THE OBSERVATORY

Vol. 135 2015 APRIL No. 1245

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2014 October 10 at $16^h 00^m$
in the Geological Society Lecture Theatre, Burlington House

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

The President. The first item of business this afternoon is really an extension of the NAM, where it was my privilege to give out a number of awards for the RAS. Unfortunately, one person was unavailable as he found himself somewhere in Brazil, watching a rather weak football team fail to qualify [laughter]. For John Zarnecki, I'm delighted to read the citation for the Gold Medal in Geophysics.

Professor Zarnecki has been involved in space research for over 30 years. He has been part of the instrument teams — often as Principal Investigator — for many ground-breaking, novel instruments, as well as the associated analysis and interpretation of the resulting data. Professor Zarnecki is part of the team responsible for the *Huygens* lander that touched down on Titan, Saturn's largest moon.

At a distance of 1·5 billion kilometres from Earth, *Huygens* holds the world record for a long-distance landing, and Professor Zarnecki's penetrometer was the first instrument to take readings on Titan's surface. Typical of his ability to communicate with the general public, Professor Zarnecki quipped that this surface was like *crème brûlée*.

At the start of his career, Professor Zarnecki's focus was on X-ray astronomy, establishing that supernova remnants were an important source of cosmic X-rays. His work at British Aerospace led to the production of the *Faint Object Camera*, Europe's contribution to the *Hubble Space Telescope*, that became the longest-serving camera in space in 2002. He led the *Dust Impact Detection System* team for the *Giotto* encounter with Comet Halley and later with Comet Grigg–Skjellerup. Professor Zarnecki's instrumental developments are now being used for the European Space Agency's *ExoMars* programme.

Professor Zarnecki has given long and distinguished service to the European — and more recently — to the UK Space Agencies. Professor Zarnecki served as part of ESA's Senior Review Committee, charged with selecting the scientific themes that would form the basis for the L2 and L3 launches in 2028 and 2034, respectively. He now chairs the Solar System Exploration Working Group, a tribute to his wide-ranging interests in the science of our Solar System. For these reasons Professor Zarnecki is awarded the RAS Gold Medal. [Applause.]

Now on to the main programme. The first speaker is the winner of the Fowler Award for Geophysics, Dr. Alex Copley. He will be presenting: 'Exploring the controls on earthquakes and tectonics: from the plains of India to the greatest mountain range on Earth'.

Dr. A. Copley. I will describe some work that aims to improve our understanding of the deformation of the shallow part of the Earth. This deformation includes what happens in individual earthquakes, and also the larger time- and length-scales involved in the creation and evolution of big mountain ranges such as the Tibetan Plateau. The cool, shallow part of the Earth is known as the lithosphere, which forms the tectonic plates, and is composed of the crust and uppermost mantle. We know remarkably little about the material properties of the lithosphere. The amount of stress released in earthquakes, and the stresses supported by thermally-activated creep in the underlying and hotter ductile part of the lithosphere, are poorly known. However, those material properties are important because they control many of the important features of the Earth's near-surface, such as the locations, magnitudes, and frequencies of earthquakes, and the growth and evolution of mountain ranges and depressions. Work on these topics has clear importance for our understanding of earthquake hazard, and the locations and evolution of natural resources.

My colleagues and I have recently addressed this subject area by examining the region where the Indian plate collides with Asia. By examining the forces required to move the Indian plate in the observed direction and rate, and the forces required to support the mountains in the Tibetan Plateau, we can estimate the total force being transmitted through the lithosphere of India. We can therefore aim to learn more about the material properties of the plate by examining earthquakes in the context of this known force.

The 2001 magnitude-7·6 Bhuj earthquake occurred in NW India. We studied that event by examining the vibrations of the Earth caused by the fault motion, and the permanent ground motions in the epicentral area. By combining those datasets, we can deduce what distribution of rock motion on a fault plane can produce the closest match to the observations. This distribution of rock motion in turn allows us to estimate the change in stress on the fault plane during the earthquake that is required to produce the observed motion. By performing this calculation for the Bhuj earthquake, and two other events within the Indian plate, we can estimate the variation with depth of the stresses involved in moving the faults in earthquakes. We can therefore calculate the total force supported by the earthquake-prone faults. This force is similar to the independently-estimated total force being transmitted through the Indian plate. The agreement provides us with an estimate of the rheology of the Indian plate: the active faults that cut the crust support most of the plate driving forces, and occasionally break in large earthquakes. The total force supported by the lithosphere is large, suggesting that India is a strong tectonic plate.

Studying the Bhuj earthquake also provides the information needed to recognize the effects of repeated active faulting on the landscape of peninsular India. It is therefore possible to recognize other active faults that have not ruptured in the relatively short time period over which we have been monitoring seismic activity, but have produced prehistoric earthquakes. These faults will rupture again in the future. By using the geomorphological 'key' provided by the earthquake and the associated landscape in the Bhuj region, we have identified a series of faults that stretch for hundreds of kilometres across central India and are visible in the landscape. Steep topographic steps running across sedimentary fans deposited by rivers 10 000 years ago (which would originally have had smooth surfaces) can be surveyed, and the slip in past earthquakes reconstructed. We studied one of those faults in detail, and estimated the magnitude of a prehistoric earthquake preserved in the landscape of between 7·8 and 8·4. The multiple faults we have identified, capable of rupturing in these large-magnitude earthquakes, represent a significant earthquake hazard in central India.

We have also studied what happens when the strong lithosphere of the Indian plate collides with Asia, leading to horizontal shortening and vertical thickening and the production of the world's largest mountain range: the Tibetan Plateau. A band of deep earthquakes, distributed under roughly the southern half of the mountain range, shows that India is thrust underneath Tibet due to the convergence between the Indian and Asian Plates. Shallow earthquakes in and around the mountain range show distinct spatial patterns, with the style of deformation changing above the 'nose' of the under-thrusting Indian lithosphere. Numerical models confirm that this spatial separation of deformation style is the result of the presence of strong Indian lithospheric material in the lower crust beneath southern Tibet, which has a large effect on the stress-state of the overlying mountain range. Furthermore, these models demonstrate that the surface motions radial to the strike of the mountain range, which are observed around southern Tibet, are the result of gravity acting to make the mountains spread out under their own weight. Tibet propagates over the rigid Indian plate much like honey over a sheet of glass. The strong Indian lithosphere has also had a dramatic effect on the large-scale shape of the Tibetan Plateau, with the distinctive flat top and steep edge of the mountain range being the result of the evolution of deformation being governed by the strong underlying Indian lithosphere.

Eastern Tibet is radically different to western and central Tibet. Topographic slopes on the edges of the mountain range are gentle, and the style of deformation in earthquakes is different from the plateau margins further west. This fundamental difference is due to the lack of strong lithosphere being thrust beneath the mountain range, as is the case where India underthrusts western and central Tibet. This absence of strong material in the lower lithosphere changes the stress-state in the range, which leads to a different style of active deformation, and more gentle topographic slopes.

In summary, by using a combination of seismology, geodesy, fieldwork, fluid-dynamic modelling, and thermal modelling, we have been able to provide new insights into the controls on the deformation in the India–Asia collision zone, and on the continents in general. Strong faults that cut the crust of the Indian plate support the majority of the forces being transmitted through the plate, occasionally break in large earthquakes, and can be recognized in the landscape. Where the strong lithosphere of India underthrusts the Tibetan Plateau it controls the distribution of deformation and the large-scale evolution of the mountain range. The dramatic difference between eastern and western Tibet is due to the absence of such underthrusting beneath the eastern margin of the mountain range.

The President. We have time for a few questions.

Professor D. Lynden-Bell. Do you have a feeling for whether Everest is going to be higher than K2 forever, or not?

Dr. Copley. Once you start talking about small length scales, life gets very complicated. You have the competing effects of what I have been talking about, as well as local effects in terms of slope gradients and erosion. In a sense it probably comes down to how the climate varies in the central Himalayas compared to the northwestern Himalayas. That's something I don't really know much about.

Professor Kathy Whaler. Presumably the results would be different if the collision rates were slower? In that case, you'd have more chance for the Indian lithosphere to warm up.

Dr. Copley. Yes, exactly. The distance underneath the Tibetan plateau to which the Indian lithosphere retains its strength entirely depends on the thermal structure of the stuff around it and the rate at which it's going underneath. If the collision rate is slower, then you'd expect this strong material to heat up much closer to the mountain front as it's going underneath. The lateral position of that rigid nose would move back towards the mountain front.

Professor Whaler. An extra comment: I couldn't help but notice that along the fault you showed there was a reservoir. Should that be there?

Dr. Copley. That's a bit of a political question for someone wanting to continue working in India [laughter]. It's worth thinking very carefully about large infrastructure projects in an area where you know you can produce magnitude-8 earthquakes.

Mr. M. F. Osmaston. Have you taken into account that around southern India the geoid-determining satellite has found the deepest dent in the geoid to be 150 m? This creates quite a problem as to what's pushing it.

Dr. Copley. We tried to calculate what was driving India's northwards motion. It's a combination of the large subduction zones underneath Sumatra, which are driving the plate northwards, and also the line of mid-ocean ridges around the Indian Ocean. They are higher than the sea floor so they exert a pressure northwards. It's a combination of those two factors that provide the northwards push on the Indian plate.

Mr. M. Hepburn. In the Alps, the continental geophysicists made a model where things go right over and around. That is a very natural explanation as to why mountains get bigger, because there is just more stuff there. Our models tend to be based on something going underneath. Can you tell the difference between the models?

Dr. Copley. In the Himalayas, what you see is a complicated mixture of both those effects. You have stacks of thrust sheets where you have thrust faults active in many strands at different points in time. Also, you see folding between them. So, we actually have both mechanisms in the Himalayas.

The President. Thank you, Alex, for an excellent talk. [Applause.]

It's a great pleasure now to introduce my opposite number from the German Astronomical Society, Professor Andreas Burkert from Munich. He will be talking about 'Watching a small gas cloud on its way into the central super-massive black hole of the Milky Way'.

Professor A. Burkert. It gives me great pleasure to talk to you about something which is more a puzzle than a solution. I want to describe what we think is going on at the centre of our Galaxy.

The presence of gas and dust clouds in front obscure the actual Galactic Centre, preventing us from seeing what is going on in the visible régime. It was Karl Jansky who first detected a signal from the Galactic Centre using his radio telescope and it told us that we could use long-wavelength radiation to see further into that region. In the infrared the Galactic Centre is brighter than the Moon. We find a large disc of cold molecular material of about 100 million solar masses of gas occupying the central 400 parsecs. Once in a while, *i.e.*, every million years, one of the molecular clouds in the disc falls into its central region, which contains a radio shell of gas at 10^6 K that we think was produced by 50 to 100 supernovae all going off together. Within this hot bubble we observe a ring of cold, atomic gas which might have been produced by one of the infalling gas clouds that was tidally disrupted, distributing its gas along its orbit. Streamers of gas from this ring fall further inwards, towards the very centre of the Galaxy. On their way inwards they accelerate to a velocity of about 1000 km s^{-1}. By using infrared adaptive optics we can see individual stars in the centre. In the inner region, 1 parsec in diameter, there are more than 2 million stars — compare this to the solar neighbourhood where stars are typically one parsec apart. This star cluster is slowly rotating in the same direction as the whole galaxy but there is a large range of ages, covering the age of the whole Galaxy. The cluster also has the same mass as the black hole at the Galactic Centre so we speculate that they might have a coordinated evolution. We can work out the age distribution of the stars which leads to the accretion history of the central black hole.

