighter, and so by hypothesis the more massive, of the two observed stars, each of which is probably dominant in its own sub-system since the companions are invisible in radial-velocity traces, so the tentative indication that the A system has the superior mass is not unwelcome. The adopted outer orbit is shown in Fig. 10.

As a final step in the calculations, fresh solutions are performed for both of the sub-systems, with the elements of the outer orbit held fixed at the values adopted just above. The solutions are given here and illustrated by Figs. 11 and 12.

Element		*Outer Orbit*	*Inner Orbit (A)*	*Inner Orbit (B)*
P	(days)	15000	5·837891* ± 0·000008	204·256 ± 0·010
T_0	(MJD)		49454·6099 ± 0·0029	
T	(MJD)	52670	49456·98 ± 0·18	50398·69 ± 0·23
Γ	(km s^{-1})	−19·01		
K_1	(km s^{-1})	3·80 ± 0·17	32·60 ± 0·07	15·73 ± 0·24
K_2	(km s^{-1})	4·20 ± 0·29		
q	($= m_1/m_2$)	1·10 ± 0·09		
e		0·374	0·0144 ± 0·0027	0·727 ± 0·007
ω	(degrees)	42	146 ± 11	225·5 ± 1·3
$a_1 \sin i$	(Gm)	727	2·617 ± 0·006	30·3 ± 0·6
$a_2 \sin i$	(Gm)	803		
$f(m_1)$	($M_\odot$)	0·068	0·02100 ± 0·00014	0·0267 ± 0·0015
$f(m_2)$	($M_\odot$)	0·092		
$m_1 \sin^3 i$	($M_\odot$)	0·33		
$m_2 \sin^3 i$	($M_\odot$)	0·30		
R.m.s. residual (unit weight) (km s^{-1})			0·50	1·27

*The true period (in the rest-frame of the system) is 5·838262 ± 0·000008 days; the difference from the observed period is 45 standard deviations.

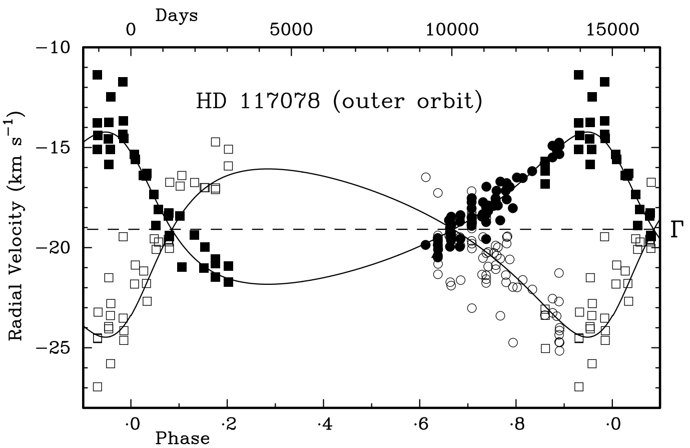

FIG. 10

Preliminary outer orbit for HD 117078. The text describes how orbital solutions were first attempted for the two stars separately, and the period of the outer orbit was then fixed at 15000 days. Next, a second pair of solutions produced the other elements of the outer orbit, sets of values which were averaged and then imposed on the solutions as a third pair to produce the final elements for the inner orbits. This figure has been produced by superimposing the plots of the outer orbit obtained for each of the components separately. Although the elements of the outer orbit, and thereby the forms of the velocity curves, were imposed on both solutions, the distribution of the plotted points depends on those solutions, each of which optimized the inner orbit and plotted the points for its own component. The coding of the points is the same as in Fig. 5; there is in principle one DAO observation of the primary, but its symbol is hidden.

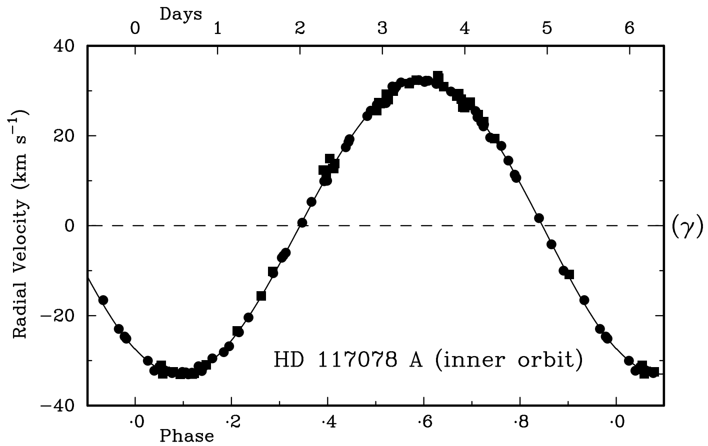

FIG. 11

The inner orbit of HD 117078 A. It has a very small but definitely non-zero eccentricity.

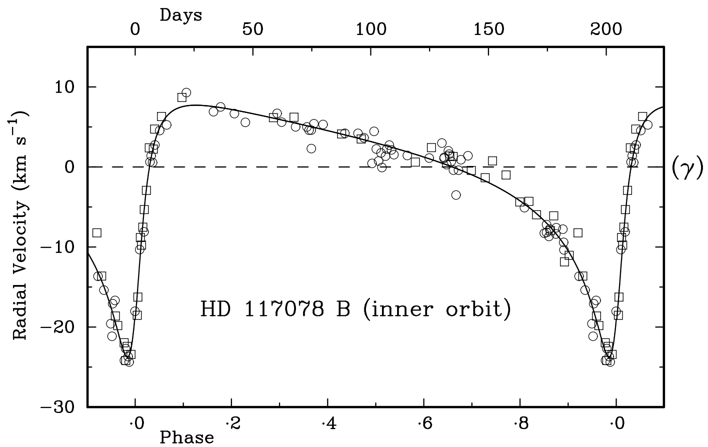

FIG. 12

The inner orbit of HD 117078 B.

It will be noticed that the A sub-system has a very small, but quite definitely non-zero, eccentricity — it is more than five times its own standard deviation. That is easily understood, as was pointed out by Mazeh & Shaman[40] quite a long time ago, as being the hallmark of a perturbing body, whose existence in this case is actually demonstrated, in the shape of B. The reality of the eccentricity is reinforced by a comparison of the residuals given by the orbital solution shown for A in the above table and a corresponding one with zero eccentricity forced upon it. The sums of the squares of the weighted residuals are 25·80 and 33·86

$(\text{km s}^{-1})^2$ in the respective cases. The former number represents 99 degrees of freedom, which cost about 0·26 $(\text{km s}^{-1})^2$ each, whereas the difference between the two numbers, 8·06, represents just two extra degrees (e and ω), which evidently cost 4·03 each. Thus the variance ratio, $F_{2,99}$, is 4·03/0·26, about 15·5; the 0·1% point is reached already at $F_{2,99} \sim 7{\cdot}42$, and by 15·5 the probability is down to about one in a million[41]. An effect of the smallness of the eccentricity is to make the epoch of periastron very uncertain. The uncertainty of e, at nearly one part in five, makes the longitude, ω, of periastron uncertain by nearly a fifth of a radian (11°), and that in turn means that the epoch T is uncertain by nearly a fifth of $P/2\pi$ (0·18 days) — quantities that are faithfully borne out by the computed solution. The quantity T_0 has therefore been added to the solution for the A inner orbit, to represent just the uncertainty of the positioning of the velocity curve of the orbit in the time coordinate. In this case it is about sixty times more accurate than T.

There can be no doubt, from the shortness of the period of A, that in HD 117078 we are dealing with a set of main-sequence stars. The two observed components have 'dip' signatures in radial-velocity traces bearing a mean ratio of 3·0 (OHP) or 2·6 (Cambridge) to one another; a mean of 2·8 expressed as a difference in stellar magnitudes is about $1^{\text{m}}{\cdot}1$, and multiplication by the factor of 1·15 suggested by Griffin & Suchkov[1] to obtain the difference in V magnitudes gives nearly $1^{\text{m}}{\cdot}3$. A pair of main-sequence stars that differ by that amount and would in combination give the observed colour index of $(B-V) = 0^{\text{m}}{\cdot}57$ would be of types F8 and G6; the combined colour index is that of a G0 V star. With their individual absolute magnitudes of $4^{\text{m}}{\cdot}0$ and $5^{\text{m}}{\cdot}3$ combined in proportion to the fluxes, the stars would give the appearance of one whose M_V is about $4^{\text{m}}{\cdot}3$, very close to the $4^{\text{m}}{\cdot}4$ determined observationally by Griffin & Suchkov[1]. The real absolute magnitude of the combination, however, is $3^{\text{m}}{\cdot}7$ — six-tenths of a magnitude brighter — so the putative distance is greater than was tabled by those authors on the basis of the object being single. The implied distance modulus is $5^{\text{m}}{\cdot}9$, corresponding to a distance of 150 pc or a parallax of $0''{\cdot}066$.

The stars could be expected to have masses near 1·2 and 0·9 $M_\odot$ — more disparate than the q value found by the outer orbit would suggest, but it is to be remembered that *that* value includes contributions from the unseen companions, about which we know almost nothing, of both A and B. If the two observed stars have the masses attributed to them here, the mass functions of their inner orbits show that their companions must both be at least about 0·36 $M_\odot$. Then the sub-systems' masses need to be multiplied by a factor that cannot be more (but may be expected to be not much less) than about 0·23 to obtain values near to those given by the orbit, so *that* is the quantity represented by $\sin^3 i$. Therefore $\sin i$ can be at most about $\sqrt[3]{0{\cdot}23}$, about 0·61, so the inclination of the outer orbit can hardly be more than about 38°.

The mean projected rotational velocity given for A by OHP radial-velocity traces is $8{\cdot}2 \pm 0{\cdot}3$ km s^{-1}; the Cambridge traces give $8{\cdot}8 \pm 0{\cdot}5$ km s^{-1}. On the assumption that the rotation of star A is synchronized with the 5·84-day orbital period, if the star had a projected radius of 1 $R_\odot$ the observed rotational velocity would be 8·7 km s^{-1}, which is seen to be just about the observed figure. The actual radius of the F8 star A must be expected to be somewhat more than 1 $R_\odot$, perhaps 1·15 $R_\odot$, which would imply an inclination of the rotational axis of about arcsin(0·85), close to 60°. The signature of star B in radial-velocity traces is so weak that its projected rotational velocity cannot be determined to useful accuracy, but it is pretty surely smaller than that of A and is not certainly distinguishable from zero.

The sum of the $a \sin i$ values of the outer orbit amounts to just over 10 AU, and by inserting the value of $\sin i$ deduced above we find the actual semi-major axis of the orbit to be not less than about 16 AU. Such a separation seen at the 150-pc distance we derived above from photometric considerations would subtend just over $0''\cdot 1$, which was the result of the one measurement that has so far been made of the angular separation of the components on the sky. The similarity is encouraging but to some extent fortuitous, because the phase in the outer orbit at the time of observation was such that the actual separation of the components would have been somewhat more than the semi-axis, but then the observed value would have been reduced by projection. It is, however, clear that the optical resolution refers to the two objects for which single-lined orbits have been determined here. All that is needed is for systematic observations to continue, interferometrically for about 40 years and spectroscopically for somewhat less, for the outer orbit to be fully determined in three dimensions.

Acknowledgement

It is a pleasure to thank Dr. Jean-Claude Mermilliod for providing the OHP radial velocities of HD 110195. It was the existence of those velocities that galvanized renewed interest in the likelihood that that system is triple, and constituted the data to demonstrate the fact.

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NOTES FROM OBSERVATORIES

CCD OBSERVATIONS OF 11 VARIABLES CLASSIFIED IN THE *GCVS* AS RR LYRAE–TYPE STARS WITHOUT LIGHT-CURVE ELEMENTS

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We took a total of 1175 B, V, and I_C-band CCD frames for 11 *GCVS* RR Lyr-type variables, for which only the coordinates were known. Observations were made with the 76-cm telescope of the South African Astronomical Observatory. We confirm the variability types, determine the periods, and build the light curves for five objects: BL Aps ($P = 0^{d}\cdot598$), GR Pav ($P = 0^{d}\cdot583$), HI Pav ($P = 0^{d}\cdot643$), HZ Pav ($P = 0^{d}\cdot803$), and IR Pav ($P = 0^{d}\cdot561$). The objects SY Aps ($P = 0^{d}\cdot279$) and IV Pav ($P = 0^{d}\cdot408$) proved to be W UMa-type eclipsing variables; TW Aps ($P = 0^{d}\cdot149$), an SX Phe-type variable; TT Aps ($P = 482^{d}$), an SRD-type semi-regular variable; and SW Aps and GI Pav, constant stars.