Embedded in this star cluster in its innermost region with a linear diameter of 0·05 pc lies a cluster of massive B stars which are about 4 million years old, and they form a spheroidal distribution whose origin is a mystery. If we now focus on this innermost star cluster we can see those stars moving around the Galactic Centre in real time and with velocities up to 2000 km s^{-1}. We have been able to follow one star around a complete orbit and can calculate that it is orbiting around something with a mass of $4{\cdot}3 \times 10^6$ solar masses. This is our Galaxy's super-massive black hole. As we know its mass we also know the Bondi radius inside which nothing can form. That radius is exactly the size of the inner star cluster so here is a contradiction. How do those stars get there? They can't be brought in from outside and slowed down and they cannot form *in situ*. In addition, around the B-star cluster is a ring of about 50 O stars and the inner edge of their distribution is exactly the Bondi radius. The question is, what does this rather peculiar geometry tell us about the formation history? We do not know the answer yet.

Black holes, when accreting gas, are amongst the intrinsically brightest objects in the Universe so why can't we see ours? We believe that it might be because inside the Bondi radius the gas is rather hot, with temperatures of 10^8 K, which is what Jansky was seeing, and this may be the reason why the black hole cannot accrete. Recently, Reinhard Genzel brought to my attention an object called G2, which he had found moving towards the black hole at 1000–2000 km s^{-1}. A spectrum revealed that it has a temperature much lower than a typical star. It is an ionized gas cloud and the dust temperature is 600 K whilst the hydrogen gas is at 10^4 K and we think it has about three Earth masses of material and is moving in a highly eccentric orbit with a period of about 400 years. We have made a model of how we think it will evolve during its passage around the black hole over the coming months and it appears that it will be torn apart and spread out into a thin string of spaghetti on the other side of the black hole.

We want to know what happens to the black hole, and whether the cloud is the evaporated remains of a star. There is a significant amount of observing time being spent on watching the passage of G2. Perhaps in the next two years an accretion disc will form a round the black hole and it will become active. We have a problem because any activity generated might be short-lived and we cannot monitor the events all the time.

The President. I'm interested — is anybody else?

Dr. K. Smith. If you've got a series of little gas clouds going around, I'm just thinking of an analogy with Shoemaker–Levy 9 when it went past Jupiter. Could there be a second pass?

Professor Burkert. The problem is that if it's a gaseous component, it will just be torn into a kind of gas spaghetti. It's not solid material and the super-massive black hole has much too strong a gravitational field to allow it to become compact after pericentre passage and go around a second time — at least in all the simulations we're doing.

Dr. Smith. Even if it's solid on the first pass, a star for example?

Professor Burkert. It's highly unlikely it's a star. We should see the star as a compact central source of radiation within G2. It would also have had to form in the last 1000 years.

Professor M. J. Ward. Is there a prediction that we will see radio emission — not attenuated by dust if it is radio? If so, could we persuade you to hang on until *SKA* is available [laughter]?

Professor Burkert. There are predictions of all kinds, depending on the model you use. There will be radio emission from accretion, but it may not be visible due to all the hot gas in the surroundings. This will depend on the amount and rate of accretion. We think there should be a factor of two to three increase in the luminosity when focussed on the central region. However, you don't know when it will happen and time-scales are very short.

Reverend G. Barber. How far out is the pericentre relative to the event horizon?

Professor Burkert. The pericentre is 2000 Schwarzschild radii away. Once the gas loses angular momentum and energy, it goes very close though.

Professor P. G. Murdin. Are there any other examples of gas clouds of this size isolated anywhere in the Galaxy?

Professor Burkert. No, but we expect probably someone has seen something so that's why we call it G2! [Laughter.]

The President. I think that's probably a good note to end it on! Thank you very much again, Andreas. [Applause.] It's a great pleasure introduce the Past President now, for the 2014 Presidential Address, which was postponed from the AGM. It's very nice to hand over to David Southwood: 'The way we live now: space science and politics'.

Professor D. Southwood. [A summary of this talk appears in *A&G* for 2014 December.]

The President. Thank you David. [Applause.] Sadly, we don't really have time for questions. I'd just like to thank all our contributors again for a really great afternoon. [Applause.] A final reminder that we have the usual drinks reception over in the RAS library and the next monthly A&G meeting of the society will be on Friday 2014 November 14th.

HOW THE UNIVERSE EVOLVED FROM SMOOTH TO LUMPY

By Eliot Quataert
University of California at Berkeley

[The Halley Lecture for 2014, delivered in Oxford on 2014 June 10]

The Milky Way galaxy is where we live. It is 100 000 light years in diameter, about 10 million times the diameter of the Solar System. It contains 100 billion stars and has a mass of about a trillion Suns and most of this mass is in the form of dark matter — some kind of fundamental particle which is not gas or stars. Dark matter plays a very significant role in the story of the evolution and history of the Universe. Our nearest galactic neighbour is M 31 in Andromeda which is 2·5 million light years away, and in 4 billion years our Galaxy will slam into it. This will not matter greatly to us since by that time the Sun will have long since expanded into a red giant.

Galaxies are often found in groups or clusters which are held together by the original dark matter. We can use the *Hubble Space Telescope* to concentrate on a tiny area of sky and observe it for a very long time; this enables us to zoom in and see the details of about 10 000 particular galaxies. These are both very faint and very distant and we see them as they were about 12 billion years ago when the Universe was much younger than it is today. This type of astronomy was pioneered in the 1920s by Edwin Hubble who measured the way in which distant galaxies were moving with respect to the Milky Way. What he found was that all the galaxies seemed to be moving away from us, although that is not a peculiar property of the Milky Way. We believe that all galaxies are moving away from each other.

One of the great goals in astronomy since the discovery of the expansion of the Universe has been to understand and determine the history of that expansion. Were the distances between galaxies always expanding at the same rate or did they expand faster or slower in the past? This was only resolved in the late 1990s when it was found that the expansion of the Universe was accelerating. We do not understand it but have ascribed the cause of this expansion to the existence of dark energy. This is completely different to the dark matter already mentioned and it leaves little room for the normal stuff of which we are made, such as the iron in our blood, or the oxygen that we breathe, for example. The next big clue to the existence of dark energy came from pictures of stars taken with an infrared telescope, orbiting around the Earth. We also look at the sky in X-rays and the point sources that we see may represent gas spiralling into black holes and generating huge amounts of radiation.

The picture of the sky which turns out to be the most important when thinking about our origins was taken by a radio telescope in the microwave region. This corresponds to a source of radiation at a particular temperature of 2·7 K or –454 degrees Fahrenheit. This is, in fact, a signature of the expansion of the Universe. If we project the expansion back in time, we find that things in the distant past are much closer together and much denser and the Universe was much smaller and much hotter. We think that this cosmic microwave background (CMB) radiation is indeed the glow of the early Universe, and as the Universe has expanded it has cooled to produce the microwave background that we observe today. First detected by Penzias and Wilson in the 1960s, since then the study of the sky in microwaves has been one of the most important areas of astronomy and has answered many detailed questions about the energy and mass content of the Universe, about the expansion of the Universe, and about the properties of the Universe when it was much younger.

Our best pictures of what the early Universe looked like were taken with the *Planck* satellite a few years ago. It was able to show the tiny fluctuations in the level of the CMB over the sky at the 0·001% level. We now interpret differences in this kind of thermal radiation produced by hot objects, and it tells us that the early Universe was essentially smooth to the level of 0·001%. That time was about 380 000 years after the Big Bang which we refer to as T_0, the point at which the Universe was so dense and hot that the laws of physics broke down; we do not know what happened before then. Today the variation in density and temperature amongst ordinary matter on the Earth or in the Sun is many orders of magnitude greater than that in the early Universe, so how did we get to this point?

There is a simple answer as to how the change happened and that is gravity. It is the dominant force in the large-scale Universe; the other forces — electromagnetic and strong and weak nuclear — pale in comparison beside it. Gravity takes the small differences in the CMB and makes them bigger with time. In a region where there is more material gravity is stronger, which in turn pulls in surrounding material and so the region becomes more massive still. This is called gravitational instability and was studied by James Jeans in the 1900s. The greatest effect on the irregularities, however, is dark matter because it is dominant. Dark energy, on the other hand, is relatively smooth and does not affect the clumping process.

Galaxy formation occurs when the gravitational force from the dark-matter clumps overcomes the expansion of material. Initially the first galaxies are quite small, maybe consisting of just a few stars, but as time goes on they get bigger. In the Milky Way the ordinary matter — gas, planets, stars — is confined to the central part whilst the dark matter forms a huge halo around this. Regions that have lots of dark matter have enough gravitational pull to bring in dust and gas from the surroundings and build galaxies. In a very real sense when we look at the distribution of galaxies we are looking at the distribution of dark matter.

That is not the whole story, however. When we look at the properties of galaxies, how big and how massive they are, for example, it turns out that they are related in a rather complicated way to the properties of the dark matter around them. One way to illustrate this is to look at the distribution of dark-matter clumps as a function of number *versus* mass. Regions with lots of dark matter are rare. We want to know what drives the variations in the size and shape of galaxies that we see. What sets the mass of the galaxies relative to the dark matter?

I want to emphasize two key questions. One is trying to understand more about how gas gets into galaxies, because that determines how the mass of normal matter grows with time. Secondly, what happens once you actually start forming stars and black holes in the centre of galaxies — how does this change the state of gravity pulling dark matter in. There are two major ways in which galaxies grow with mass in time. One happens when galaxies slam into each other. The *HST* shows a number of examples of this and the result is a complicated distribution of matter. The other way is due to galaxies pulling in surrounding gas to the central part of the galaxy, and eventually it turns into stars. People have been working for the last five to ten years to find which of these processes predominates, but trying to observe gas flowing into galaxies is a very challenging observation. We also need to understand what is the behaviour of the gas in the dark-matter halo — how does it get to the centre of the galaxy? How does it cool to form stars? Work done in the last few years by Steven Balbus and me has shown that the gas does not flow smoothly, rather it can be likened to water boiling — the flow is turbulent. How does this motion affect star formation at the very central regions of galaxies where the stars form? It's not just a case that the mass of stars equals the mass of dark matter. It is a complicated relationship between the distribution of the groups of galaxies that we see and the properties of dark matter. That is a challenge that the modern theory of star formation is trying to solve.

The most important question in our modern understanding of galaxy formation is trying to understand, both observationally and theoretically, what happens once stars and black holes start forming. This leads to feedback as the stars affect the gas — which, once it gets into galaxies, is affected by processes other than gravity. Once inside the galaxy the dominant effects on the gas are stellar explosions and the effect of black holes in the environment, with the result that 90% of the incoming gas is later blown out again by the combined effects of supernovae and black holes. Pictures with the *Chandra* X-ray telescope show that there are dramatic outflows of gas.