Introduction

Eleven variable stars discovered by Swope[1] (SW Aps, SY Aps, TT Aps, and TW Aps) and Hoffmeister[2] (BL Aps, GI Pav, GR Pav, HI Pav, HZ Pav, IR Pav, and IV Pav) were classified as RR Lyrae-type variables, although no periods could be determined for them. We included these stars in our programme of CCD observations of southern-hemisphere pulsating variables and here we present our results.

Observations

We performed our CCD observations in 2011 May (over the JD 2455700–2455704 time interval) with the 76-cm telescope of the South African Astronomical Observatory (SAAO) in South Africa using an SBIG CCD ST-10XME camera equipped with BVI_C filters of the Kron–Cousins system[3].

We used the same reduction procedure as in our previous paper[4] and inferred the following transformation coefficients ζ and μ from extra-atmospheric magnitudes b, v, and i into magnitudes of the BVI_C system of Kron and Cousins[3]: $\zeta_B = 0{\cdot}0211 \pm 0{\cdot}0008$, $\zeta_{BV} = -0{\cdot}0665 \pm 0{\cdot}0010$, $\zeta_{VI} = -0{\cdot}0615 \pm 0{\cdot}0011$, and $\zeta_I = 0{\cdot}0144 \pm 0{\cdot}0008$, which we employed to determine the zero points μ for each night by formulae (1) and (2) from ref. 4.

As a result of the reduction of the data for all photometric nights, we obtained the catalogue of positions and magnitudes of all objects on the best CCD frames. We identified constant stars from this catalogue and used them as comparison stars for the differential photometry of all stars on all CCD frames including those taken on non-photometric nights, for which we made atmospheric corrections based on the average extinction coefficients: $\alpha_B = 0{\cdot}262$, $\alpha_V = 0{\cdot}140$, and $\alpha_I = 0{\cdot}063$.

Results

We obtained a total of 1175 CCD frames in five observing nights. The first observations showed SW Aps ($V = 12{\cdot}67$, $B-V = 1{\cdot}64$, $V-I_C = 1{\cdot}83$), TT Aps ($V = 12{\cdot}75$, $B-V = 1{\cdot}72$, $V-I_C = 3{\cdot}68$), and GI Pav ($V = 13{\cdot}92$, $B-V = 1{\cdot}08$, $V-I_C = 1{\cdot}24$) to be too red for RR Lyrae–type variables and we therefore dropped them from the programme of further observations. We found all three stars in the *ASAS-3* catalogue[5], where TT Aps is classified as a "MISC" type variable with a period of about 482^d (Fig.1), whereas SW Aps ($V = 12{\cdot}65 \pm 0{\cdot}06$) and GI Pav ($V = 13{\cdot}86 \pm 0{\cdot}16$) exhibit no light variations within the errors. Given its colour index, TT Aps can be classified as a semi-regular SRD-type variable.

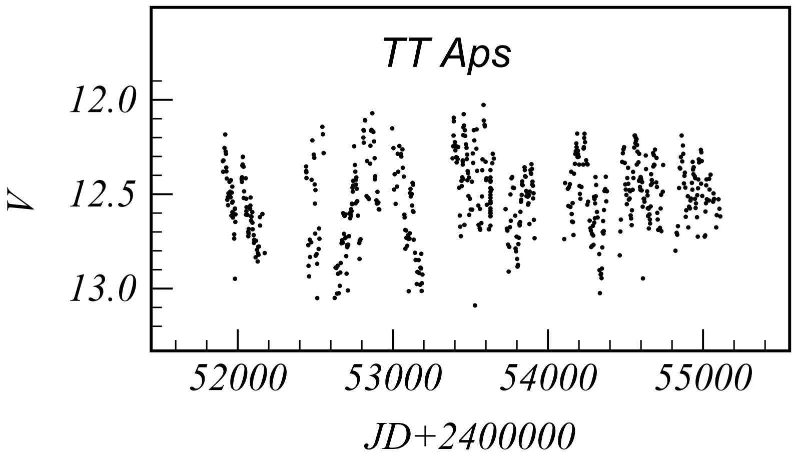

Fig. 1

Time-series data for TT Aps covering approximately 3000 days. A period of about 482 days has been suggested based upon the *ASAS-3* data by Pojmanski[5].

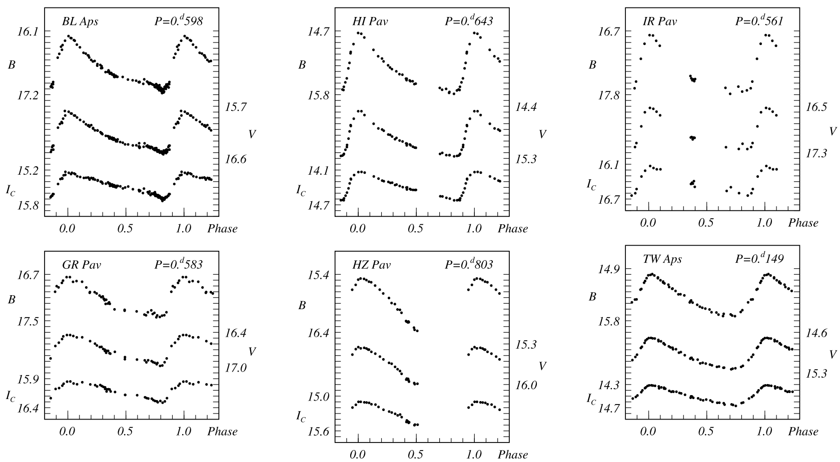

FIG. 2

Light-curves of five RR Lyrae-type variables (BL Aps, GR Pav, HI Pav, HZ Pav, and IR Pav) and the SX Phe-type variable TW Aps.

The remaining eight stars proved to be periodic variables and we give their observations in a table which is available in electronic form at the CDS *via* anonymous ftp to cdarc.u-strasbg.fr (130.79.128.5). Five stars — BL Aps ($P = 0^{d}{\cdot}598$), GR Pav ($P = 0^{d}{\cdot}583$), HI Pav ($P = 0^{d}{\cdot}643$), HZ Pav ($P = 0^{d}{\cdot}803$), and IR Pav ($P = 0^{d}{\cdot}561$) — are RR Lyrae-type variables, whereas TW Aps ($P = 0^{d}{\cdot}149$) is an SX Phe-type variable. We show their light-curves in Fig. 2, with the elements listed in Table I, which we determined based on observations presented in the on–line table.

SY Aps ($P = 0^{d}{\cdot}279$) and IV Pav ($P = 0^{d}{\cdot}408$) are rather bright stars and we found observations of them in the *ASAS-3* catalogue, where they are classified as contact eclipsing variables (of the EC type). Our observations confirm this classification, which corresponds to the *GCVS* type EW[6]. Fig. 3 shows the light curves of SY Aps and IV Pav based on our observations and the data from the *ASAS-3* catalogue[5]. We analysed the available observations using the Hertzsprung method[7], with a software version developed by one of us[8].

TABLE I

Light-curve elements of eight variables

Star	*Type of variability*	*Reference epoch*			*Period days*		
BL Aps	RRab	2455704·327	±	0·001	0·5980	±	0·0001
GR Pav	RRab	2455703·393	±	0·001	0·5830	±	0·0001
HI Pav	RRab	2455702·659	±	0·001	0·6426	±	0·0001
HZ Pav	RRab	2455704·013	±	0·001	0·8026	±	0·0001
IR Pav	RRab	2455703·312	±	0·001	0·5614	±	0·0001
TW Aps	SX Phe	2455703·487	±	0·001	0·1493	±	0·0001
SY Aps	EW	2453905·3714	±	0·0003	0·27890798	±	0·00000007
IV Pav	EW	2453871·6019	±	0·0002	0·40779295	±	0·00000007

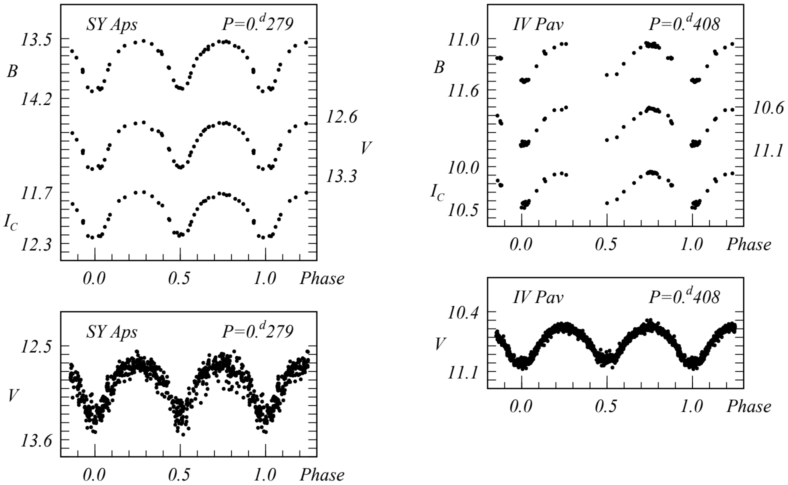

FIG. 3

Light curves of the eclipsing variables SY Aps and IV Pav according to the data from our observations (upper panel) and *ASAS-3* catalogue (lower panel).

The computed times of minimum light are listed in Table II, where columns 1 and 2 give the times of minimum light and its error, respectively; column 3, the type of observations used; columns 4 and 5, the epoch number *E* and O – C

TABLE II

Times of minimum light of SY Aps and IV Pav

Max HJD	*Uncertainty*	*Filter*	*E*	*O – C*	*N*	*Reference*
			SY Aps			
2452109·4832	0·0008	V	−6439	0·0003	100	Pojmanski[5]
2452773·5628	0·0008	V	−4058	0·0000	100	Pojmanski[5]
2453333·6106	0·0008	V	−2050	0·0005	100	Pojmanski[5]
2453896·4456	0·0008	V	−32	−0·0008	100	Pojmanski[5]
2454435·8531	0·0008	V	1902	−0·0013	100	Pojmanski[5]
2454858·9583	0·0009	V	3419	0·0005	98	Pojmanski[5]
2455700·9816	0·0002	V	6438	0·0006	45	This paper
			IV Pav			
2452041·0197	0·0005	V	−4489	0·0003	100	Pojmanski[5]
2452192·7178	0·0005	V	−4117	−0·0005	100	Pojmanski[5]
2452535·2637	0·0006	V	−3277	−0·0007	100	Pojmanski[5]
2452799·9227	0·0006	V	−2628	0·0007	100	Pojmanski[5]
2452989·5454	0·0005	V	−2163	−0·0003	100	Pojmanski[5]
2453435·6718	0·0005	V	−1069	0·0005	100	Pojmanski[5]
2453669·7449	0·0007	V	−495	0·0005	100	Pojmanski[5]
2454054·7022	0·0005	V	449	0·0013	100	Pojmanski[5]
2454353·6127	0·0006	V	1182	−0·0004	100	Pojmanski[5]
2454593·8035	0·0006	V	1771	0·0003	100	Pojmanski[5]
2454777·7175	0·0005	V	2222	−0·0003	100	Pojmanski[5]
2455051·7535	0·0006	V	2894	−0·0012	111	Pojmanski[5]
2455702·5927	0·0008	V	4490	0·0005	43	This paper