Why is it that star formation has such an effect on the gas? The process of forming stars actually dumps energy back into the surroundings and this can happen in two ways. Firstly, stars produce enormous amounts of light and heat which are transferred to the gas. Secondly, massive stars explode at the end of their lives after they collapse to form a neutron star or black hole. This process releases an enormous amount of energy. If we take a certain amount of gas and turn it into stars we get about 0·01% mc^2 back out in the form of radiation and stellar explosions. This amount of energy pales in comparison with that which we get out of a black hole, since turning gas into a black hole is a very much more efficient process of getting energy out of massive stars — about 100 times more efficient. That happens as gas falls in to form a black hole: it orbits around getting hotter and hotter and releases about 10% of mc^2. Some is light and some powerful outflows which carry away energy of motion in relativistic particles. This energy is carried away in several forms. It is one of the real surprises in our thinking about how galaxies form. Over the last ten years there has been the recognition that black holes at the centre of galaxies are actually important for that process. The mass of the black hole is about 100 000 times smaller than the mass of the surrounding galaxy and the black hole is about 100 $R_{\odot}$ in diameter, so as a volume it is ten billion times smaller than the parent galaxy. How can such a small volume, which is completely negligible in size and mass, have such a large effect? We think that the amount of gravity produced by gas falling into a black hole produces a force on that gas greater than the gravity in all the rest of the galaxy including the dark matter. However, it only takes a tiny fraction of energy to push gas entirely out of a galaxy.

Our modern picture of a galactic life cycle is one in which gravity pulls matter into the galaxy, gravity causes matter to collapse and form stars, which then undergo fusion for millions of years and end their lives as white dwarfs, neutron stars, or black holes before gas is blown out again and captured once more, to start the next cycle. Carl Sagan said that we are the stuff of stars. That is correct but it is really more appropriate to say we are the stuff of galaxies. So the secret of understanding how the Milky Way was built is to understand the interplay of all these processes. A lot of effort is going into understanding how gas gets blown out of galaxies, how it comes back in, and trying to combine those processes to form realistic simulations.

Only in the last few years have simulations been able to explain reliably why galaxies have roughly the masses that they do. Some of the things that we need to understand are what makes galaxies disc-like rather than elliptical or spherical. That remains a rather thorny theoretical problem.

VINTAGE PLASMA INSTABILITIES

By I. Lerche
Institut für Geowissenschaften, Martin-Luther Universität Halle

I gave a short talk at an international meeting (Kinetic Processes in Plasma: Instabilities, Turbulence and Transport, in 2010 November at the Ruhr-Universität, Germany) on four historical plasma instabilities that have since proven to be of major significance in astrophysics. This paper provides the written version of that talk, discussing briefly the Buneman two-stream instability, the Harris instability, the Weibel instability, and the cosmic-ray instability. Some suggestions for further research were also given in the talk and they are also presented here in the hope that the challenge to address the problems will soon be undertaken if it has not already been so done.

Introduction

I would like to discuss briefly a pot-pourri of four basic plasma instabilities that were first studied over 50 years ago. The reason for this discussion of such vintage problems is that they have proven to be of inordinate relevance in the understanding of plasma processes in astrophysical situations. While I will not attempt to detail the many astrophysical applications, instead taking the lazy way out of suggesting that the reader research the literature for such, I will concentrate on the basic understanding that arose as a consequence of the early work on those plasma instabilities.

It is always easy to use information of the last half-century or so to see why one had to have such instabilities but, if one casts one's mind back half a century, the struggles to develop and understand the instabilities on the basis of then-available information made it clear that we were only scratching the surface of major later developments. For instance, 50 years ago we knew there was a Galactic magnetic field of about 3 microgauss but we had virtually no knowledge of the turbulent component of the field. So the intrinsic understanding of the instabilities has been modified many times since the original publications. I am not concerned here with such modifications, although in truth such have often been pivotal to providing a sustainable marriage between observations and theories.

Again, many of the original discussions of the instabilities were performed as though the plasmas in question were non-relativistic, and later generalizations to include relativistic effects often changed, sometimes in major ways, a basic instability. It is these generalizations that have, most often, been most effective in increasing our understanding of astrophysical situations. Apart from the cosmic-ray instability (which intrinsically requires the cosmic-ray-plasma component be relativistic in accord with measurements), only brief mentions will be given of such generalizations. But without a firm historical basis one is left without the requisite tools and methods needed to effect the generalizations needed.

As the years have advanced, those who can remember the players involved on a personal level tend to become scarcer. I still, however, have memories of how the developments went, personal knowledge of some of the authors, and also how some of the comments went when papers were first published — and not all such comments were kind. So, for the generations of researchers younger than I am, here is what we had in those days — and remember that the struggles then to understand are equally valid today albeit with enormous amounts of data now.

The four instabilities I will discuss date between 1958 and 1967. In sequence I will consider briefly: (*i*) the Buneman two-stream instability; (*ii*) the Harris instability; (*iii*) the Weibel instability; and (*iv*) the cosmic-ray instability.

The Buneman two-stream instability

Buneman[1] addressed the following question: if, in one dimension, a collisionless, charge-neutral, non-relativistic plasma is composed of cold ions and some cold electrons, with the rest of the electrons forming a beam that moves through the cold plasma with constant velocity v and in the absence of an ambient magnetic field (Fig. 1), then waves are excited in the plasma due to the electrostatic interaction between the particles. Are these waves stable or not? With a dependence for the perturbations of the form $\exp(i(kx–\omega t))$ Buneman obtained a dispersion relation between k and ω of the form

$$(\omega_{pi} / \omega)^2 + (\omega_{pe} / (\omega–kv))^2 = 1 \qquad (1)$$

which can also be written as

$$k^2 = (\omega_{pi} / a)^2 + (\omega_{pe} / (a–v))^2 \qquad (2)$$

where ω_{pi} and ω_{pe} are the ion and electron plasma frequencies, respectively, and a is the wave phase velocity $a = \omega/k$. Solutions to equation (1) yield a most unstable solution in the form

$$\omega = \omega_r + i\gamma$$

with

$$\omega_r = 2^{-4/3} (m_i / m_e)^{1/6} \omega_{pi}; \gamma = 3^{1/2} \omega_r \text{ and } k \approx \omega_{pe} / v \qquad (3)$$

where m_i (m_e) is the ion (electron) mass. The instability rate is rapid — at about the ion plasma frequency — so it influences the beam plasma in a very short time (Fig. 2). Buneman[2] pointed out that a warm plasma will tend to quench the instability by Landau damping unless the bulk speed v is somewhat in excess of the thermal speed.

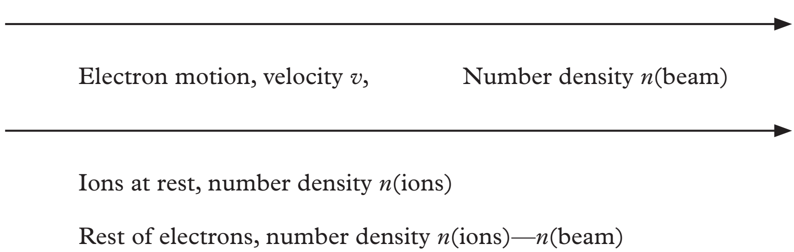

FIG. 1

Sketch of the plasma situation envisaged by Buneman[1].

Relaxation of the intrinsic assumptions leads to many factors influencing the original Buneman results. For instance, if one were to do the same calculation relativistically, then a resonance denominator in the form $(a–v)^{-2}$ again appears, but now particle speeds v are restricted to be in the domain $v<c$, whereas there is no such restriction on wave phase speeds. Thus any waves with phase speeds $a>c$ cannot resonate with the particles and so do not damp or grow (at least such is true at linear order) and are not subject to Landau damping. This effect leads to a major change in the Buneman instability behaviour. Or again, the original calculations envisaged only a single electron beam moving through an otherwise stationary plasma. The introduction of multiple beams changes drastically the instability behaviour, as does the influence of a direct collision frequency. If one moves away from the constraint of a one-dimensional plasma and considers a three-dimensional warm-plasma system including an ambient magnetic field, then the simplicity of the original Buneman calculations becomes mired in complex mathematics. Such more-general situations have kept many a plasma astrophysicist happily employed over the last half-century or more. A discussion of such effects is well beyond the scope of this paper and so will not be considered further here.

One can leave this instability with the following unanswered questions (at least as far as I know): what happens with multiple beams? How does one take

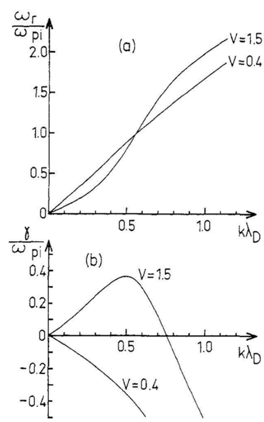

FIG. 2

Dispersion relations showing the real (*a*) and imaginary (*b*) parts of the frequency normalized to the ion plasma frequency as functions of the wavenumber normalized to the Debye wavenumber for a hydrogen plasma. When $v = 0{\cdot}4$ the dispersion and damping are those of an ion acoustic wave, while for $v = 1{\cdot}5$ the behaviour is that of the Buneman instability. (For further details of this example see Tautz & Lerche[3].)

the limit of many beams (n) with n approaching infinity and when the beams are in all directions? One should presumably arrive at a stable system if the beams eventually yield a Maxwellian distribution. Does one, and under what conditions?

The Harris instability

In the presence of a uniform magnetic field, discussion of plasma waves propagating in arbitrary directions to the field tends to be fraught with mind-boggling complex mathematical problems. First, there is now coupling of electrostatic and electromagnetic fields; second, particles tend to gyrate around the uniform field at their gyrofrequencies. The upshot is that one is asked to solve a 3×3 determinant for the relations between frequencies and wavenumbers, and each entry in the determinant is made up of an infinite series of terms, and each term contains resonant denominators of the form $(\omega - v_{\|}k_{\|} - n\Omega)$ (here $v_{\|}$ and $k_{\|}$ are the velocity component and wavenumber component parallel to the uniform magnetic field; Ω is the gyrofrequency for a particular charge component of the plasma), and each such denominator factor is present for both ions and electrons, leading to a sum over particle species as well as over the integer n $(0 < n < \infty)$. To date there has been no full analytical solution of the determinant for arbitrary propagation directions, although there are to be sure approximate behaviours known. Some simplifications are possible to the determinant because there are known methods[4] of performing in closed form the summations over n occurring in the determinant. However, while such procedures bring some clarification to the problem, to this day the difficulty remains of obtaining general solutions. And when Harris[5] was involved with the problem that now bears his name, the summation methods were a good seven years in the future and so massive effort had to be expended.

Considerable simplification to the general dispersion relation can be achieved if one restricts attention to waves that propagate exactly parallel or exactly perpendicular to the ambient magnetic field. Harris[5] chose to look at electrostatic waves propagating exactly normal to the ambient field in a non-relativistic plasma with cold immobile ions, for which the resonant denominator reduces to $(\omega - n\Omega)$ and the summation is only over the electron species. Even then no exact solution was generally available, but Harris invoked a so-called neutral-point method to obtain low-frequency solutions. Basically Harris took the Maxwell operator

$$\Lambda_{ij} = (kc/\omega)^2(k_ik_j/k^2 - \delta_{ij}) + \delta_{ij} + 4\pi i\sigma_{ij}/\omega, \tag{4}$$

where σ_{ij} is the conductivity tensor and where zeros of $\det\Lambda_{ij}$ correspond to the dispersion relations for waves in the form $\omega(k)$, and expanded the determinant around $\omega = 0$. After some considerable mathematical manipulations Harris was able to reduce the perpendicular dispersion relation to the form

$$F(k_\perp) + E(k_\perp)\,\omega^2 = 0 \tag{5}$$

valid at low frequencies. Here $F(k_\perp)$ and $E(k_\perp)$ are extremely complicated expressions that involve the distribution function for the electron velocity. One can also write

$$\omega^2 = -F(k_\perp)/E(k_\perp). \tag{6}$$

Harris then proceeded on two fronts at once. First he noted that equation (6) admits solutions for which either $\omega^2 > 0$ (representing real frequencies) or for which $\omega^2 < 0$ (representing aperiodic unstable modes with no propagating component, unlike the Buneman modes). Second, Harris noted that if a chosen distribution function allowed specific real positive values of $k_\perp$ (say $k^\star$) for which $F(k^\star) = 0$, then to one side or the other of $k^\star$ there had to be an aperiodic mode that was unstable (the sole exception being if $E(k_\perp)$ also enjoyed a zero at $k^\star$). And such a zero for F would then allow one indeed to claim a low-frequency aperiodic mode, thus validating the intrinsic assumption that an expansion around $\omega = 0$ was acceptable (Fig. 3). Harris also noted that if there were no zero in F/E one could still have an aperiodic unstable mode as long as $F^{\star}E > 0$.

One of the major surprises with this analysis came about when one inserted specific distribution functions and evaluated E and F. It turned out that some

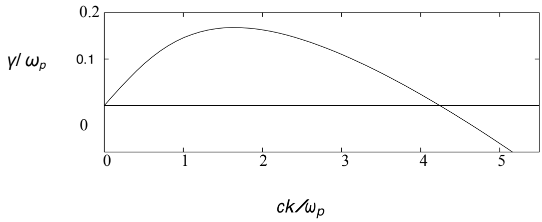

FIG. 3

Sketch of the Harris instability rate (in units of the plasma frequency) as a function of the wavenumber (in units of the plasma frequency/c). Further discussion of this example is available in Tautz & Lerche[6].

distribution functions indeed gave rise to aperiodic modes while others did not. There seems to be no rhyme nor reason as to why such is the case, nor does there seem to be any procedure available to determine (ahead of doing the heavy-duty calculations for F and E) which distribution functions will lead to aperiodic modes and which not. It also turned out that some distribution functions gave rise to neutral points (*i.e.*, values of $k^{\star}$ where $F = 0$) while others did not, but both situations could lead to aperiodic unstable situations or not, as the case may be, depending on the distribution-function choice. This situation persists to the present day and is indeed most unsatisfactory — a general rule is not to hand.

However, the major point of the analysis is that when one has an instability in the Harris sense (*i.e.*, modes varying perpendicularly to an ambient magnetic field) then the instabilities grow in place without propagating, a characteristic shared with the Weibel instability. Thus one has at least two mode types that can lead to strong local disturbances.

As might have been anticipated, modifications to the original Harris work have been undertaken: the inclusion of finite-mass ions that are also mobile, fully relativistic effects, longitudinal and transverse mode coupling, but all done within the spirit of the original Harris work, *i.e.*, expand the Maxwell operator around $\omega = 0$ so that one can see how the generalizations modify the original work which is used as a template. But the same uncertainties still exist on which distribution functions will allow aperiodic modes. There seems to be no other recourse than to work through each particular situation — a most annoying state of affairs. Perhaps this discussion will stimulate some student to see if there are general criteria that can be invoked!

One further aspect of the inclusion of relativistic effects is mentioned here. The resonant denominator becomes $(\omega\gamma - n\Omega)$ where γ is the total energy of a particle in units of rest-mass energy. Note first that if one deals with a *non-relativistic* plasma ($\gamma = 1$) then the resonant denominator has a true singularity every time ω crosses a multiple of Ω. When one allows for the relativistic effect of the particle energy ($\gamma \neq 1$) then at $\gamma = n\Omega/\omega$ there is a resonant singularity. Because $\gamma \geq 1$ one requires $n \geq \omega/\Omega$. The resonance is then capable of producing a resonant instability with the usual real and imaginary parts to the complex frequency ω. Whether the plasma is unstable or not then depends on the choice of distribution function. So the Harris instability *per se* produces aperiodic disturbances that can be unstable while the relativistic resonance produces propagating but unstable disturbances. How one morphs into the other (or indeed *if* one morphs into the other) is as yet unknown — so another problem for aspiring students!

The Weibel instability

One of the classic introductions to magnetic fields and currents for students is to have them draw the magnetic-field lines around two current-carrying wires when the currents are in the same or opposite directions. For like-directed currents there is then a weak zone between the wires where the fields cancel and so the wires feel a force trying to push them together, while for unlike currents the field between the wires is strengthened and so the wires feel a repulsive force (Fig. 4).

Weibel[7] noted that one can view a plasma with an asymmetry (in the sense of no bulk speed per charge species but rather a pressure difference in different directions) as a bunch of current-carrying 'wires', but in this case the plasma

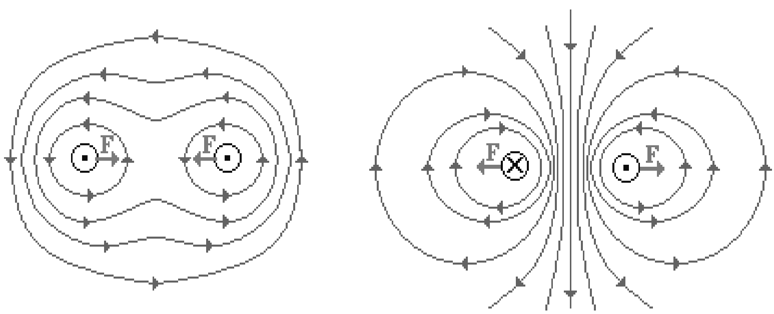

FIG. 4

Sketches of the magnetic field around two current-carrying wires when the currents are in the same and opposite directions.

system is free to move and so the excess current plasma components tend to merge and so create filaments; the filaments in turn tend to merge if they are like, and so produce magnetic fields that are increased in strength that, again in turn, attract even more like current filaments. This on-going process is sometimes referred to as the filamentation instability and is a highly non-linear instability that is difficult to saturate. (A similar process occurs in geology where fractures in rocks tend to merge if their stress patterns are like and so produce a larger fracture that attracts even more fractures with the same sense of stress pattern, with the final upshot that one ultimately creates a very large fault running through a sedimentary sequence.)

For small perturbations the result is a dispersion relation in the form

$$\omega^2 \propto k^2 - K^2 \tag{7}$$

where K is a constant related to the plasma parameters but especially is proportional to the degree of anisotropy: no anisotropy means $K = 0$ so ω is then real. Thus only in the long-wavelength domain $k < K$ does one have an

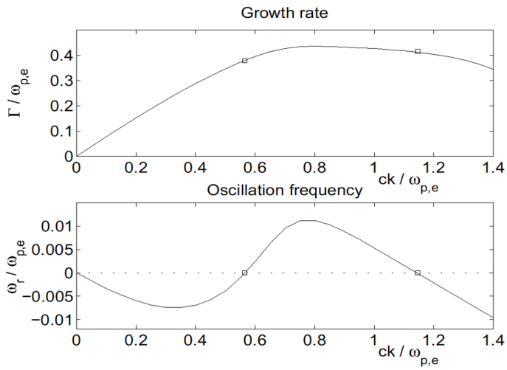

FIG. 5

Sketches of the growth rate and oscillation frequency for the Weibel instability (both in units of the electron plasma frequency) as functions of the wavenumber (in units of the electron plasma frequency/c). The small squares mark the wavenumber locations where the oscillation frequency is zero and so yielding aperiodic mode behaviour. Further discussion of this illustration is available in Tautz & Lerche[8].

aperiodic mode (Fig. 5). And this régime is precisely where one wishes to have an aperiodic mode, for then one is dealing with a disturbance that covers a broad physical domain encompassing many 'filaments'.

Now, as the perturbations grow with time, the linear analysis becomes invalid, and one needs to address the fully nonlinear self-consistent problem and so deal with finite-amplitude disturbances. In addition one needs to include relativistic effects and multiple species of charged particles plus a background magnetic field. The importance of including such factors is manifest. Perhaps the dissipation of collisionless shocks and magnetic-field self-generation are relevant areas of interest in astrophysics. The behaviour of fully self-consistent Weibel modes and their saturation, together with the emission of particle radiation from electrons trapped in such a situation, are needed if one is to use the observed emission to infer the characteristics of the modes. While highly non-linear aspects are urgently needed to apply Weibel-mode behaviour to astrophysical conditions, it is already clear from work done over the last 50 years that such effects can have profound impacts on our understanding of objects in the astrophysical theatre, as is also true for the aperiodic Harris instability. I can only encourage the modern generation of students to get to work on those problems!

The cosmic-ray instability

Set to one side current information we have about cosmic rays and concentrate now on what was known some 50 years or so ago. At that time it was said that supernovae were the source of the majority of the baryonic (mainly hydrogen) cosmic rays seen at Earth. We also knew from the secondary-to-primary ratio of such cosmic rays that they did not live longer than about a million years in the Galactic disc. That fact becomes important later. Now, we also measured cosmic rays at the Earth and found that they were isotropically received from all directions. However, if supernovae were indeed the sources of cosmic rays, then one would anticipate increased fluxes of cosmic rays from the directions of known supernovae remnants — something that was not observed. Including the presence of the known Galactic background magnetic field of about 3 microgauss would imply that cosmic rays should stream without hindrance along the field lines, so producing a large anomaly in arrival directions at Earth — something that also was not observed. So one was left in a quandary: either supernovae had nothing to do with the observed cosmic rays or something was making the cosmic rays isotropic on their journey through the interstellar medium to Earth. However, from radio measurements of synchrotron emission from supernovae one inferred that electrons were being accelerated to high energy and there was no reason to suppose that a supernova did not also produce high-energy baryons. We also knew from polarization measurements of supernovae that the magnetic field varied spatially, at least in the local neighbourhood of a supernova (although the small size of supernovae remnants in comparison to the distances between the supernovae and Earth did not allow us to say anything then about any fluctuating component of the Galactic magnetic field — and how things have changed in the last half-century!).

So one needed a mechanism that would make cosmic rays isotropic, and it had to operate on a time-scale much shorter that the million-year lifetime of the cosmic rays in the disc of the Galaxy. Viewed as a plasma one then had interstellar hydrogen (of about 1 particle/cm^3) together with the highly-energetic cosmic-ray component (of about 10^{-10} hydrogen ions/cm^3) and all embedded in a uniform magnetic field of around 3 microgauss. Ignoring the cosmic-ray component meant that any electromagnetic disturbances in the relatively cold interstellar plasma would produce waves travelling along the magnetic field at the Alfvén speed, V_A with $\omega = k V_A$ for the real part of the frequency. Including the cosmic rays and allowing them to be anisotropic (in order to see whether such an anisotropy could be removed by instabilities in a time short compared to the million-year limit) meant that one had a dispersion relation for the parallel propagating Alfvén waves for which the real part was controlled by the cold plasma and the imaginary part by the cosmic rays. After some complex calculations one had a contribution to the dispersion relation that had as its core the expression N/D where

$$N = [\omega\ (1+p^2)^{1/2} + ck(p_\perp \partial f/\partial p_\| - p_\| \partial f/\partial p_\perp)] \tag{8a}$$

and

$$D = [\omega\ (1+p^2)^{1/2} + \Omega - ck\ p_\|] \tag{8b}$$

with f the cosmic-ray-distribution function, $p_\perp$ ($p_\|$) the momentum component perpendicular (parallel) to the ambient magnetic field, and where momentum is measured in units of mc with m the rest mass of a cosmic ray. For an Alfvén speed $V_A \ll c$ one has a resonance (determined by the zeros of D) at about $\Omega/ck = p_\|$ that, for $ck \ll \Omega$, is a long-wavelength relative to the cyclotron radius. Equally in N the relative contributions of the first and second factors are in about the ratio $V_A : c(p_\perp \partial f/\partial p_\| - p_\| \partial f/\partial p_\perp)/(1+p^2)^{1/2}$ (*i.e.*, V_A/c : *Anisotropy Factor*) so that for $p \gg 1$ and $V_A/c \ll 1$ the anisotropy factor dominates D. The result is a very fast instability with a time-scale (the inverse of the imaginary part of the frequency) considerably less than about 10^3 years — well inside the million-year limit, so that any cosmic rays will be isotropic, thus resolving the problem.