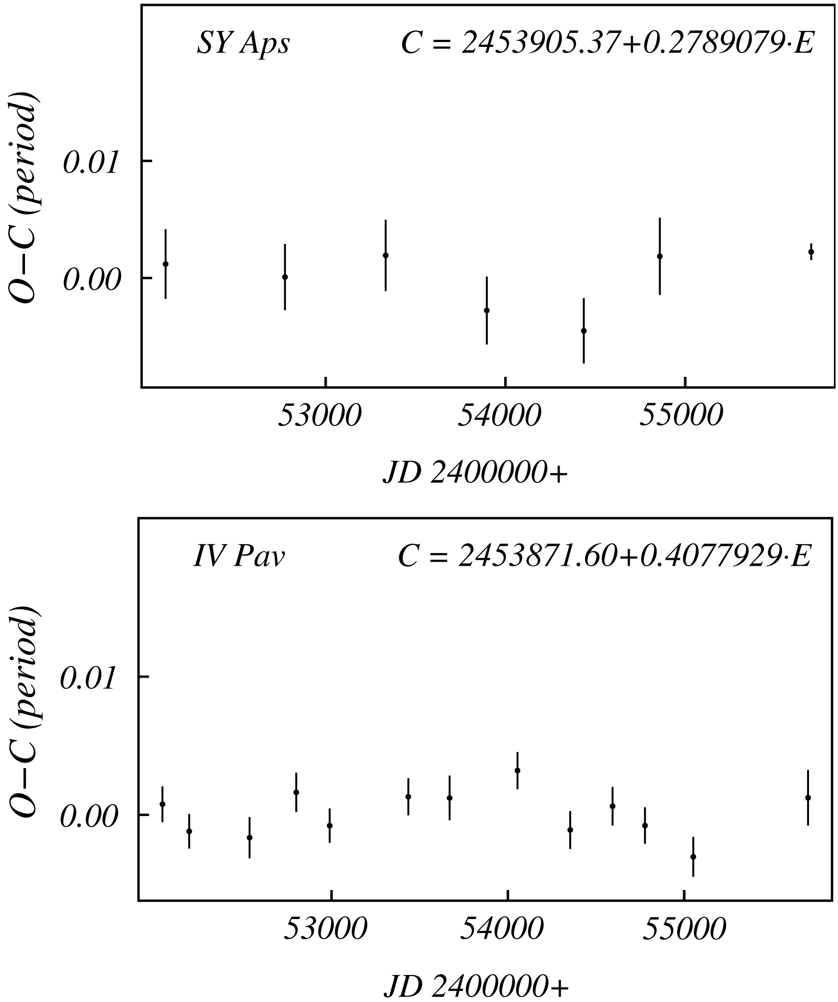

FIG. 4
O – C diagrams for SY Aps and IV Pav.

residual, respectively, and columns 6 and 7, the number *N* of observations and the source of data, respectively. We show the data from Table II in O – C diagrams (Fig. 4) as circles with vertical O – C error bars. The O – C residuals in Fig. 4 are given in fractions of the period. The data in Table II allowed us to refine the light-curve elements included in Table I.

The wave-like oscillations of O – C residuals in Fig. 4, if real, can be explained by the light-time effect, *i.e.*, by the presence of a third body.

Acknowledgments

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CORRESPONDENCE

To the Editors of 'The Observatory'

Factor L of the Drake Equation

The recent catastrophic events in Japan, at the Fukushima Dai-ichi nuclear plant, remind us again of the frailty of our contemporary civilization. These events, in the context of natural catastrophes and environmental disasters, bring to mind factor *L* of the Drake equation: the longevity of a technological civilization.

In 1961 — when the Drake equation was derived — and also now, the estimate for factor *L*, the duration of a technological civilization, is scientifically almost impossible to answer. Considering what we do know, the estimate depends upon the stability of a civilization and the risks of advancing technologies, nuclear and chemical, triggering self-destruction. The capacity for our civilization to destroy itself, since 1945, has been addressed by prominent people with a sense of urgency, among whom are Russell, Einstein, Schweitzer, Huxley, von Weizsäcker, and Sagan. More recently, in 2003, Lord Rees[1] highlighted the downside of advancing technologies, as well as the hazards of cometary and asteroid impacts with the Earth.

If this is the cosmic principle of life, upon which the Drake equation rests, natural or other catastrophic destruction, combined with fallibility, imply elsewhere sapient beings with technologies are not commonplace or could be extinct. The chances of detecting transmissions from such technologies may largely depend upon existent artificial signals originating from now-defunct technological civilizations among the stars. The scope for this depends upon the evolution of sapient life-forms and the number, N, of communicating technological civilizations in the Galaxy. According to the Drake equation, the following factors determine N: $R_\star$, the rate of star formation in the Milky Way galaxy; f_p, the fraction of stars with planets; n_e, the number of habitable planets; f_l, the fraction of planets on which life appears; f_i, the number of habitable planets on which occur advancing technologies; f_c, the number of technological civilizations communicating with other star systems; and L, the longevity of a technological civilization.

As to the possible occurrences of planetary civilizations elsewhere in the Galaxy, too many uncertainties exist to speculate about the chemistry, characteristics, habitat, and heritage of such civilizations. Chauvinisms as to the type of habitat seem to have been influenced by H. G. Wells[2]: "… we discover another planet, much the same size as ours to judge by the scale of its inhabitants, circulating, we may certainly assume, round a sun like that in our skies, a planet bearing life and being slowly subjugated, even as our own is being subjugated, by intelligent life which has evolved under almost exactly parallel conditions to those of our own evolution."

Wells, trained as a biologist under T. H. Huxley, advanced this as a mix of fiction and non-fiction around the time he predicted a calamitous future for our species, science, and civilization — that humankind would either destroy itself or become extinct because it could not adapt to changing environments.

Mindful of this and the lessons we learn from human-made environmental effects on the climate, nuclear accidents, and from other forms of hazardous technologies, the potential to self-annihilation or extinction continues to cling. Consequently, if our species is to survive, we need to assess and adapt wisely.

Yours faithfully,
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The Power of Stars

In his review in this *Magazine* (**131**, 169, 2011) of Bryan Penprase's *The Power of Stars: How Celestial Observations Have Shaped Civilization*, David Hughes recognized three answers to the principal question behind the book:

"How, throughout history, have humans responded to the sky?" One answer, Hughes points out, "fits in with today's ... view of science," namely innate human curiosity and a genuine desire to understand the natural world around us. Another answer Hughes recognizes is quite simply that the sky is useful: heavenly bodies have been used since the dawn of civilization as "compasses, clocks, and calendar markers." Hughes's "more embarrassing" third answer "is tied up with the world of astrology," that "excellent foppery of the world," as Edmond of Shakespeare's *King Lear* saw it, "that when we are sick in fortune ... we make guilty our disasters the sun, the moon, and stars." Of course astrology, a kind of sky myth offering us a comforting sense of familiarity and 'at homeness' in the Universe, can be regarded as a surrogate science (today, we would say pseudoscience) of developing cultures.

The purpose of my correspondence is to point out that such a three-fold response to the sky — mythical, practical, and intellectual — was suggested over a century ago by the British astronomer Sir J. Norman Lockyer in his 1894 pioneering book on archaeoastronomy, *The Dawn of Astronomy*. At the very beginning of his book, Lockyer proposes that "man's earliest observations of the heavenly bodies ... may very fairly be divided into three perfectly distinct stages." Lockyer labels the first (mythical) stage "wonder and worship", and recognized as a second (practical) stage "the need of applying the observation of celestial phenomena in ... the direction of utility — such as the formation of a calendar and the foundation of years and months....". "Only more recently," Lockyer writes, "were any observations made of any celestial object for the mere purpose of getting knowledge ... . This abstract inquiry [the intellectual reason for doing science] is now practically the only source of interest in astronomy to us."

At about the same time that Lockyer's book appeared, a somewhat similar scenario for the historical roots of science was proposed by the British anthropologist Sir James George Frazer in a rather weighty tome titled *The Golden Bough* published in 1890. Frazer also identified three ways of "dealing with nature": the magical, to control nature by tapping into occult forces; the religious, to petition nature through prayer to a supernatural being; and the scientific, to understand nature by careful observation, experimentation, and theorizing. In comparing Lockyer's and Frazer's three developmental stages, the following pairings can be made: Lockyer's mythical is similar to Frazer's religious; the practical stage and magic are related in belonging to the craft tradition and the "art of doing things" (magic itself was a means to practical ends); and, perhaps most obviously, Lockyer's intellectual stage can be identified with Frazer's science, although understanding the occult forces operative in nature was, particularly during the Renaissance, every bit as intellectual as what we today recognize as science.

It would seem that Professors Penprase and Hughes are not alone in their recognition of humanity's "three-fold way" of responding to the sky throughout history.

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Who discovered the expansion of the Universe?

Does it really matter who discovered the expansion of the Universe? Great discoveries are anyway never made single-handedly. This is a valid attitude. However, those interested in the evolution of our scientific culture are eager to know the intricate patterns that lead to new insights. As the expanding Universe is one of the most important discoveries ever made, it is not astonishing that the question of how it happened is still widely discussed.

The debate on this topic has flared up again owing to an article in *Nature*'s on-line 'naturenews', where Eugenie Reich highlighted two contributions by Sidney van den Bergh[1] and David Block[2]. Their effect was to reanimate the discussion as to whether Hubble or Lemaître discovered the expanding Universe, or whether it was simply a nearly predictable outcome of the normal scientific activity of those days. We have investigated this question in our book *Discovering the Expanding Universe*[3], where we reconstructed the discovery from original documents. The two authors just mentioned agree with our assessment of the events.

The purpose of the present contribution is twofold. (*a*) We point out that the Lemaître/Hubble discovery stories exemplify two different paths of scientific progress. The Hubble-myth is that of a chance discovery, the story of Lemaître is a deliberate search by an individual scientist for the solution of a long-standing problem. (*b*) We also feel it highly desirable to summarize the main facts for those too busy to do their own research, or to collect all the publications dedicated to particular details, or to read our whole book[3] with all its details.

During the whole story we should not forget that neither Lemaître nor Hubble worked in isolation, and that a discovery also feeds on direct and indirect contributions from others. In this case the discovery was imbedded in the many contributions to General Relativity, the search for an adequate interpretation of the enigmatic large nebular redshifts, and the challenge of measuring distances to extragalactic objects.

The facts

(*1*) In 1917 Einstein found from his fundamental equations of General Relativity a static model of the Universe. More than two thousand years of astronomical observations showed the Universe to be stable and practically immutable in space as well as in time. Thus, very naturally, Einstein was looking for a static world. To achieve his aim he introduced the cosmological constant Λ.

(*2*) In 1917, a few months after Einstein, de Sitter found another solution which seemed to contain the explanation of Slipher's then already well-known nebular redshifts, which he had been observing at Flagstaff since 1912. De Sitter's universe also contained the cosmological constant Λ, but no other energy terms.

(*3*) In 1922 Friedmann found that Einstein's equations allowed a dynamic universe. He did not connect this finding to astronomical observations, and he did not spot the flaw in de Sitter's model. Except for Einstein, who did not think that dynamic solutions were physically relevant, no one took notice of Friedmann.

(*4*) There were other theoreticians, who in the 1920s derived dynamical universes from the Einstein equations, but none of them linked their results to observations, nor did they propose an expanding universe. Lanczos in 1922 derived a formal solution for a spatially closed dynamical universe. In the same paper, he commented on publications by Hermann Weyl of 1918 and 1919, which discussed redshifts in de Sitter's model. These papers attest to the confusion generated by de Sitter's empty universe. And as the mathematical tools had not yet been as developed as they are today, interpretations often took intricate paths. An important contribution came from Weyl with his concept of a "causally connected world". It is also instructive to follow the Einstein–Weyl postcard exchange about the cosmological constant. More details about this topic and other players in the game can be found in our book[3].

(*5*) During the early twenties, not only the theoreticians but also the observers tried to make sense of de Sitter's universe, and to determine its radius of curvature. In addition, the community still debated whether nebulae were extragalactic or not. In the course of these investigations Carl Wirtz found in 1924 a relationship for spiral nebulae between their apparent photographic diameters and the radial velocities, and in the same year Knut Lundmark published in *Monthly Notices* the first distance–velocity diagram, distances being given in units of the distance to the Andromeda Nebula. In the discussion on the nature of spiral nebulae, Öpik, by an ingenious method, had already in 1922 found a distance of 450 kpc to Andromeda, much closer to the real value than Hubble's later distance of 285 kpc. But it was Hubble's paper, read at the 1925 January 1 meeting of the American Astronomical Society, which cleared the way for extragalactic nebulae (now called galaxies) as building blocks of the Universe: the 'island universes' hypothesized by Kant and Laplace were accepted as reality.

(*6*) In 1927 Lemaître[4] criticized de Sitter's deadly sin for all the believers in the Copernican hypothesis: he violated the principle of homogeneity by treating the observer in a preferential way. He then discovered a set of dynamical solutions to Einstein's fundamental equations. From the theory of relativity he derived the linear velocity–distance relationship $v = \mathrm{H} \times d$ (now called the Hubble relation).

Lemaître then connected his solution to observations. To derive the numerical value of H, Lemaître employed Hubble's 1926 distances to extragalactic nebulae and Slipher's redshifts. Depending on his choice of observations he arrived at either 625 or 575 km s^{-1} Mpc^{-1} (compared to Hubble's 500 km s^{-1} Mpc^{-1} in 1929). He was satisfied that the observations did not contradict his theoretical conclusions: the Universe itself is expanding. But he was also well aware that there was an enormous scatter in the observations, and that further observations would have to confirm the linear relationship. As his article clearly shows, Lemaître was fully aware of the significance of his discovery. It is all the more astonishing that he did not try to place it in one of the prestigious astronomical journals, but published it in French in the *Annales de la Société scientifique de Bruxelles*.

(*7*) In 1929 Hubble[5] set out to study the motion of the Sun against the background of the extragalactic nebulae, for which he tabulated, as others had done before, distances and velocities of extragalactic nebulae:

$$v = rK + X \cos \alpha \cos \delta + Y \sin \alpha \cos \delta + Z \sin \delta.$$

In the course of that investigation he found with his improved distances that "The data in the table indicate a linear correlation between distances and velocities, whether the latter are used directly or corrected for solar motion, according to the older solutions". Having realized that fact, he turned away from the solar problem, concentrating on the linear relationship. Depending on how he grouped the galaxies, he found $K = 473$, 513, or 530 km s^{-1} Mpc^{-1}, but opted for $K = 500$ km s^{-1} Mpc^{-1} as his favoured value. To derive the numerical value of K, Hubble worked with his own distances. For the redshifts he mainly took those of Slipher, as tabulated in Eddington's 1923 *The Mathematical Theory of Relativity* (second edition 1924), without, however, giving references. Hubble refrained from interpreting his observational discovery, and concluded, "The outstanding feature, however, is the possibility that the velocity–distance relation may represent the de Sitter effect, and hence that numerical data may be introduced into discussions of the general curvature of space". In a later letter to de Sitter, Hubble wrote that he would leave the interpretation of his observations to those "competent to discuss the matter with authority". In none of the seven pages of Hubble's paper is there a single word about an expanding universe; actually Hubble never believed in such a thing. (This fact is also emphasized by Sandage in the foreword to our book.) Hubble's observations confirmed Lemaître's predictions. In our book we also show how they re-ignited the cosmological debate, as exemplified by the crucial de Sitter–Eddington discussion[6] at the RAS meeting on 1930 January 10.

(*8*) Lemaître's article of 1927 was translated into English and published in 1931 in *Monthly Notices*[7]. However, there was an historically momentous omission. His derivation of the numerical value of H was cut out by a deliberate act. A letter to Lemaître by Dr. Smart, secretary of the RAS, recently discovered in the Lemaître archives of Louvain, provides additional evidence for this fact. Dr. Smart asks Lemaître on 1931 February 17 whether he would allow his 1927 article to be reprinted in *Monthly Notices*: "It has been felt that it has not circulated as widely — or isn't as well known as its importance warrants — especially in English-speaking countries". He further suggested that the translation (the letter does not specify who should do the translation) should be identical to the original up to § 72, which corresponds to Lemaître's formula 23 (personal communication from Mme. Moens-Haulotte, Archives Lemaître, Louvain, to H.N.) and is indeed the place where Lemaître starts to talk about Slipher's radial velocities (which Lemaître got from Strömberg) and Hubble's distances. Smart suggests that the remainder should be re-written. It is still an unsolved puzzle as to why this was suggested, and who did the re-writing which consisted essentially of cutting out the derivation of the 'Hubble constant'. Thus the public, who read the English version, were left with the impression that Hubble had been the first to derive H.

(*9*) It is occasionally stated that the mathematical prediction of the expanding Universe was also made by Robertson. This is a misunderstanding which we also discuss in our book. In 1928 Robertson submitted to the *Philosophical Magazine* an article in which he wanted to replace de Sitter's line element by "a mathematically equivalent solution in which many of the apparent paradoxes inherent in [de Sitter's solution] were eliminated". He also arrived at the formula which in Lemaître's hand had become the distance–velocity relation. However, he wrote this as $v = c \times (l/R)$, where l is the distance of the nebula and R the radius of curvature of the Universe, for which he was looking within a static solution.

Robertson then took practically the same set of observations as had been taken by Lemaître one year before and would be taken by Hubble one year later. From this he calculated $R = 2 \times 10^{27}$cm. His c/R corresponds to $H = 463$ km s^{-1} Mpc^{-1}; but this he did not calculate. Robertson placed an important milestone in our understanding of cosmological solutions of the Einstein equations. Solutions of Einstein's equations, in general, do not obey special symmetries. Yet, to describe the large-scale structure of a spatially homogeneous universe, the four-dimensional space–time is usually separated into a spatial and a time component. Moreover, to "treat every point in this world equally" — the content of the Copernican principle — and to implement the observational constraints into our models, leads to the hypothesis of universal homogeneity and isotropy. These premises imply symmetries in the solutions of Einstein's equations. Robertson was the first to search in detail for all the mathematical universes that satisfy these physical requirements.

(*10*) It is sometimes claimed that it was Hubble who converted Einstein to the expanding Universe. This is very unlikely. Although there is no written report about the moment when Einstein was converted, it is highly probable that it happened when Eddington showed him that his static solution was unstable. We discuss the circumstances further in our book, where we provide the evidence.

(*11*) Neither Hubble nor Lemaître rested on their laurels. With the help of the world's most powerful telescopes on Mount Wilson, Hubble and Humason measured distances and redshifts far beyond all other existing observations. Their data, later continued by Sandage, would become one of the cornerstones of observational cosmology. Lemaître had another impact, when in 1931 he suggested in a one-column letter to *Nature*, what would become the Big Bang, and in 1933, in a paper read before the American National Academy of Sciences, Lemaître suggested vacuum energy as the deeper meaning of the cosmological constant Λ. These exploits have also been highlighted in detail by Jean-Pierre Luminet[8].

(*12*) If Hubble was not the discoverer of the expanding Universe, why is he still often highlighted as such? Kragh & Smith[9] have looked into the evolution of the 'Hubble-myth'. They find that not until the 1950s did the notion of 'Hubble's law' and 'Hubble as the astronomer who had discovered the expanding Universe' become common in the scientific literature, where Hubble's rôle was gradually elevated at the expense of everyone else's. They conclude: the label 'Hubble's law' is an example of what has been called Stigler's law of eponymy, namely, 'No scientific discovery is named after its original discoverer'.

Discussion

The discovery of the expanding Universe is a picture-book example of an individual scientist who was aware of a burning scientific issue and solved it. It did not happen in a vacuum. Lemaître had benefitted from Eddington's insights into General Relativity. In his 1927 paper he also cites Lanczos and Weyl, and he stood, of course, on the shoulders of Einstein and de Sitter. But similar arguments could be held against Newton and Einstein. However, if we apply our usual standards of attributing scientific discoveries, we should recall the situation of 1927. Einstein's static universe could not explain Slipher's redshifts, and de Sitter's theory which provided redshifts was incomprehensible. (It was unphysical in the sense that it violated the principle of homogeneity. Today's 'de Sitter universe' has not the same attributes as de Sitter's 1917 model.) Lemaître spotted the problem in de Sitter's work, one of the great figures of astronomy in the first half of the 20th Century. Before Lemaître, only Friedmann had been sufficiently reckless to follow up seriously the idea of a truly dynamical universe. Then, in 1927, Lemaître derived from Einstein's fundamental equations the solution of a dynamical universe. To create a link to observations, he looked for the effect that his model would have on spectra of distant sources. This gave him the linear velocity–distance relationship $v = \mathrm{H} \times d$, where a redshift signified an expanding universe, blueshifted spectra would have meant a shrinking universe. He then collected the available redshifts and distances to derive the missing factor of proportionality, which could not be derived from theory. The observations assured him that we live in an expanding universe. This was one of the most fascinating discoveries ever made.

The full story is much richer and more colourful than what can be summarized on a few pages, and the following very incomplete list of references is much extended in our book[3].

Yours faithfully,

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REVIEWS

Taking Possession of Astronomy — Frontispieces and Illustrated Title Pages in 17th-Century Books on Astronomy, by Inga Elmqvist Söderlund (Center for History of Science, Royal Swedish Academy of Sciences, Stockholm), 2010. Pp. 400, 24 × 17 cm. Price: SEK 150 (about £14), Paperback (ISBN 978 91 7190 137 8).

Here is an unusual book. Conceived as a thesis, this luxurious compendium gathers together about 300 frontispieces and illustrated title pages from European books on astronomy published in the 17th Century. In her own words, the author has "undertaken to show how a frontispiece in a 17th-Century astronomy book is able to invite the user to enter into a sumptuous palace or a beautiful garden, ornate with works of art, to be offered to drink from the fountain of wisdom."

But this book is much more than a mere collection of pictures with their descriptions. The author undertook a quantitative and qualitative survey of how their motifs were related to use, identification, and display, distinguishing elements linked to factual content as opposed to those aimed at raising the perceived value of astronomy.

The first chapter reviews the objectives, earlier research, the material studied, the methodology used, and the contents of the thesis. The second chapter is devoted to 17th-Century astronomy, to the profession or rather the identity of the astronomer of the time, as well as to the then-links between books and graphical arts — in other words, to the astronomical context and to the 'book anatomy' in the 17th Century. The relationships between frontispieces and the general topics of the books are also analysed. A third chapter investigates the pictorial themes of the frontispieces while a fourth one studies the frontispieces as a genre. A final analysis and a summary make up a fifth chapter.

The quantitative study shows that astronomical phenomena and scientific instruments are the most common motifs used to inform the reader of the contents of the books. Besides these, a wide range of depicted features indicate the particularity of each title. The motifs mainly promote delectation and erudition, although some retain attention through a deliberately enigmatic design, occasionally evoking fear. The survey determines prevalent settings (such as palaces, theatres, gardens, wilderness, and heaven), activities (skilful usage of instruments, conversations, and disputes), references to the ancients as well as heraldic components. They present both the self-image of astronomers at the time and ideal components that contain connotations of an enhanced reality.

Many of the illustrations are reproduced in colour. An appendix lists all of them at the end of the volume where readers will also find an extensive bibliography as well as a section gathering four descriptions of frontispieces (translated from Latin) too long to be quoted within the body of the book itself.

This volume is not only for the bibliophile and the astronomy historian, but also for the devotee of beautiful works of art. It can be ordered from The Observatory Museum, Drottninggatan 120, 113 60 Stockholm, Sweden (observatoriet@kva.se). Shipping expenses will be added to the price mentioned. It is also available at http://wwwcenter.kva.se/bilder/Avhandling_5.pdf — A. Heck.

Exploring Ancient Skies: A Survey of Ancient and Cultural Astronomy, 2nd Edition, by D. H. Kelly & E. F. Milone (Springer, Heidelberg), 2011. Pp. 639, 28 × 21 cm. Price £72/$99/€79·95 (paperback; ISBN 978 1 4419 7623 9).

The first edition of this excellent textbook came out in 2005, and, apart from a new photograph or two and a few textual corrections, very little has changed. The book is based on an archaeoastronomy course at the University of Calgary, Canada, taught by the authors to a collection of astronomy, archaeology, and general second-year students. And, quite rightly, the lectures are designed to be supplemented by extensive private reading and reference to current research work. In *Exploring Ancient Skies* the students are encouraged to wander the early pre-telescopic world, and investigate the usefulness of astronomy to ancient cultures.