Of course, since those early days one has learnt considerably more about the interstellar medium and about the turbulence in the Galactic magnetic field. One has also been able to relate turbulent waves to the diffusion of cosmic rays in the Galaxy and a host of other effects. However, the central theme of a fast cosmic-ray instability persists as the main factor in such processes. Further technical details about this early investigation of the cosmic-ray instability are available in Lerche[9] and a more personal account is presented in my scientific autobiography[10].

Discussion and conclusion

In case you think that this short discussion wraps up the subject of plasma instabilities in astrophysics I would point out that it is just the beginning. The underlying assumption is that the particle distribution functions are prescribed. While such an assumption helps one to see how perturbations grow with time (in terms of both periodic and aperiodic disturbances) there is no quantitative discussion given of the influence of the waves in altering the distribution functions. Thus the waves discussed are linear deviations from an assumed form. What is missing is the non-linear feedback of the waves on the plasma distribution function and how, in turn, such changes influence the waves. Presumably one can solve those problems numerically, but analytical prescriptions are most helpful in both controlling the veracity of any numerical codes as well as providing physical insight into the long-term joint evolution of aperiodic and periodic disturbances. Perhaps that aspect is one of the more important to develop for the future.

In conclusion I do not want to leave one with the impression that the four instabilities discussed are the only available channels influencing astrophysical plasmas. There are bulk instabilities that can be addressed very successfully by magnetohydrodynamic (MHD) procedures. There are instabilities that use spatial inhomogeneities and temporal variations to drive dominant effects in astrophysical systems. However, the four vintage instabilities considered here have considerable strength in modifying the distribution functions of particles — something that MHD methods do not do because MHD deals with bulk properties of a plasma without the fine-scale effects of the actual distribution of the particles in velocity.

Perhaps one can argue that these ancient (more than 50 years old) instabilities are of major significance in astrophysics because they have stood the test of time in terms of their relevance and have been modified, often massively, by later developments, so their impact has increased with time rather than being diminished. There is still much to do and I look forward in my remaining years to seeing further developments and ramifications. I enjoyed being a small part of this original uncovering of fascinating instabilities and I trust that the next generation of workers will make equally fascinating contributions.

Acknowledgements

I am very grateful to Reinhard Schlickeiser who invited me to present this material. Truly, without his blandishments I probably would not have thought any further about the historical matters, so these vignettes owe much to his cajoling.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 241: HR 1884, HD 174103, HD 182563, AND HR 8442, WITH A NOTE ON ζ CEPHEI

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The four binary stars treated in this paper came to attention in different ways. The velocity variability of the two bright stars was recognized by other observers, who in the case of HR 1884 offered a premature orbit for it. The binary nature of the other two stars came to light when they were observed in a presently unpublished extension of the writer's 'Clube Selected Areas' programme. HR 1884 has an orbit with a period of over 21 years and a very high eccentricity (0·89). The two HD stars, on the other hand, have small orbital eccentricities of less than 0·1 (though definitely non-zero); the periods are 435 days for HD 174103 and 1155 days for HD 182563. HR 8442 has a period of 737 days (inconveniently close to two years) and a moderate eccentricity of 0·3. The K-type supergiant ζ Cephei, which is less than 1° from HR 8442, has been said to be a spectroscopic binary, but the writer's 28 measurements of it since 1993 do not support that idea.

Introduction

The two bright binary stars (those with *Bright Star Catalogue* (*HR*) designations) have naturally accumulated a considerable literature, which is summarized in the sections that are respectively devoted to them below. Contrastingly, the two *HD* stars, which are about the ninth magnitude, have remained almost anonymous until now: in fact *Simbad* retrieves only one paper that is said to relate to HD 182563, and even *that* appears to be a mistake, since the paper concerned does not mention HD 182563 at all as far as this writer can see.

HR 1884 is at a declination near +40°; the other three stars are all at still higher declinations (68, 66, and 59 degrees) and pass north of the Cambridge zenith.

HR 1884 (HD 36891)

HR 1884 is a 6^{m} star that is to be found in the interior of the obvious pentagonal figure that is presented to the naked eye by the constellation Auriga; it is nearly half-way towards Capella from the diagonally-opposite bright star θ Aur. It is no stranger to the writer, who observed it[1] in the course of the first of the Cambridge programmes of narrow-band spectrometry, published 55 years ago. At that time, *UBV* photometry was lacking for many of the programme stars, including HR 1884, and (as more specifically related in the Paper 239[2] in this series) Argue[3], a member of the Cambridge staff, was deputed to rectify that lack. He obtained the results $V = 6^{m}{\cdot}08$, $(B - V) = 1^{m}{\cdot}03$, $(U - B) = 0^{m}{\cdot}69$; quite similar values have since been given by Fernie[4], and by Parsons & Montemayor[5] who gave V and $(B - V)$ but not $(U - B)$; Humphreys[6], however, obtained $V = 6^{m}{\cdot}16$. The likelihood that the discrepancy could stem from real variability is diminished by the circumstance that Fernie & Hube[7] specifically looked for photometric variations in a number of stars whose positions in the H–R Diagram fell within the Cepheid instability strip, as HR 1884's probably does, without finding any significant variation in the star of present interest.

HR 1884, type K0 in the *Henry Draper Catalogue*, was first classified on the MK system by Nassau & Morgan[8] as 'G0 Ib:', and soon afterwards by Bidelman[9] as G3 Ib. Bidelman's eagle eye for spectral peculiarities subsequently enabled him to recognize that HR 1884 has a composite spectrum: he[10] gave it as 'cG + F' with the explanatory remark, "K slightly filled in". It might appear to have been clairvoyant of Parsons & Montemayor[5] to have included HR 1884 *the previous year* in a paper entitled *Ultraviolet and optical studies of binaries with luminous cool primaries and hot companions. II. BVRI observations*, but in fact they were not aware of the existence of any companion then: they included HR 1884 in their paper only in an extra table simply giving R and I photometry (as well as V and B, as mentioned above) for "various late-type stars". The spectral classification attributed to me[1] by *Simbad* was merely quoted, as was thought to be clear in the paper.

The absolute magnitude of class-Ib stars is tabulated by Schmidt-Kaler[11] as −5·0 for type G2 and −4·6 for G5; if we adopt a value of $-4^{m}{\cdot}9$ for HR 1884 and allow for a little over half a magnitude of extinction on the basis of the $E(B - V)$ of $0^{m}{\cdot}183$ given (to seemingly untoward precision) by Bersier[12], we find a V_0 magnitude of about $5^{m}{\cdot}5$ and a distance modulus of about $10^{m}{\cdot}4$, corresponding to a distance of about 1200 pc. Humphreys[6] lists a distance, the source of whose estimation is not clearly stated, of 1·16 (presumably kiloparsecs); Andrievsky & Kovtyukh[13] obtained an absolute magnitude of $-5{\cdot}6 \pm 0{\cdot}6$, and Fernie & Hube[7], too, evidently considered the star to be of Cepheid-like luminosity. Balona & Dziembowski[14], who estimated the star's luminosity just from its spectral type (as I have done immediately above) and obtained $M_V \sim -4^{m}{\cdot}8$, say that if it is a Cepheid its period of pulsation should be 20·5 days, although they produce no evidence of the actual existence of any such pulsation.

Kovtyukh, Gorlova & Belik, however, in a paper[15] with the promising title, *Accurate luminosities from the oxygen λ7771–4 triplet* … , assert an M_V as comparatively faint as $-2^{m}{\cdot}79$. That may be quoted — but that is not obvious from the paper, although the value is the same — from the somewhat analogous work of an overlapping consortium, Kovtyukh *et al.*[16], on *Accurate luminosities* *from Fe II/Fe I line depth ratios*. In *that* paper, HR 1884 features in a list of 96 'calibrator' stars, all supergiants, but it is difficult (at least for the present writer) to divine how all those stars and their absolute magnitudes got there. The text tells us that "for the *40* (*my* italics) supergiants in our calibration sample we took the bulk of the M_V estimates from ..." (and there follow some references, in which HR 1884 does not feature). Numbered references are actually listed against 40 of the 96 stars, but the relevant column is blank in respect of the others. Then there are columns headed M_V, σ, N, and s.e.; the impression is given that the real calibration depends on the 40 stars for whose M_Vs references are given, and that, using those as calibrators, the authors of the paper have derived their own M_Vs for all 96 stars in the table, including the initial 40 calibrators. It seems that the quantity listed in the M_V column is the one found by those authors, by reading back from their observed Fe ratios the magnitudes corresponding to a relationship derived from their 40 real calibrators; then the σ is the r.m.s. discrepancy of N (quite numerous, ranging from 6 to 77) individual pairs of Fe II and Fe I lines; and certainly the quantity in the 's.e.' column is seen to be the quotient of the σ divided by $\sqrt{N}$.

The parallax corresponding to the distance modulus of $10^{m}\cdot4$ that is proposed above by the present writer is just under 1 arc-millisecond. The value originally obtained by *Hipparcos*[18] was 1·28 ± 1·01, so it was very compatible with that expectation. Less agreeably, the revised value[19] puts the star 'beyond infinity', with a parallax of –0·75 ± 0·93 milliseconds — about two standard deviations away from my value and from the distance and/or luminosity estimates of Fernie & Hube[7], Andrievsky & Kovtyukh[13], and Balona & Dziembowski[14]; it wishes the star to be of even higher luminosity than all those authors supposed. Two standard deviations is not a fatal discrepancy, and of course we know that the true parallax has at least to be positive. The absolute magnitude repeatedly put forward by Kovtyukh and his collaborators[15,16], however, that corresponds to a distance modulus of about $8^{m}\cdot3$ and thus to a parallax of about 2·2 arc-milliseconds, is almost 'beyond the pale'.

Another absolute-magnitude estimate for HR 1884 that does not agree well with those mentioned in the paragraph next but two above is one of $-3^{m}\cdot66$ given by Andrievsky[17], who appears to set out to determine luminosities through the intermediary of the strength of the Ba II lines at λλ 5853 and 6141 Å. The table of results, which includes Cepheids and also non-variable supergiants, has not only a column headed M_v but others headed M_v^{max} and M_v^{min}, with entries only for the non-variable stars, among which is HR 1884. The columns are not referred to in the text of the paper until right at the end, long after the table is presented; it then appears that they stem from the *Hipparcos* parallaxes and not from the barium lines at all. In the case of HR 1884 the limits are very wide, $-2^{m}\cdot40$ and $-7^{m}\cdot04$, so the result is not very useful, but at least there is no actual conflict with the Cepheid-like assessments referred to in the relevant paragraph above. The limits are evidently those that correspond to the parallax and its uncertainty as given in the original *Hipparcos* publication[18]; the substitution of the revised value[19] would render them nonsensical.