We read how astronomy both aided navigation, provided a clock, and regulated the calendar. But there was much more to ancient astronomy than the mere utilitarian. The book stresses how mankind's viewpoint was more holistic in those distant days. Events in one's life, in the local civic society, and the surrounding countryside, and in the sky above, were all thought to be interrelated. Not only did the movement of the Sun and Moon affect us, we thought our actions could affect them. Our rôle and place in the Universe was a matter of great interest and discussion.

Kelly & Milone start by setting out the fundamentals of the relevant astronomical phenomena. They then investigate the astronomical artefacts of megalithic society, moving on to the rôle of astronomy in Mesopotamian, Hellenistic, Egyptian, Indo-Iranian, and far-eastern cultures. Much is also made of the astronomy of the Oceanic societies and of the peoples of Mesoamerica, and North and South America.

This excellent introduction to the subject is profusely illustrated, and eminently readable. The book contains a host of references to research work, but the fact that this edition is published in 2011 does not hide the fact that the references tail off drastically after about 1999. — DAVID W. HUGHES.

Quantum Man: Richard Feynman's Life in Science, by Lawrence M. Krauss (W. W. Norton, New York), 2011. Pp. 350, 20 × 13 cm. Price $24·95 (about £16) (hardbound; ISBN 978 0 393 06471 1).

Richard Feynman did not dislike conventional astrophysics and cosmology quite as did Lev Landau, but he was in the running, and a letter he wrote back to his wife, Gweneth, from a relativity meeting in Warsaw in 1962 would border on the libellous if one were absolutely sure of the names of the people he refers to. He came, nevertheless, eventually to try to understand why gravity is so weak, approaching it as the theory of exchange of massless particles of spin 2 (spin 1 gives you photons and electromagnetism). In author Krauss' view, Feynman's work on this territory underlies current developments in (*i*) black holes and Hawking radiation, (*ii*) string theory and beyond, (*iii*) path integrals in quantum gravity and 'quantum cosmology', and (*iv*) cosmology, flatness, and gravitational waves (the sort left over from the early Universe, not the sort that were already being discussed at Warsaw). The book absolutely bristles with other important parts of physics for which RPF did something between laying the foundations and completing the structure, all explained at just the right level for those of us who hope we remember our physics.

There exist several other biographies of Feynman, none of which seemed to describe the man I knew. This one does, and it is hard to believe that he has been dead for nearly 25 years. — VIRGINIA TRIMBLE.

Mapping the Universe: The Interactive History of Astronomy, by P. Murdin (Carlton Books, London), 2011. Pp. 124, 30·5 × 27 cm. Price £30 (hardbound; ISBN 978 1 84732 885 4).

Of all the sciences, surely astronomy has the longest and most fascinating history, and similarly is blessed with the most amazing of visually stunning subjects. In which case, this latest book from Paul Murdin ought to be a sure-fire success, containing as it does a wealth of superb illustrations and a wide-ranging selection of historical topics outlining our progress, from the speculations of early man to the latest results from the most sophisticated instruments on the ground and in space.

Of course, in just over 100 pages, the history cannot be complete but does represent the most important steps forward. In a number of these cases, 'evidence' is presented in the form of facsimile documents, such as Galileo's drawings of the Moon, a page from William Herschel's notebook describing the discovery of what later became the planet Uranus, and a beautiful composite image of Saturn and its rings captured by the *Cassini* spacecraft. These 17 documents, which come in four fold-out envelopes distributed through the book, might cynically be regarded as a gimmick, but equally should be considered as valuable and inspirational teaching aids. Thus I see this lavishly illustrated volume as destined for the school library as much as for the coffee table. And at the modest price of £30, it could be considered a bargain for either.

The titles are somewhat curious: I'm not quite sure how one can 'interact' with history, and neither does 'mapping' seem quite the right term for an historical *paseo*. And for future editions, perhaps the printers can ensure that the dark colours from a number of the illustrations don't 'rub off' on the white facing pages. That said, it's a delightful volume to peruse with a padded 'hard' cover making it a pleasure to handle. — DAVID STICKLAND.

The Disappearing Spoon, by Sam Kean (Little, Brown & Co., New York) 2011. Pp. 391, 20 × 13 cm. Price $14·99 (about £10) (paperback ISBN 978 0 316 05163 7).

A widely and favourably reviewed bestseller, Kean's book touches upon a number of scientific (and a good many non-scientific) topics, organized around groups of chemical elements that, in his view, have something in common, different from what you might deduce from a periodic table or previous experience. 'Elements as money', for instance, includes zinc, gold, tellurium, europium, and aluminium; but silver lives with copper, vanadium, gadolinium, sulphur, and rhenium as elements with medical applications. Interesting and, one hopes, informative.

Or so one hoped until I hit the bits I'm supposed to know something about, 'Where the atoms come from', the astronomy chapter. The author begins by telling us that most young stars contain only hydrogen and helium. Three pages later, he has reached howler number 21, describing the Shoemaker–Levy 9 event of 1994 as "the first intergalactic collision humans have ever witnessed." You'll know when you get there, because he also mentions the 21 fragments. I counted 35 by the end of the chapter, plus two more in the associated notes. (No, silicon fusion doesn't work as $^{28}Si + ^{28}Si \rightarrow ^{56}Ni$.) A glass of good wine is on offer to the first reader who catches at least as many infelicities as I did. Meanwhile, please read Eric Scerri's *The Periodic Table*, or *Uncle Tungsten*, by Oliver Sacks.

The disappearing spoon is, of course, made of gallium and disappears when melted by hot tea, in a chapter shared with arsenic, ytterbium, and a subset of the lanthanides. You might want to add to your store of lore that astatine is the only element whose discovery was confirmed by a non-primate, a literal guinea pig belonging to Emilio Segre. This can live next to the only scientific discovery ever made by a whole horse (soliton waves on canals) and the untruth that the atomic weight of holmium is twice that of hafnium.

An ancient objection to electron spin was that the particles' surfaces would have to be moving at 137 times the speed of light. Kean suggests that the inner electrons of element 137 and beyond would have orbital speeds in excess of *c*, crediting the idea to Feynman, in honour of whom element 137 should be called Feynmanium. "Not in my lifetime", is both a safe prediction and a pious hope. — VIRGINIA TRIMBLE.

Weird Astronomy, by D. A. J. Seargent (Springer, Heidelberg), 2011. Pp. 317, 23·5 × 15·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4419 6423 6).

David Seargent brings together anecdotes and descriptions of what are best labelled 'bizarre' observations into a book that unashamedly looks at the lighter side of astronomy. Since most observations that would fall under such a title have to do with things that move visibly or which seem to appear or disappear suddenly, it is not surprising that the majority of the topics involve the Solar System — the Moon, comets, meteors, and meteorites getting the lion's share of attention. Only rather brief space is given over to stars, but a concluding discussion of the Star of Bethlehem offers what seems to be a quite convincing explanation of it.

The author attempts to reconcile the observations with known facts, though in almost every case there are crucial gaps in the data so it becomes more of a guessing game as to what the observer did actually see. Most of the time it is not possible to explain the weird reports, and as hardly any were captured on any kind of detector it is not easy to revisit them in the cold light of modern knowledge — and in any case the epithet 'weird' ceases to be relevant when a completely rational explanation is found. Many of the sightings of puzzling phenomena date back far enough to be called 'historical', and those also suffer from the limitations of equipment (or of none at all); it is well known that astronomy makes significant progress when a new measuring or recording tool is introduced, and it is hard — both scientifically and emotionally — to place realistic errors on some of the uncalibrated observations described here. The occasional observing 'tasks' that are dotted throughout the book nudge the reader into trying to do better.

Seargent writes well in an easy, chatty style, and his book is remarkably devoid of errors (though I did not understand why an Australian author needed to write in American English). I found the over-frequent use of exclamation marks a little irritating, and his joke that "Father Hell" was a curious name for a priest only revealed an ignorance of German. He manages to impart a sense of fun and good humour to his subject, but that has to be the limit of what such a book can provide. There is so much in *real* astronomy that is 'weird' in some way (quite apart from the discovery of potassium flare stars) — the stars that do not conform to the general run of the mill or have spots and other unexpected defects, the ones that seem to be lying on their backs as they orbit, the objects which appear as multiples when seen through a nearer intervening mass, the things that flash on and off without warning … . Things that are truly 'weird' play a critical part in scientific understanding (it is by introducing the 'abnormal' into currently accepted models that we learn the inadequacies of the models), and it is to be hoped, therefore, that this book is not seen as a reflection of the state of our art. The mention of so many dubious 'observations', and even of hoaxes, may not bring professional astronomy much credit. — ELIZABETH GRIFFIN.

Questions of Modern Cosmology: Galileo's Legacy, edited by M. D'Onofrio & C. Burigana (Springer, Heidelberg), 2009. Pp. 530, 24 × 16·5 cm. Price £81/$129/€89·95 (hardbound; ISBN 978 3 642 00791 0).

Editors D'Onofrio and Burigana have given us a book most unusual in both form and content. It is not, however, quite unique, because D'Onofrio and two other colleagues have just about finished the editing of a similar volume on quasars, marking the 50th anniversary of their discovery.

The general idea is that the editors have provided a short introduction and conclusion, and the rest consists of four long chapters in which the editors ask questions of other astronomers, beginning "Dear Juan (Francisco Macias-Perez)", "Dear Isabella (Gioia)", *etc.*, followed by extended answers from 43 contributors, most answering multiple questions. These are described as "interviews" in the preface, but in fact seem to have been done at a distance.

The intent is to explore pros and cons of, and alternatives to, current, conventional, ΛCDM wisdom, in honour of Galileo, especially his *Dialogo* (though the opposing view for him is described as "Tolemaic-Aristotelic"). Indeed, the cons and alternatives are given far more space here than in the vast majority of other books about cosmology, and it is no bad thing that these rarely-heard voices should have their say. Many interesting nuggets of information appear here, *e.g.*, that your expression for the cosmological Jeans mass must include dark matter, though what, if anything, you do about dark energy or a cosmological constant in this context is not obvious; and Galileo's analogy between sounds produced by cicadas and formation of comets.

I have two major criticisms of the book. First, it is almost impossible to find anything. The 1356 references occur in five chunks, at the ends of the chapters, rather than either all together (probably best) or with the individual contributors' answers. And they are cited in the text as numbers, not name and year, so that (64) in Chapter 2 (on a possible new test of GR from *Planck* satellite data) is entirely different from (64) in Chapter 5 (on the data-stream processing from *WFCAM*). The indexing helps hardly at all. There is no name index, and the subject index is very sketchy, with no entry for supernovae (in their context as standard candles or anything else), and none of the nine pages ascribed to the Sunyaev–Zel'dovich effect provides even the basic equations.

The second is the choice of contributors. These are, one-sixth women (fine) and almost three-quarters European (fine, given where the editors started from). But 20 people have won the Gruber Cosmology Prize since its inception in 2000, and only two of them are here. John Mather provides cogent and mainstream words about possible deviations of the microwave-background spectrum from a Planck curve, and Simon White is represented primarily by his reservations about devoting astronomical resources to the study of dark energy. Were the other 18 not asked on the grounds of being too conventional to appeal to the editor? Did many of them decline to participate? Or what? Nothing is said in the preface about why the contributors were chosen nor why others were not.

Much of the English is non-idiomatic. The odd part is that this includes text by native speakers ("I never felt to be advocating any particular replacement for standard paradigm." p. 406), leaving me wondering about the extent to which contributors are speaking for themselves.

But if you are really in the mood to be left wondering, read Section 2.8.7 by Pierre-Marie Robitaille of the Department of Radiology at Ohio State University. For him, not only is the temperature of the CMB not 2·725 K (indeed he suggested that *Planck* might not see a CMB from L2), but also the temperature of the solar photosphere is not 6000 K. It is millions of degrees, with the nuclei arranged in a lattice. Searching for CMB anisotropies in his view resembles the difficulties of water suppression in biological NMR. And having just signed on for a series of fMRI (the polite synonym for NMR for folks frightened of 'nuclear') experiments concerning brain function in aged university faculty, perhaps I ought to worry, but whether about the state of the Universe or the state of my aged brain is not quite clear. — VIRGINIA TRIMBLE.

Weaving the Universe: Is Modern Cosmology Discovered or Invented?, by Paul S. Wesson (World Scientific, Singapore) 2011. Pp. 204, 23 × 16 cm. Price £45/$65 (hardbound; ISBN 978 981 4313 94 0).

Wesson says that our description of the Universe is invented rather than discovered. In saying that he is not implying that its physical laws are socially determined, as some post-modernists claim, but rather that physical laws are either biologically determined in the way Kant says our notions of space and time are, or that they are decided by convention to suit our convenience, as dimensions are.

The best example of the biological determination of physics is probably the arrow of time. Wesson devotes a chapter to his suggestion that time is an illusion: time, he says, is a subjective ordering device used by humans to make sense of their world. I myself find the notion of time being an illusion hard to accept, but then I also find the idea of free will being an illusion hard to accept. (As Isaac Bashevis Singer said, "We have to believe in free will; we've got no choice".) After reading Wesson's arguments, I found myself thinking that the nature of time is as big a mystery as the nature of consciousness.

Dimensions, on the other hand, are neither mysterious nor rigidly fixed. We can, after all, put the fundamental quantities G, c, and h equal to one, and then all physical quantities can be expressed in terms of a single dimension such as length. Wesson has devoted two previous books (*Space-Time-Matter* and *Five Dimensional Physics*) to exploring the consequences of making mass a fifth dimension on a par with the four dimensions of space and time.

Wesson claims that science and religion are immiscible — a claim with which many scientists today would agree. Indeed, Galileo long ago defended science by asserting that it is completely separate from religion: "The bible teaches us how to go to heaven, not how heaven goes." The lunacies of creationism show us that this position has a lot to recommend it, yet it is not how most pioneers of the scientific revolution viewed the relation between science and religion. Kepler saw his discoveries as revealing the archetypes underlying God's creation. Newton worked with such obsessive intensity on his system because he believed it revealed the inner nature of God, and his extensive studies of alchemy were designed to reveal the processes involved in God's creation.

Perhaps the most interesting chapter is the second one, in which Wesson discusses some of the greatest puzzles of physics. Surprisingly, he starts with Olbers' paradox (first stated by Kepler long before Olbers), even though this should no longer puzzle us: Wesson himself has demonstrated that the darkness of the night sky is overwhelmingly due to the finite age of the Universe, only a small contribution to the darkness coming from the redshifts of receding galaxies.

Wesson's other puzzles, however, remain unsolved. The observed value of Einstein's cosmological constant Λ is some 10^{120} times smaller than the value deduced from quantum field theory. And the Planck length is much, much smaller than any other physical length.

Dark matter is often explained by particle physicists in terms of supersymmetry and its resulting WIMPs but, as Wesson points out, this comes at the price of introducing several mysterious extra dimensions. Moreover, this leaves unaccounted for the even more mysterious dark energy that produces the acceleration seen in supernovae and that has been measured exactly by the *WMAP* satellite.

Finally Wesson discusses Fermi's question: if there are intelligent beings elsewhere in the Galaxy, why aren't they here? Michael Hart has adopted the extreme position that this puzzle is so insoluble that it is proof that there is no intelligent life at all elsewhere in the Galaxy. I would prefer the opposite extreme position that intelligent aliens do exist and have sent inconspicuous probes that we don't notice. Whatever resolution eventually turns out to be correct, Fermi's question is a major puzzle.

Whether one agrees or disagrees with Wesson's positions, his book is profoundly thought-provoking, and this I think is the greatest virtue that any scientific book can have. — MARTIN CLUTTON-BROCK.

The Galactic Supermassive Black Hole, by F. Melia (Princeton University Press, Woodstock), 2007. Pp. 296, 18 × 15·5 cm. Price £32·50 (paperback; ISBN 979 0 691 13129 0).

I never saw a Kerr black hole,
But now can hope to see one
At angles equal microsecs,
For Sag A must be one.*

It is very much to author Melia's credit that he presents no doggerel along these lines. Instead, he ends his volume with descriptions, and references to detailed calculations, of the shadowing, micro-lensing, and spin-induced disc precession that can be expected from a $3 \times 10^6 M_\odot$ black hole about 8500 pc from us. Similar effects are expected from the black holes in X-ray binaries and active galaxies, but only at still finer angular resolution.

Bruce Balick begins the story with a 1974 February observing run at NRAO that revealed a very compact radio source at the Galactic Centre. He and Robert Brown had asked to look there in 1973 January as part of a general study of H II regions. Awkwardly, Miller Goss and Dennis Downes asked specifically to look for a black hole in Sag A a few months later, but you must read Balick's preface to the book for more details.

In between discovery and forecast come radio, X-ray, and infrared observations of the central region and its core, including variability (nearly always a signature of compact structure); the central star cluster with its mass-defining star orbits; 40 pages of essential relativity; mass accretion and expulsion; and flares.

The book is light on history. Bondi–Hoyle capture is a sub-section heading, but you must root in the list of references to learn that one of the two key papers is actually by Hoyle & Lyttleton*. And no amount of fine-tooth-combing will enable you to learn from Melia that Yakov B. Zel'dovich proposed the fuelling of quasars with accretion on very massive black holes the same year that Edwin E. Salpeter suggested it (1964). The text mentions the rôle of Donald Lynden-Bell and Martin Rees in predicting the potential observability of radio emission from the vicinity of a Galactic black hole (in 1971) but not the on-going rôle of the 40-year-older Jan H. Oort in drawing repeated attention to the range of phenomena that could best be explained that way.

The blurbs on the back cover describe Melia's volume as a "gripping high-level account", a "timely, comprehensive, and readable account", and an "ideal resource for researchers", and since I didn't write any of them, I can heartily concur with all. — VIRGINIA TRIMBLE.

Galaxy Collisions: Forging New Worlds from Cosmic Crashes, by C. Struck (Springer, Heidelberg), 2011. Pp. 300, 24 × 16·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 0 387 85370 3).

This is quite a tricky one to review. Not that there is anything particularly problematic in the text itself — it is more a question of where the book might fit in the market and therefore on what basis it should be judged. According to Springer's website, the content level is 'popular/general', but it doesn't really appear 'general' enough to be 'popular'. Perhaps a pointer is in the publishers' blurb on the back cover, where the bullet point, "uses analogies and metaphors to help explain the complex nature of galaxies", is presumably supposed to be a selling point. In fact, the form resembles a non-technical version of *Annual Reviews*, if you can imagine such a beast, with (usually very formal) 'namechecks' for all the authors of work mentioned. The uncertainty about which way to jump is perhaps best illustrated by the appended 'Glossary' and 'Resources'. The Glossary includes "km/s — kilometers per second. One kilometer per second is 3600 kilometres per hour", while the Resources end with Binney & Tremaine's *Galactic Dynamics*. I doubt that anyone not knowing the former would get far with the latter!

The book is clearly a labour of love by author Curtis Struck, a long-time researcher into galaxy collisions, who attempts to put across his fascination with these events to a wider readership. I found it something of a slow build-up, with early chapters on particular special cases, like ring galaxies and galaxies with tidal tails. Later on, though, it picks up the pace when discussing wider issues of galaxy mergers and star formation as key elements in galaxy formation and evolution. And, of course, it provides the opportunity to showcase the many wonderful images (from *HST* and elsewhere) of interacting systems.

In terms of style, I probably got off on the wrong foot by noting the persistent (over) use of the phrase "world of galaxies", but again, things improved as it went along. I'll forgive the occasional attempt to communicate with a younger readership *via* comments such as "there are several cool features about this phenomenon", and trust that something being "very Rube Goldberg" means more to an American than it did to me (apparently, it roughly translates as "Heath Robinson").

*Some years ago a distinguished biologist remarked that while there are organizations devoted to saving whales, elephants, and even sea turtles, there are no societies in support of nematodes. I have occasionally been tempted to try to organize a Lyttleton Appreciation Society.

Errors in the text are relatively rare, but (probably through no fault of the author's; I sympathise from experience here!) the same cannot be said about the figure captions. In addition to typos, some captions are for figures different to the one shown, an inset on one figure just shows the same field of view, but shrunk down in size, and one claims to show different Local Group galaxies "to scale", but has NGC 205 the same size as M 31. But my favourite has to be a reference to the "Hundbuch der Physik". Clearly dogged by misfortune at the publishers! — STEVE PHILLIPPS.

The Galactic Centre: A Window to the Nuclear Environment of Disk Galaxies (ASP Conference Series, Vol. 439), edited by M. R. Morris, Q. D. Yang & F. Yuan (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 494, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 758 2).

Editors Morris, Wang & Yuan have provided a fairly standard, minimalist volume of proceedings for a conference that took place in Shanghai in 2009 October. The list of 132 participants is unusual in that only a dozen are from the host country. Papers range from 17 pages down to two for what were presumably poster presentations, and are divided into six sections on interstellar matter, stars near the Galactic Centre, accretion, supermassive black holes in general, and so forth. There are no indices of any kind and neither an introductory presentation indicating the scope of the conference and its subject matter nor a concluding one with highlights and remaining unsolved problems. My favourite of the invited review papers is that by Andrea Goldwurm on high-energy emission, which begins by reminding us that X-ray emission from the Galactic Centre in Sagittarius (long before it had its A*) was suspected as long ago as 1965, and ends with hopes for future progress from data obtained with space missions, very few of which are now likely to fly. On balance, however, a reader hoping to come up to speed on our own black hole and its significance might be better served reading the monograph by F. Melia (see page 405). — VIRGINIA TRIMBLE.

Physical Processes in Circumstellar Disks around Young Stars, edited by Paulo J. V. Garcia (University of Chicago Press), 2011. Pp. 422, 23 × 15 cm. Price $150 (about £92) (hardbound; ISBN 978 0 226 28228 2), $55 (about £34) (paperback; ISBN 978 0 226 28229 9).

Editor Garcia begins by setting the stage with an outline of observed properties of circumstellar discs that begin to say something about their appearance, masses, compositions, accretion rates, structure, irradiation, and evolution. He also points out that an important motivation for much of the work on discs surrounding young stars and protostars is the wish to understand the origins of planetary systems. At this point, one already wishes the volume had come with an index to check how many of the chapters actually got as far as planet formation. The answer is Durisen, on disc hydrodynamics, yes; some of the others, no. I also wished that the colour plates had included some parallel pairs comparing calculation/simulation results with observations. In fact, nearly all show calculations in one form or another.

There are, of course, lots of equations, of which the ones I found most interesting are those where, having calculated or measured something, the author ends with an expression with a typical number at the beginning, followed by dependence on relevant physical quantities, so that one can deduce that a typical disc mass is 0·03 $M_{\odot}$ or a typical viscous time-scale is 10^4 years, compared with a gas freeze-out time of 5×10^3 years. Discs, rightly, disperse in the last chapter (Clarke), though which of several possible mechanisms dominates (photo-evaporation by the central star, photo-evaporation by external radiation fields, dynamical effects of planets, or stellar companions, or others) is less clear and, of course, need not be the same for all discs.

The volume is perhaps not quite self-contained for the announced readership, beginning with advanced undergraduates (what is Hall-Ohm MRI?), but comes much closer than a typical conference volume. The eight main chapters range from 30 pages on up, *i.e.*, long enough to tell the story. Each chapter has been refereed, mostly by people who waived anonymity and could just as well have written the chapter (Ant Jones for Henning and Meeus; Ewine van Dishoeck for Bergin; *etc.*).

James Pringle, Suzan Edwards, and I each provided a few lines of friendly words for the back cover (based on proof copies) in return for which we each received a paperback copy of the published book. — VIRGINIA TRIMBLE.

Atomic Astrophysics and Spectroscopy, by A. K. Pradhan & S. N. Nahar (Cambridge University Press), 2011. Pp. 363, 25 × 19·5 cm. Price £45/$75 (hardbound; ISBN 978 0 521 82536 8).