Radial velocity and orbit for HR 1884

Surprisingly for such a bright star, the radial velocity of HR 1884 remained unknown until 1945, when a mean of three measurements was published by Young[20] from the David Dunlap Observatory. No significant variation was noticed: the result was given simply as a mean, of –17·2 km s^{-1}, with a 'probable error' of 0·5 km s^{-1}. It is transcribed to the head of Table I, where it has been adjusted by a zero-point correction of +0·8 km s^{-1} in an effort to place it on the Cambridge zero-point commonly used in this series of papers, and has been attributed an estimated mean date of MJD 31 000; the actual dates of the observations were not published, but the estimate is a reasonable time before Young's paper went to press, and is not of critical importance because (*a*) the orbital period is very long and the observations were made near apastron, when the velocity was changing only slowly, and (*b*) the datum is zero-weighted in the solution of the orbit in any case, so it represents merely a cosmetic addition to the orbit diagram (Fig. 1).

The high luminosity of HR 1884 seems first to have been recognized by the David Dunlap observers. Although spectral types for almost all the objects that they observed had long been available in the *Henry Draper Catalogue*, all the stars were classified anew, but normally only in the spectral-type dimension — the MK two-dimensional classification system had only just been developed at that time, although spectroscopic luminosity criteria had already long been appreciated. A few stars among the 681 in the relevant David Dunlap list[20] did have lower-case letter suffices, mostly 'n' to indicate the broad-line ("nebulous") nature of the spectra of some of the A-type stars, but just five later types were suffixed 'g', which was not specifically mentioned or explained in the paper but evidently meant that the spectrum was recognized as that of a giant star. HR 1884 is one of the 'g' stars — its type is listed as G5g. The others are HR 2977 (49 Cam, now recognized as a peculiar F0 giant of no exceptional luminosity but with the Sr–Eu idiosyncrasy, which no doubt enhances the very line, λ 4077 Å, upon which luminosity estimates so often depend) and HR 8374, 8656, and 8952, all stars of high luminosity with *Bright Star Catalogue* types of G8 Iab, G3 Ib–II, and G0 Ib, respectively.

It was only much later that the variability of HR 1884's radial velocity gradually became apparent. In 1983 Burki & Mayor[21], at the conclusion of a specific investigation of late-type supergiants with the then-new *Coravel* photoelectric radial-velocity spectrometer, offered the opinion "SB?" against that star's identity. Not until 1998 did Butler[22], who included the star in a paper entitled *A precision* [*sic*] *velocity study of photometrically stable stars in the Cepheid instability strip*, demonstrate definite variation, starting remarkably abruptly with a change of more than 40 km s^{-1} between an initial observing run and the next a year later. (That later proved to represent a periastron passage in a highly eccentric orbit of long period.) Butler's 42 measurements were made in bunches in discrete observing runs and represent only ten distinct epochs, which themselves clearly cover only a rather small part of the orbital cycle. All the same, Butler ventured a preliminary orbit for the star, obtaining an orbital period of 9390 days that is more than a thousand standard deviations adrift from the solution given below, although the general character of the solution is correct.

Subsequently de Medeiros *et al.*[23] reported observing the star, in *A catalog of rotational and radial velocities for evolved stars. II. Ib supergiant stars*. They gave a rotational velocity of 6·2 ± 1·0 km s^{-1}, and found a variation in radial velocity: they gave a mean value of –13·23 ± 0·65 km s^{-1} from 13 measurements spanning 3122 days, the r.m.s. deviation from the mean, per observation, being listed as 2·34 km s^{-1}. (So the standard error of the mean must have been calculated as the root of 2·34/13 and not of 2·34/12.) The actual value of the mean velocity is surprising, because for well over the 3122-day span of the observations before the time of submission of the paper, the radial velocity of HR 1884 had *always* been higher (more positive) than the cited mean; in fact, however, the time interval had ended nearly sixteen years before the paper was submitted, and the velocities that it embraced were in the middle of the gently descending side of the velocity curve, as will become apparent from Fig. 1.

The first of the writer's own observations was made in 1991, less than a year before the periastron passage seen in Butler's observations, and have been continued to date, while the ensuing periastron was awaited and observed. Twelve measurements were made on a guest-investigator basis with the Haute-Provence *Coravel*, following on from the series of 13 that were made by others with the same equipment and were noted by de Medeiros *et al.*; then one was made with the spectrometer at the DAO[24], and 54 more have been made with the Cambridge *Coravel* in 1996–2014. Naturally, special attention was paid to the star near the time of periastron in 2013; unfortunately half of the sudden rising branch of the velocity curve was unavoidably missed after the relevant part of the sky was overtaken by daylight in May of that year, and the event was practically over by the time HR 1884 was again accessible in September. Despite that *lacuna* at a critical phase in the observations, the orbit is now quite well determined. The observations are set out in Table I. They include the 13 early Haute-Provence measures that were made by others and were kindly forwarded to me by Dr. S. Udry at my request in 1999; there are also 13 velocities derived from CCD spectra obtained with the 48-inch coudé reflector of the Dominion Astrophysical Observatory (DAO), Victoria, by Dr. R. E. M. Griffin, who very kindly collaborated to ensure satisfactory coverage of as much of the recent periastron passage as astronomical circumstances permitted*.

The radial velocities published by Butler[22] for HR 1884 (and in all the other analogous tables in his paper) have the dates in a column headed 'MJD', which of course is the received abbreviation for 'Modified Julian Date'. They have been faithfully transcribed into the corresponding column in Table I. MJDs were defined by the IAU[25] in 1974 and were intended to do away with (*a*) the unwieldy length of the Julian Date itself, whose initial epoch is absurdly long ago (in 4713 BC) by subtracting 2 400 000 and thereby bringing it up as far as 1857 AD, and (*b*) subtracting a further 0·5 day to remove the half-day phase offset from Universal Time, which is a fruitful source of confusion†. As they stand, Butler's MJDs all seem to refer to times when it would have been daylight at his observing site, and the suspicion arises that, while dropping the initial digits from the JD, Butler omitted to subtract the half-day that aligns MJDs with Universal Time, so they should all be reduced by half a day. Since they were not made at times when the radial velocity of HR 1884 was varying rapidly, timing errors of half a day are not of great significance, but the timing of the first periastron passage must depend to a large extent on Butler's observations. If really they should all be reduced by half a day, the only significant effect will be to lengthen the orbital period by nearly that amount, to 7829·8 days. That change is about a third of the standard error of the period.

*The writer could not afford to await the next one, which is not due until he will (or — much more likely — *would*) be in his 100th year.

†There is a story that, at the time that JDs were invented, the half-day offset was instituted with a view to *reducing* confusion, at least for most astronomers, by avoiding a date change in the middle of the night. But that was when the world — the astronomical world at least — was more or less co-terminous with Europe. A no-less-unlikely explanation is that the idea of starting days at noon originated with Ptolemy, who could at least hope to determine noon (local apparent noon, anyway) by observations of the altitude of the Sun, whereas he had no means of defining when it was midnight.

TABLE I

Radial-velocity observations of HR 1884

Except as noted, the sources of the observations are as follows:
1978–1986 — OHP Coravel (observed by others; weight ¼);
1991–1996 — Observed by Butler[22] at Lick; weight 1;
1997–2014 — Cambridge Coravel observation; weight 1

Date (UT)	*MJD*	*Velocity* km s^{-1}	*Phase*	*(O – C)* km s^{-1}
1943 Oct. 3·00*	31000·00	−16·4	0·754	−0·2
1978 Mar. 15·82	43582·82	−8·9	2·361	0·0
Oct. 5·19	786·19	−9·8	·387	−0·5
10·18	791·18	−9·6	·387	−0·3
22·13	803·13	−10·6	·389	−1·2
1981 Dec. 20·00	44958·00	−11·7	2·536	+0·2
1982 Dec. 14·02	45317·02	−12·1	2·582	+0·6
1983 Jan. 19·87	45353·87	−12·5	2·587	+0·2
Feb. 7·85	372·85	−12·3	·589	+0·5
Dec. 9·04	677·04	−12·7	·628	+0·8
1984 Jan. 13·99	45712·99	−13·1	2·633	+0·5
Nov. 24·07	46028·07	−14·4	·673	0·0
1985 Nov. 2·10	46371·10	−15·5	2·717	−0·2
1986 Oct. 2·15	46705·15	−16·6	2·760	−0·2
1991 Feb. 5·92†	48292·92	−29·2	2·962	+0·4
Oct. 6·02	535·02	−40·25	·993	−0·03
8·04	537·04	−40·14	·994	+0·05
9·03	538·03	−40·14	·994	+0·02
9·98	538·98	−40·20	·994	−0·07
11·03	540·03	−40·24	·994	−0·15
14·03	543·03	−39·87	·994	+0·07
15·03	544·03	−39·92	·994	−0·04
1992 Jan. 24·00†	48645·00	−1·2	3·007	+0·1
Sept. 30·04	895·04	+1·87	·039	+0·10
Oct. 1·02	896·02	+1·78	·039	+0·02
5·05	900·05	+1·75	·040	+0·03
5·99	900·99	+1·87	·040	+0·16
7·04	902·04	+1·76	·040	+0·07
8·04	903·04	+1·74	·040	+0·06
10·05	905·05	+1·77	·041	+0·11
1993 Feb. 13·70	49031·70	+0·07	3·057	−0·28
14·71	032·71	+0·07	·057	−0·27
15·70	033·70	+0·25	·057	−0·08
15·99†	033·99	+0·1	·057	−0·2
Oct. 20·00	280·00	−1·56	·088	+0·04
21·05	281·05	−1·55	·089	+0·06
1994 Jan. 5·10†	49357·10	−1·7	3·098	+0·4
Aug. 3·99	567·99	−3·26	·125	−0·03
6·00	570·00	−3·23	·125	+0·01
6·99	570·99	−3·42	·126	−0·17
8·00	572·00	−3·44	·126	−0·19
8·99	572·99	−3·30	·126	−0·04
10·99	574·99	−3·31	·126	−0·04
12·99	576·99	−3·33	·126	−0·05

TABLE I (*continued*)