Spectroscopy is *a*, if not *the*, principal tool distinguishing observational astronomy from observational astrophysics. While there are shelves of books on spectroscopy for physicists and chemists, books on spectroscopy directed at astrophysicists are surprisingly rare indeed. Therefore, the arrival of *Atomic Astrophysics and Spectroscopy* was warmly anticipated, particularly as the authors have worked extensively at the spectroscopy–astrophysics interface.

Pradhan & Nahar undertake a bold experiment to cover both atomic spectroscopy from radiative to collisional processes and astrophysics from stars, nebulae, AGN, and quasars to cosmology. Atomic spectroscopy is primarily covered at or beyond the graduate level but much of the astrophysics is at the undergraduate level. The strength of the book from the point of view of this practising stellar astrophysicist is found in early chapters on atomic structure and processes, especially the chapters on electron–ion collisions, and electron–ion recombination. An emphasis is on theoretical approaches to the calculations of cross-sections, but experimental results are discussed in places.

On the astrophysical side, some areas are discussed in too little detail for the linkage with atomic physics to be clearly apparent. Stellar atmospheres is an area where one could have conveyed to atomic spectroscopists the beauty of stellar spectra and the continuing need for theoretical and experimental information on atomic processes. Yet the section on 'atmospheres' in the chapter on 'Stellar properties and spectra' is a very general discussion occupying just one half of a page. In other areas, the authors do successfully provide astrophysical examples with which to intrigue atomic spectroscopists, *e.g.*, the discussion of Fe II-line formation to be found in the chapter on AGN and quasars.

The challenge of writing a book on atomic astrophysics and spectroscopy is a daunting one that few would even contemplate accepting. I commend the authors for accepting the challenge and providing a text that despite its uneven handling of the vast subject matter is one I shall consult frequently when next I face a class in spectroscopic astrophysics. — DAVID LAMBERT.

Astronomical Data Analysis Software and Systems XIX (ASP Conference Series, Vol. 434), edited by Y. Mizumoto, K.-I. Morita & M. Ohishi (Astronomical Society of the Pacific, San Francisco), 2010. Pp. 518, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 748 3).

Astronomical Data Analysis Software and Systems XX (ASP Conference Series, Vol. 442), edited by I. N. Evans, A. Accomazzi, D. J. Mink & A. H. Rots (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 706, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 764 3).

The appearance of a single review covering the proceedings of ADASS conferences from two consecutive years reflects more than a reviewer with insufficient respect for editorial deadlines. The volume for *ADASS XX* appeared little more than six months after the conference itself, if not quite overtaking the proceedings of *ADASS XIX* in the race to publication then, at least, giving it a good run for its money in the home straight. The *ADASS XX* proceedings volume is also the first of this series to appear in full colour, which is a great improvement given that many ADASS proceedings papers include screenshots of software designed to present information in a visually powerful manner.

Both these developments are the result of a reflection, by the ADASS Program Organising Committee and by the wider ADASS community, on the status of the ADASS conference series and on the rôle played by its proceedings volumes. As noted by Arnold Rots in his contribution to the *ADASS XIX* proceedings, the ADASS conferences regularly attract ~300 attendees each autumn, and for many the proceedings provides their single annual opportunity to publish their work, given that the mainstream astronomy journals do not see themselves as the proper home for papers on astronomical computing that do not present original astronomical research results; the policies of the different journals are detailed in the contribution that Norman Gray and I made to the *ADASS XX* proceedings, and the gap in the market that we delineate is currently being addressed by a major journal publisher.

For the time being, the ADASS proceedings volumes should continue to find a home in all serious astronomy libraries, but whether they will retain for much longer their unique rôle in recording advances in the computational techniques and technologies supporting astronomical research is an open question. — Bob Mann.

An Introduction to the Solar System, Revised Edition, edited by D. A. Rothery, N. McBride & I. Gilmour (Cambridge University Press), 2011. Pp. 412, 26·5 × 21 cm. Price £35/$65 (paperback; ISBN 978 1 107 60092 8).

This is an Open University second-level textbook, divided into nine chapters with separate authors for each. The authors are the editors, together with P. A. Bland, E. A. Moore, M. Widdowson, and I. Wright. The chapters are on individual themes which discuss all the germane planets and planet-like bodies together, and there are copious cross-references to prevent material becoming lost in the gaps between the chapters. There are problems for the student interleaved in the text and a summary at the end of every chapter.

The terrestrial planets, the Moon, and some other planet-like bodies have much in common. The internal structure of the Earth and Moon can be deduced from seismometry. The internal structure of the others, particularly the bulk density, can be found from the orbits of their satellites, natural or artificial. The Earth's surface is continually changing because of plate tectonics, volcanism, and erosion. These phenomena are different on other planets and may have occurred in the past although absent at the present. Other phenomena such as cryovolcanism are absent on Earth. All planets are subject to collisions with asteroids, comets, or the small bodies which comprise interplanetary dust. The frequency of impact craters is an index of the age of a surface but the existence of a dense atmosphere reduces the effects of any collision.

There are several theories of the origin of the Solar System which must explain the separation of the gas giants from the terrestrial planets by the main belt of asteroids. The theory must also explain the layering in the interiors of the terrestrial planets. Knowledge of the interiors of the gas giants is limited by their opaque atmospheres and must be deduced in the same way as for the terrestrial planets. The *Galileo* and *Cassini* space missions have revealed an enormous amount about Jupiter and Saturn and their satellites. All the gas giants have dipole magnetic fields but differ from each other in the offset between the dipole and mass centres, and in the angle between the dipole and rotation axes. All are nett heat sources except for Uranus, for reasons not understood.

Although they contribute little towards the mass of the Solar System, asteroids and comets affect the major planets through frequent collisions. Comets produce tails when their ices melt and the outflow of water produces non-gravitational forces.

The rapid increase in knowledge of the Solar System, mostly from space missions, well merits the present revision after only seven years. This is an excellent textbook, well up to the standard of other Open University texts, and could well be used at other universities. It also provides a valuable reference book for specialists in other fields. — DEREK JONES.

The Kaguya Lunar Atlas, by M. Shirao & C. A. Wood (Springer, Heidelberg), 2011. Pp. 184, 22·5 × 28·5 cm. Price £35·99/$39·95/€39·95 (hardbound; ISBN 978 1 4419 7284 2).

The Japanese *Kaguya/Selene* spacecraft was one of the most successful lunar missions of recent times. Between its launch in 2007 September and its planned impact on the lunar surface on 2009 June 10, the craft returned a wealth of data about our satellite from a suite of thirteen science instruments, including a laser altimeter and a high-definition television camera. The altimeter measurements allowed the construction of a detailed topographic map of the entire Moon, while the HDTV camera produced stunning images of specific surface features.

It is those images that form the heart of the present volume. Unlike most results from other lunar spacecraft, the *Kaguya* images are taken from an oblique angle and provide a dramatic astronaut's-eye view that reveals familiar features in an unfamiliar and novel light. This in turn encourages fresh perceptions of the forces that have contributed to the moulding of the lunar surface. *The Kaguya Lunar Atlas* is not an atlas in the strictest of senses — it makes no attempt at completeness of coverage, for example — but it does offer a valuable new resource to the armchair explorer of the Moon. The images of selected formations are of the highest quality, and they permit detailed examination. They are supported by a well-judged text by two authors who are experts in their fields. Motomaro Shirao was a co-investigator on the *Kaguya* project, and he provides a detailed overview of the mission and its scientific aims. Charles Wood, a professional lunar scientist well-known for his 'Lunar Picture of the Day' website and his regular contributions to *Sky & Telescope*, offers a review of the types of feature to be found on the lunar surface, as well as detailed geological explanations to accompany each image in the atlas. These explanations permit an informed reading of the chronology of events that have contributed to what we see today.

In brief, this is a marvellous book. The *Kaguya* images are spectacular, the authors have provided outstanding explanatory support, and Springer has risen to the challenge of producing the finished atlas to a very high standard. All are to be congratulated on the result — a book that will prove indispensable to any serious student of the Moon. — BILL LEATHERBARROW.

David Levy's Guide to Eclipses, Transits and Occultations, by D. H. Levy (Cambridge University Press), 2010, Pp. 177, 24·5 × 17·5 cm. Price £18·99/$25·99 (paperback; ISBN 978 0 521 16551 8).

The preamble to this book says that "In this simple guide, David Levy inspires readers to experience the wonder of eclipses and other transient astronomical events for themselves". Sadly, this book does not inspire me at all. For me, eclipses are spectacular phenomena requiring good-quality photographs to try and convey the sense of awe and wonder of the event. This book contains some rather poor-quality black-and-white photographs which inspire nothing other than the thought that the book was prepared down to a price.

Levy's book comes in six parts with chapters on the 'Magic and history of eclipses', 'Observing solar eclipses', 'Eclipses of the Moon', 'Occultations', 'Transits', and the author's favourite eclipses. The overwhelming majority of the content is related to eclipses, both solar and lunar. While the information provided is fairly standard fare, I had hoped for something different, perhaps some real insight into the magic of these events or perhaps a description of the observational techniques needed to capture them, bearing in mind David Levy's background as an expert observer of comets. Sadly, I felt this was all missing.

The 'Occultations' section does not offer much useful information either, focussing on subjects like "Ways to muff an occultation". The 'Transits' section is particularly disappointing, consisting of little more than half a dozen pages of historical information and personal reminiscences. On the more positive side, Appendix A is a slightly more useful list of upcoming solar and lunar eclipses in the period 2010 to 2024; however, it offers little more than dates and rough areas of visibility. Appendix B is a somewhat sketchy glossary of some of terms used in the book.

The best way to sum this book up is to say that it lacks that something which will capture people's interest in eclipses, transits, or occultations. This book may well make people wonder what all the fuss is about! It is an opportunity missed. Furthermore, it was produced down to a price with some images whose inclusion still mystifies me. There are much more comprehensive and better illustrated books out there. I suggest that you search them out before considering the purchase of this one. — STEVE BELL.

Stargazers' Almanac 2012, by Bob Mizon (Floris Books, Edinburgh), 2011. Pp. 32, 29·5 × 42 cm. Price £14·99/$25 (ISBN 978 086315 817 9).

I have a theory that the perceived passage of time is some function of the actual time divided by one's age, possibly to some modest power. This presumably explains the increasing rapidity with which the *Stargazers' Almanac* seems to appear, in good time to make a nice present for Christmas.

The format is now familiar, with the main pages, one to a month, giving the mid-month nightly panorama, the upper page facing north and the lower one facing south, together with phases of the Moon, a brief description of the observable constellations and notes on Solar System highlights. However, a notable omission from the June page is the last chance of a lifetime for observers in the UK to catch a glimpse of a transit of Venus, which — weather permitting — should be visible in its closing phases at sunrise on June 6.

The 'special' for the 2012 *Almanac* is a two-page spread on calendars, outlining the bases of various calendars in use today, particularly among different religions, together with the fundamentals of an accurate calendar system. The Campaign for Dark Skies also gets its usual page to bang the drum for less light pollution.

All in all, a fine item to hang on the wall in place of that portrait of a disgraced aunt or uncle. — DAVID STICKLAND.

Hunting and Imaging Comets, by Martin Mobberley (Springer, Heidelberg), 2011. Pp. 408, 23·5 × 15·5 cm. Price £35·99/$39·95/€39·95 (paperback; ISBN 978 1 4419 69040).

Comets have always held a special place in astronomical history and there are few better ways to become famous in the astronomical media than to discover a great comet. The names of George Alcock, Thomas Bopp, Alan Hale, Yuji Hyakutake, Kaoru Ikeya, David Levy, Rob McNaught, Eugene and Caroline Shoemaker are there, and yours might be too if you work hard at it.

In the old pre-telescopic days the job was relatively simple. Comets brighten as they approach the Sun. As soon as the comet had brightened to around 3rd magnitude, its position in the sky altered the well-known shape of the constellation it was in, and its presence became obvious to the naked-eye sky-gazer. It was then admired, recorded, and its progress across the sky was followed. If an incoming comet failed to reach third magnitude is was probably overlooked and went off on its orbit undiscovered. The number of comets recorded per decade depended on the 3rd-magnitude brightness limit.

Now, cometary-magnitude distribution is such[1] that if this limit could be changed to, say, 4th, 5th, 6th, 7th, *etc*. magnitude there would be 2, 4, 8, 16, *etc*. more comets per unit time available for discovery. So replacing the naked eye with wide-field binoculars and telescopes became an essential part of the comet-hunting game. Then, usually after a hundred or so hours of searching per comet, if you were lucky enough to be among the first three both to find a new comet and e-mail your discovery to the Central Bureau for Astronomical Telegrams in Cambridge Massachusetts, the comet would be named after you.

In *Hunting and Imaging Comets*, Martin Mobberley, a well-known British amateur astronomer, describes in loving detail the comet hunting and observing process. Like all hunting, when to do it and where to do it are stressed. He then gives a blow-by-blow account of how, with small telescopes and CCD cameras, these comets can be successfully imaged. Techniques such as image-stacking, flat-fielding, and mosaicing are carefully reviewed. Help is given when it comes to the choice of telescopes, mountings, and cameras. Mobberley also discusses historical comet records, the successes of the amateur comet hunters of recent times, and the serendipitous cometary discovery successes of professional asteroid surveys such as LINEAR (Lincoln Laboratories Near-Earth Asteroid Research Project) and the Spacewatch Patrol.

The joy of comet hunting and comet chasing springs from every page. Sensible practical advice and useful clues abound. But what I liked especially was the underlying aim of the endeavour. There is more to it than just fun. Today's amateur with relatively modestly priced equipment, and home computers, can do excellent cometary photometry and accurate positional measurements. In this way the physics of cometary-nucleus decay, and cometary orbital evolution can be advanced. — DAVID W. HUGHES.

References

(1) D. W. Hughes, *Nature*, **325**, 231, 1987.

Go-To Telescopes Under Suburban Skies, by N. Monks (Springer, Heidelberg), 2010. Pp. 281, 23·5 × 15·5 cm. Price £23·99/$34·95/€26·95 (paperback; ISBN 978 1 4419 6850 0).

I can imagine the discussions about the title for this book. Actually, it is far less about Go-To telescopes than the title suggests. It gives rather general instructions and tips in a couple of pages, along with more general hints on observing, but anyone who has a problem with a particular instrument will get little assistance. And if you are primarily interested in the planets or other Solar System objects, this book is not for you as no mention is made of any of them, even those where Go-To is helpful. Having said that, most of the book is about some 400 deep-sky objects that you can observe in skies that are less than perfect.

As such, it is an excellent guide. The author has often observed from conditions that he describes as 'exurban' — not the inner suburbs of a great city such as London, but farther out, in the dormitory towns where light pollution is only too evident and still creates a major barrier to observing. Let's face it, these days there are very few objects that you can satisfactorily observe visually from the genuine suburbs, where only a few stars are visible no matter what type of telescope you use. And the good thing is that Neale Monks has observed from both the UK, as far north as Aberdeen, and the southern US, so he is aware of the difficulties of seeing some of the sky's more southerly showpieces and yet has seen them from more suitable latitudes. However, the book does not include objects that are only visible from the southern hemisphere.

For each season, deep-sky objects are organized into three categories — basically, 'Showpiece'; 'Interesting'; and 'Obscure', plus 'Colorful and interesting stars'. His remarks about each one often show evidence of actually having observed them with various apertures under poor conditions. The only relevance to Go-To telescopes is that the author assumes that you can find the object, so he doesn't have to bother with tips for locating it in the first place. He also gives useful and sometimes detailed information about the objects themselves, thus providing something of a replacement for the classic three-volume *Burnham's Celestial Handbook*, though obviously with a much more limited scope. Not every potentially observable object is mentioned, but the choice is clearly arrived at on the basis of experience rather than guesswork.

Deep-sky objects are notoriously dependent on aperture and conditions. Go-To telescopes range in aperture from around 70 mm up to — well, you name it — but most remarks refer to the popular Schmidt–Cassegrain or Maksutov instruments, which are usually of long focal ratio. It's practically impossible in any guide to detail just how visible a particular object will be in all apertures and conditions. I notice the words "should be visible" cropping up, suggesting that in some cases this is what he'd expect rather than what he's seen with a particular aperture — not a criticism, as one can't check every object in all apertures or conditions, though the author does a good job nevertheless. Incidentally, you don't necessarily need a Go-To telescope to make use of this book, as the observing information applies to any telescope.

So if you want a more comprehensive title to this book, it would be 'A Practical Guide to Deep-Sky Objects Observable from the Northern Hemisphere in Less Than Perfect Conditions'. As such it is an excellent, handy and easy-to-use guide, and I'm happy to recommend it. — ROBIN SCAGELL.

A Visual Astronomer's Photographic Guide to the Deep Sky, by Stefan Rumistrzewicz (Springer, Heidelberg), 2011. Pp. 354, 20 × 12·5 cm. Price £40·99/$44·95/€44·95 (paperback; ISBN 978 1 4419 7241 5).

This book, part of the Springer Pocket Field Guide series, is aimed at showing visual observers what they might be able to see with a small telescope. The author, after a brief chapter of introduction to his interest in astronomy, leads on to a discussion of the way that the images in the book were taken. The author used a Watec video camera in place of an eyepiece and then a single image frame was captured. The image was then displayed with no processing. The images were mostly taken through a Celestron C11. The book also contains brief chapters on equipment, observing techniques, and on the classification of deep-sky objects. The bulk of the book, however, is taken up with a catalogue of objects arranged by constellation, with information accompanied by an image. Unfortunately I think this is where the problem lies. I am not sure if it is the reproduction or the quality of the original images, but many images appear to show nothing at all. Some of the images contain white squiggly lines, which I first thought were badly trailed stars only to realize that in fact they were pointers to the object. I guess if one was observing certain objects from the suburbs then you may see nothing and the book would contain an accurate representation of that. The book rounds off with a couple of very short appendices as a bibliography and a list of web sites for further information. I think both of these miss many useful sites and references. Unfortunately I would have to say that this book fails on all counts and given its price I am surprised that Springer has published it. I am afraid I could not recommend this book to anyone. — OWEN BRAZELL.

The Cambridge Atlas of Herschel Objects, by J. Mullaney & W. Tirion (Cambridge University Press), 2011. Pp. 183, 30·5 × 24·5 cm. Price £27·99/$35 (spiralbound; ISBN 978 0 521 13817 8).

In recent times there seems to have been quite an industry devoted to the story of William Herschel and perhaps even more so his sister Caroline. Although most readers will know that Herschel senior discovered 2500 objects, it has been difficult without access to the original *NGC* catalogue with its cross reference to find how they relate to modern designations. The publication of Wolfgang Steinke's book on the history of the *NGC* and the forthcoming publication of Mark Bratton's book on *Observing the Herschel Objects* will, I think, also stimulate the interest in where the Herschel objects are. The last atlas actually to give Herschel numbers was the 16th Edition of *Norton's Star*

Atlas. The rise in enthusiasm for observing Herschel objects as popularized by the Astronomical League (AL) observing clubs in the United States, the Herschel 400, and the Herschel II, has meant that it would be useful to have an atlas showing where these objects are. This thought obviously occurred to James Mullaney, and he and Wil Tirion have created an atlas showing where the Herschel objects are on the sky. James Mullaney was perhaps the first person to try and put together a list of the 650 best Herschel objects, although it was shortened to 400 by the AL. After a short introduction on observing techniques and some of the history, the main section of the book is a set of 31 charts drawn by Tirion showing the whole sky with stars listed down to $7^{m}\cdot5$. The *Atlas* shows objects discovered by both William and John Herschel. This may be confusing to observers who perhaps were expecting just a selection of William Herschel's discoveries. There is also a large appendix, which takes up over a third of the book, relating the Herschel numbers to their *NGC* equivalents so that you can use modern computer star charts. There is also a table of some of the authors' favourite Herschel showpiece objects along with notes on them.

The *Atlas* is spiral bound, in a similar format to the successful *Cambridge Double Star Atlas*. And the objects are marked in blue, which helps when using a red torch to read the maps.

So how does the *Atlas* work? Well, the charts as expected are superbly drawn and very clear and do include a decent selection of deep-sky objects. The introduction to the *Atlas* is also well done and the production excellent. However, I see this as a very specialist publication for those interested in the historical aspects of observing which perhaps will not find itself on every observer's bookshelf. — OWEN BRAZELL.

Advancing Variable Star Astronomy: The Centennial History of the American Association of Variable Star Observers, by T. R. Williams & M. Saladyga (Cambridge University Press), 2011. Pp. 432, 25 × 19 cm. Price £65/$99 (hardback; ISBN 978 0 521 51912 0).

This book does not just cover the history of the AAVSO but also corrects previous histories and covers the formation of the Association. It is split into six main, logical, sections and commences with a Foreword (by Owen Gingerich), a Preface, and Acknowledgements, and concludes with six Appendices, Notes from the various chapters, a Bibliography, and an Index.

It is written in an easy style and there are very few typographical errors although the use of "signal" rather than "single" was noted on two occasions. But — and it is a large but — it is extremely and unnecessarily repetitive, with many chapters commencing by summarizing what had been described in the previous one. Sadly, this only seems to be following the modern trend of television programmes that insist on telling you "what's coming up in today's programme" and "what's still to come"! This is annoying enough on television, but to be found in a book is extremely irritating. Furthermore, this repetition can amount to telling you the same thing, in only slightly different words, in the same paragraph! Another example is the apparent necessity to tell you almost every time they use the words "Long Period Variable" that the abbreviation is LPV. And very strangely, neither Long Period Variable nor LPV appears in the index. I realize this is not a textbook but as LPVs played such an important part in the early history of the AAVSO, I was still surprised not to find them in the index.

A couple of points closer to home are that, although the authors note on p. 204 that the BAA has a President and each observing section a Director, on p. 320 yours truly is described as the Chairman of the BAA VSS. Also, there is no mention at all of the joint AAVSO–BAA Meeting held at Cambridge (UK) in 2008 which formed the 97th Spring Meeting of the AAVSO and is fully reported in their own Annual Report. The book is not without other occasional factual errors, but as with the previous comment, nothing too serious.

So, whilst generally a good read about the foremost variable-star organization in the world at the moment, what the book needs is a thoroughly good editing to remove the repetition and perhaps some of the irrelevant text. — ROGER PICKARD.

OTHER BOOKS RECEIVED

Foundations of Perturbative QCD, by J. Collins (Cambridge University Press), 2011. Pp. 624, 25 × 18 cm. Price £80/$130 (hardbound; ISBN 978 0 521 85533 4).

Directed at graduate students in high-energy physics as well as experienced research scientists, this monograph covers in depth the subject of the strong interaction — quantum chromodynamics (QCD) — emphasizing the links between theory and observation demonstrated by the methods of perturbative QCD.

Solar Polarization 6 (ASP Conference Series, Vol. 437), edited by J. R. Kuhn *et al*. (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 511, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 754 4).

These proceedings carry the presentations at a conference held on Maui, Hawaii, in 2010 June. Topics covered include the physics of polarization, modelling polarization in stellar atmospheres, synthesis and analysis of polarization data, stellar-atmosphere diagnostics — especially relating to the Sun, and observational techniques.

Here and There

A MONKEY WRITES . . .

Made of brass, I estimated the length as six feet, — *Astronomy Now*, 2010 May, p. 20.

MOST UNUSUAL

The geology around Svalbard and Spitzbergen is most unusual. It has fossil evidence of trees that lived 300,000 million years ago, — Exodus Travels, advertising brochure, 2011 April, p. 11.

APRIL FOOL'S DAY EVERY SIX MONTHS

.. the 'great planetary alignment' of 2010 — the circumstance of Saturn and Uranus both being at opposition on September 31, less than a degree apart in the sky, and Saturn being in conjunction with the Sun one day later. — *JBAA*, **121**, 166, 2011.

QUITE A LOT LESS, ACTUALLY

Their previous record [for storing antimatter] stood at 172 microseconds, rather less than a fifth of a second. — *The Daily Telegraph*, 2011 June 11, Science column.