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1995 Jan. 8·10†	49725·10	–4·2	3·145	–0·3
Feb. 20·67	768·67	–4·13	·151	0·00
Mar. 16·68	792·68	–4·25	·154	–0·02
Oct. 25·00	50015·00	–5·16	·182	–0·08
26·00	016·00	–5·00	·182	+0·08
26·98	016·98	–5·08	·183	0·00
27·99	017·99	–5·21	·183	–0·12
28·97	018·97	–5·13	·183	–0·04
29·99	019·99	–5·06	·183	+0·04
30·97	020·97	–5·07	·183	+0·03
Dec. 22·07	073·07	–5·7	·190	–0·4
1996 Jan. 1·01†	50083·01	–5·5	3·191	–0·2
Mar. 29·89†	171·89	–5·2	·202	+0·4
Oct. 16·98	372·98	–6·23	·228	+0·01
17·93	373·93	–6·22	·228	+0·02
18·95	374·95	–6·32	·228	–0·08
20·93	376·93	–6·30	·229	–0·05
21·90	377·90	–6·13	·229	+0·12
Nov. 22·14‡	409·14	–6·7	·233	–0·4
Dec. 15·13†	432·13	–6·7	·236	–0·3
20·89	437·89	–6·44	·236	–0·01
24·86	441·86	–6·44	·237	0·00
1997 Jan. 25·03†	50473·03	–6·8	3·241	–0·3
Mar. 2·93	509·93	–6·0	·245	+0·6
Apr. 9·86	547·86	–6·5	·250	+0·2
Sept. 11·06†	702·06	–8·1	·270	–0·9
Dec. 22·09†	804·09	–7·2	·283	+0·2
1999 Apr. 17·21§	51285·21	–9·2	3·345	–0·6
Dec. 29·03	541·03	–8·7	·377	+0·5
2000 Feb. 11·97	51585·97	–9·0	3·383	+0·3
Apr. 5·84	639·84	–8·9	·390	+0·5
Sept. 21·17	808·17	–9·9	·411	–0·1
Nov. 14·13	862·13	–9·8	·418	+0·1
2001 Nov. 14·17	52227·17	–10·7	3·465	0·0
2002 Mar. 29·86	52362·86	–10·7	3·482	+0·2
2003 Jan. 11·09	52650·09	–11·5	3·519	+0·1
Apr. 17·86	746·86	–11·6	·531	+0·2
2004 Apr. 23·88	53118·88	–12·7	3·579	–0·1
Oct. 27·14	305·14	–13·1	·603	–0·1
2005 Mar. 25·90	53454·90	–13·6	3·622	–0·2
Nov. 5·17	679·17	–14·0	·650	–0·1
2006 Apr. 4·87	53829·87	–14·3	3·670	0·0
Oct. 27·14	54035·14	–15·0	·696	–0·1
2007 Mar. 26·94	54185·94	–15·3	3·715	0·0
Oct. 21·16	394·16	–16·1	·742	–0·2
2008 Mar. 31·87	54556·87	–16·6	3·762	–0·1
Dec. 27·07	827·07	–17·5	·797	0·0
2009 Mar. 5·92	54895·92	–17·8	3·806	0·0

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O–C)* *km s*$^{-1}$
2010 Feb. 1·02	55228·02	−19·6	3·848	−0·3
Mar. 2·91	257·91	−19·7	·852	−0·2
Dec. 6·12	536·12	−21·4	·887	−0·1
2011 Apr. 7·91	55658·91	−22·6	3·903	−0·2
Nov. 23·12	888·12	−25·1	·932	−0·1
2012 Jan. 3·96	55929·96	−25·3	3·938	+0·3
Mar. 1·85	987·85	−26·1	·945	+0·5
Sept. 19·20	56189·20	−31·7	·971	+0·1
Nov. 6·19	237·19	−33·7	·977	+0·1
Dec. 26·08	287·08	−36·5	·983	−0·1
2013 Jan. 31·89	56323·89	−38·5	3·988	+0·2
Feb. 8·30¶	331·30	−39·0	·989	+0·1
17·24¶	340·24	−39·6	·990	0·0
26·00¶	349·00	−40·4	·991	−0·4
27·86	350·86	−40·0	·992	+0·1
Mar. 3·24¶	354·24	−40·4	·992	−0·2
10·22¶	361·22	−40·1	·993	+0·1
11·18¶	362·18	−40·1	·993	+0·1
16·23¶	367·23	−40·1	·994	+0·1
19·24¶	370·24	−40·3	·994	−0·2
30·88	381·88	−39·0	·995	0·0
Apr. 5·84	387·84	−37·9	·996	+0·1
15·21¶	397·21	−35·0	·997	+0·4
15·85	397·85	−35·1	·998	+0·1
16·83	398·83	−34·6	·998	+0·2
18·83	400·83	−34·0	·998	0·0
22·20¶	404·20	−33·1	·998	−0·5
25·19¶	407·19	−31·2	·999	0·0
27·84	409·84	−29·9	·999	0·0
29·85	411·85	−28·9	·999	−0·1
30·85	412·85	−28·5	·999	−0·3
May 2·20¶	414·20	−27·2	4·000	+0·3
2·85	414·85	−27·1	·000	0·0
3·85	415·85	−26·4	·000	+0·1
5·21¶	417·21	−25·4	·000	+0·3
6·86	418·86	−24·8	·000	−0·1
7·86	419·86	−24·2	·000	−0·1
Sept. 8·18	543·18	+2·5	·016	−0·7
Oct. 17·17	582·17	+3·6	·021	+0·4
Nov. 13·15	609·15	+3·1	·025	0·0
2014 Jan. 9·99	56666·99	+2·6	4·032	+0·2
Mar. 3·83	719·83	+2·1	·039	+0·3
Oct. 10·19	940·19	−0·2	·067	+0·2
Nov. 4·17	965·17	−0·4	·070	+0·2

*Mean of three DDO observations[20]; weight 0
†Observed at Haute-Provence by author; wt. ½
‡Observed with Cambridge *Coravel*; weight 1
§Observed with DAO spectrometer by author; wt. ¼
¶Observed at DAO with CCD by R. E. M. Griffin; wt. 1

It should be mentioned that the DAO spectra show the *K* line to be slightly filled in, just as Bidelman[10] noted thirty years ago, but the effect is so small, and so difficult to follow even qualitatively beyond the core of the *K* line itself, that there seems to be no possibility of disentangling the very weak spectrum of the earlier-type companion star. We might, however, be optimistic enough to imagine that we could retrieve *some* estimate, however rough, of the nature of the companion star, from photometry, since the $(U-B)$ colour index[3] of $0^{m}\cdot69$ is somewhat 'too blue' to correspond well with the $(B-V)$ of $1^{m}\cdot03$. If those colour indices are corrected for the $E(B-V)$ reddening that has been put[12] at $0^{m}\cdot18$ and the corresponding[26] $0^{m}\cdot13$ for $E(U-B)$ they become $0^{m}\cdot85$ and $0^{m}\cdot56$, respectively, whereas the (interpolated) $(B-V)_0$ and $(U-B)_0$ for type G3 Ib are[27] $0^{m}\cdot90$ and $0^{m}\cdot68$. The differences are small enough, in truth, to be ascribed in their entirety to slight mis-classification and/or mere stellar idiosyncrasy, but they might at least equally be interpreted as evidence, however shaky, for the presence of a hot companion that reduces the $(B-V)$ colour index by $0^{m}\cdot05$ and the $(U-B)$ one by $0^{m}\cdot12$. Such reductions, taken at face value, would imply[28] a companion that is fainter than the primary by $\Delta B = 3^{m}\cdot3$ and $\Delta U = 2^{m}\cdot3$, *viz.*, having magnitudes close to $B = 9^{m}\cdot7$ and $U = 9^{m}\cdot2$, so $(U-B)$ would be about $-0^{m}\cdot5$. A star of type B7 V has[27] a $(U-B)$ colour index of $-0^{m}\cdot43$, and to correspond with the B and U magnitudes its V would have to be about $9^{m}\cdot8$. As its absolute magnitude would be[27] about $-0^{m}\cdot6$ its distance modulus would be $10^{m}\cdot4$. That is exactly the same as we found above for the primary, so the picture of a G3 Ib + B7 V system hangs together very well, although while complimenting ourselves on our success (or luck!) we must not lose sight altogether of the fact that it depends on what are really quite small and possibly misleading photometric discrepancies between the observed and expected colour indices.

When the zero-point of the Haute-Provence velocities is adjusted by +0·8 km s^{-1}, as has been quite usual in this series of papers, in an effort to keep to the Cambridge zero-point that was set up long ago[29] when the photoelectric method of measuring radial velocities was first developed, it is found that the recent Cambridge *Coravel* velocities need no adjustment, the DAO velocities from spectra would benefit from an adjustment of +1·1 km s^{-1}, and Butler's measurements[22], which seem to be on an arbitrary zero-point, need to be altered by −5·0 km s^{-1} to make them homogeneous with the others. Butler's velocities are listed as having internally estimated uncertainties of only 17 metres per second, but Butler's own effort at deriving an orbit showed an r.m.s. residual of 91 m s^{-1} for them, possibly indicating some slight instability in the star itself. When the orbit is solved in the normal fashion from the whole *ensemble* of observations available to us here, it indicates that, for approximate equalization of the variances of the different data sets, the Butler velocities should be attributed five times the weight of the Cambridge *Coravel* ones. If that is done, however, it causes an overpowering proportion of the total weight of the data to be concentrated in one rather small range of phase, possibly causing the apparent standard errors of some of the elements to come out unrealistically small. Not only are those observations clustered in one region of phase, but they were typically taken in bunches of four or five quasi-daily measurements, so although there are 42 of them they represent only ten distinct epochs (arguably nine, as two of the bunches are very close together). After experimenting with their weighting and finding that the solution is not very sensitive to it, the writer opted to give them only the same weight as his own Cambridge *Coravel* velocities. There is a sort of poetic justice in that, inasmuch as there are really only ten (or nine) distinguishable observational epochs, with an average of about four (or five) individual measurements in each bunch; so at the bunch level the Butler observations are being accorded a weighting of about 5, just as the residuals warrant, although at the individual level they are weighted 1, which is quite as much as is sensible to attribute to observations made on successive nights when the orbital period is over 20 years.

The velocity residuals from the DAO spectra warrant those observations being given unit weight. Like Butler's, only more so, they too are restricted to a small range of phase, within which they were made frequently. In *their* case, however, that was deliberate and appropriate, being at a periastron passage in an orbit of extraordinary eccentricity, when the velocity was changing remarkably rapidly — indeed, by more than the measuring error between one night and the next.

There seemed to be a very noticeable difference between the residuals of such of the Haute-Provence *Coravel* velocities as were made personally by the writer and those that had been made by others, and to bring the variances more or less into line the writer's observations needed to be weighted ½ and the earlier ones made by others ¼.

With those preliminaries settled, the orbit was computed; the result appears in the first column of elements in Table V, towards the end of this paper, where the elements of the other orbits discussed below are also tabulated. The orbit is illustrated by Fig. 1. Noteworthy features of the elements start with the period, which despite its length of more than 21 years has a standard error of only 1·4 days. Then the eccentricity commands attention: it is the (approximately equal-) fourth-highest found among the 400-odd binary systems treated in this series of papers; slightly higher eccentricities have been found[30,31] for HD 210647 (Paper 56) and HD 113023 and 117901 (Paper 173), while a very similar value to that of HR 1884 was found[32] for HR 831 (Paper 204). Finally, the mass function

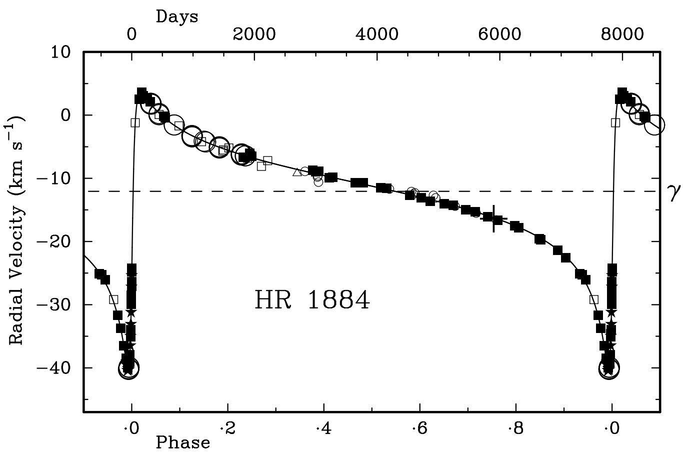

FIG. 1

The observed radial velocities of HR 1884 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The writer's own observations are plotted as squares, open for those made with the Haute-Provence *Coravel* and filled for those made with the Cambridge one; the former have been weighted ½, the latter have unit weight. Small circles denote earlier observations (weight ¼) made by others at Haute-Provence; large circles near the sides of the diagram plot the velocities obtained by Butler[22] at Lick, which have smaller residuals than the Cambridge measures but have been accorded no greater weight owing to their restricted distribution in phase. There is one observation made with the DAO spectrometer (open triangle, to be found near phase ·35, weight ¼). The large plus at about phase ·75 represents the mean velocity published[20] from the David Dunlap Observatory, assigned by the writer to an estimated mean date of MJD 31 000. It dates from a time about 3⅓ cycles before the phase current at the time of writing, which is just to the right of the two overlapping filled squares near zero velocity; it was given no weight in the solution of the orbit.

is quite unusually high, and we need to try to interpret it. It can be regarded as fixing a relationship between $m_2 \sin i$ and m_1, where m_2 and m_1 are the masses of the secondary and primary stars, respectively, and i is of course the orbital inclination.

An immediate difficulty is that we have no means of estimating the mass of the primary, but we can consider the implications of various masses within a reasonable compass. For primary masses of 3, 5, and 10 $M_\odot$, the required values of $m_2 \sin i$ (the minimum masses for the secondary) are about 3, 4, and 6 $M_\odot$, respectively. Such masses[33] belong to main-sequence stars with types of about A0, B8, and B5, whose *U* magnitudes can be expected to be fainter by about $4^{m}\cdot0$, $2^{m}\cdot7$, and $1^{m}\cdot4$ than that of HR 1884. The composite nature of the spectrum would certainly be more conspicuous than is observed if the secondary were B5. The B8 case nearly corresponds with the model that has already been canvassed above, which has a ΔU of $2^{m}\cdot3$. It is to be noted that the types just mentioned are the *latest* ones, giving the least-conspicuous blending of the spectrum of the system, and represent the situation if $\sin i \sim 1$; if the inclination is not high, then the secondary would be *more* massive than the minimum values above, and correspondingly more conspicuous. Although the calculation would be easy to perform, it would be trespassing beyond the authority of the data to compare the $\Delta U = 2^{m}\cdot7$ suggested here from the mass function with the $2^{m}\cdot3$ proposed from photometry, to divine an orbital inclination! In any case the value from the mass function is not in any sense determinate: it is merely the one that corresponds to one of three arbitrarily selected examples among a spread of primary masses. We could obtain any ΔU value that we liked (such as $2^{m}\cdot3$) simply by adopting an appropriate value for the primary mass, which is otherwise unconstrained. The sole new insight that we can validly gain from this discussion of the mass function is that the orbital inclination must be high enough for $\sin i$ to be approaching unity, otherwise a secondary star massive enough to fulfil the mass function would not be so unobtrusive in the spectrum as it actually *is*.

The 'dip' widths seen in the traces from the Cambridge *Coravel* repeat very consistently; reduced in the usual way, with a zero-rotation model whose width is the minimum normally seen among other stars, they give a very precise formal mean $v \sin i$ value of $10\cdot34 \pm 0\cdot08$ km s^{-1}. The Haute-Provence traces, however, which are reduced in Geneva according to a recipe to which the writer is not privy, gave a value of $5\cdot3 \pm 0\cdot4$ km s^{-1} according to the output sent to me in 1999; in the paper[23] published is 2002 it appears as $6\cdot2 \pm 1\cdot0$ km s^{-1}. The uncertainty in the latter represents a realistic lower limit to the uncertainties claimed for rotational velocities obtained just from dip widths. The difference between the Geneva and Cambridge values is no doubt caused by the adoption in Geneva of different zero-rotation widths for stars of different luminosities; certainly, line-widths in supergiants tend to be increased by atmospheric mass motions of the *genre* usually designated as 'turbulence'. Trusting that that issue has been properly considered by the authorities in Geneva, whereas in Cambridge it has not, we should probably trust *their* value for $v \sin i$ in preference to our own.

HD 174103

This is a star, near the ninth magnitude, to be found about 3° preceding and slightly north of the third-magnitude star δ Dra. It came to the writer's attention through being in an extension to Area 2 of the 'Clube Selected Areas'. Those Areas, 16 in all, are in principle distributed around the sky, all at Galactic latitudes of ±35° and at every 45° in Galactic longitude. Originally[34], when the observations were made only in Cambridge, only ten of the 16 Areas could be observed, the others being out of reach in the southern hemisphere. Later, the six missing Areas were observed[35] from ESO, and opportunity was taken to reinforce the observations in some of the near-equatorial Areas too. The stars were selected from the *Henry Draper Catalogue*, the criteria being that they should all be of spectral type K0 and their 'photovisual' magnitudes should be within half a magnitude of $9^{m}\cdot 0$. Purely owing to the disparity between the two hemispheres in the densities of classified stars per unit area of the sky, far more stars fell within the selection criteria, and were observed, in the southern hemisphere than in the northern one. In an effort to redress the disparity and to improve the overall results of the project, additional stars were adopted in the northern Areas by simply increasing their sizes while retaining the same centres. Thus many of stars that had originally been on the fringes of the northern Areas became incorporated within them. The principal results of the expanded programme remain unpublished, but naturally enough some previously unknown spectroscopic binaries were discovered soon after its inception and have been more or less diligently followed since. One of them is HD 174103, the subject of this section, and another is HD 182563, treated below. Both stars were just beyond the northern limit of the original[34] Area 2*.

There is little in the way of previous publications to summarize in respect of HD 174103. There seems to be no ground-based photometry, but *Tycho* (actually *Tycho 2*[36]) has given photometry that is transformed in *Simbad* as $V = 8^{m}\cdot 61$, $(B - V) = 0^{m}\cdot 98$. The star was observed by *Hipparcos*; its (revised[19]) parallax is 2·69 ± 0·63 arc-milliseconds, equivalent to a distance modulus of about 7·9 ± 0·5 magnitudes and thus yielding an absolute magnitude of about $+0^{m}\cdot 7$, with the same uncertainty. Famaey *et al.*[37] noted one measurement of the radial velocity, –6·19 ± 0·43 km s^{-1}, but as its date is not available it is of little utility here. In the Famaey *et al.* tabulation the star is flagged as having constant velocity, but since there was only one observation it is hard to see how such a conclusion could have been reached, unless indeed the concept of constancy was implicitly expanded to embrace all cases in which there was no evidence of *lack* of constancy, even as in the case of interest here where there was no evidence to go on at all.

The writer's initial observation was made at Haute-Provence in 1998; it appears at the head of Table I, where the velocity as reduced in Geneva has been increased by the usual amount of 0·8 km s^{-1} as described for HR 1884 above. One measurement was made in Cambridge in 2002, and the next in 2003, when a modest discordance with the previous ones was found. The star was then followed on a monthly basis for a few years, after which the observations were scheduled in such a way as to improve the uniformity of phase coverage; there are 54 Cambridge observations altogether. The period of rather more than one year naturally became apparent after about a year, and is now determined to a fraction of a day. The observations are set out in Table II and (the initial Haute-Provence one being given half-weight in partial recognition of its relatively bad residual) lead to the elements that are shown in Table V towards the end of this paper. The orbit is illustrated in Fig. 2, where it will be seen to be not far off circular. The eccentricity is actually 0·075 and is getting on for five times its standard deviation, so it is certainly non-zero; a plot analogous to Fig. 2 but showing the circular solution is noticeably 'off' in a systematic fashion.

*Apologies are offered for an oversight in Table 3 (a listing of the Areas) in ref. 34, where the declination of Area 1 was inadvertently repeated for Area 2 in place of true declination, which is 60°.

TABLE II

Radial-velocity observations of HD 174103

All but the first were made with the Cambridge Coravel.

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O – C)* $km\ s^{-1}$
1998 July 28·05*	51022·05	−5·4	$\bar{4}$·680	−0·6
2002 Sept. 10·01	52527·01	−5·4	0·139	−0·1
2003 Sept. 19·90	52901·90	−2·8	1·001	0·0
Oct. 3·86	915·86	−3·2	·033	+0·1
Nov. 3·78	946·78	−4·9	·104	−0·3
Dec. 7·74	980·74	−5·7	·182	+0·4
2004 Mar. 31·17	53095·17	−8·0	1·445	−0·2
May 19·10	144·10	−7·0	·557	−0·3
June 15·06	171·06	−5·8	·619	0·0
July 9·98	195·98	−4·8	·677	+0·1
Aug. 8·04	225·04	−3·7	·743	+0·1
Sept. 1·00	249·00	−3·0	·799	0·0
Oct. 18·90	296·90	−2·2	·909	0·0
Nov. 13·82	322·82	−2·2	·968	+0·3
Dec. 11·72	350·72	−3·9	2·032	−0·6
2005 Jan. 13·28	53383·28	−4·8	2·107	−0·1
Mar. 25·17	454·17	−7·5	·270	−0·1
Apr. 22·13	482·13	−8·1	·334	−0·3
May 15·08	505·08	−8·0	·387	−0·1
June 27·02	548·02	−7·7	·486	−0·2
July 17·01	568·01	−6·9	·532	+0·1
Aug. 6·93	588·93	−6·1	·580	+0·3
Sept. 27·86	640·86	−4·7	·699	−0·2
Oct. 4·81	647·81	−4·3	·715	0·0
25·82	668·82	−3·6	·763	−0·1
Nov. 16·76	690·76	−3·1	·814	−0·3
29·82	703·82	−2·4	·844	+0·1
Dec. 17·71	721·71	−1·9	·885	+0·4
2006 Mar. 1·24	53795·24	−3·5	3·054	+0·1
23·20	817·20	−4·8	·104	−0·2
Apr. 9·13	834·13	−5·4	·143	0·0
26·08	851·08	−6·1	·182	0·0
May 6·13	861·13	−6·6	·205	−0·1
June 22·07	908·07	−7·9	·313	−0·2
July 12·01	928·01	−7·5	·359	+0·4
Aug. 8·01	955·01	−7·9	·421	0·0
28·92	975·92	−7·7	·469	−0·1
2007 July 22·98	54303·98	−6·6	4·223	+0·2
31·98	312·98	−7·1	·244	−0·1
Dec. 15·72	449·72	−6·5	·558	+0·2
2008 July 21·99	54668·99	−3·6	5·062	+0·2
30·01	677·01	−4·0	·081	+0·1
Aug. 3·02	681·02	−4·2	·090	+0·1
Oct. 31·82	770·82	−7·2	·296	+0·4
2009 Mar. 30·18	54920·18	−5·4	5·639	+0·1
2010 Apr. 8·18	55294·18	−7·4	6·499	0·0
May 18·10	334·10	−6·1	·591	+0·2
Sept. 12·95	451·95	−2·8	·862	−0·4
Oct. 10·85	479·85	−1·9	·926	+0·3
19·89	488·89	−2·2	·946	+0·1

TABLE II (*concluded*)

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
2011 Sept. 14·93	55818·93	−4·3	7·705	+0·1
Dec. 5·79	900·79	−2·5	·893	−0·3
2013 May 14·08	56426·08	−4·3	9·100	+0·2
2014 June 6·09	56814·09	−2·9	9·992	−0·2
July 24·98	862·98	−4·7	10·105	−0·1

*Observed with Haute-Provence *Coravel*; weight ½

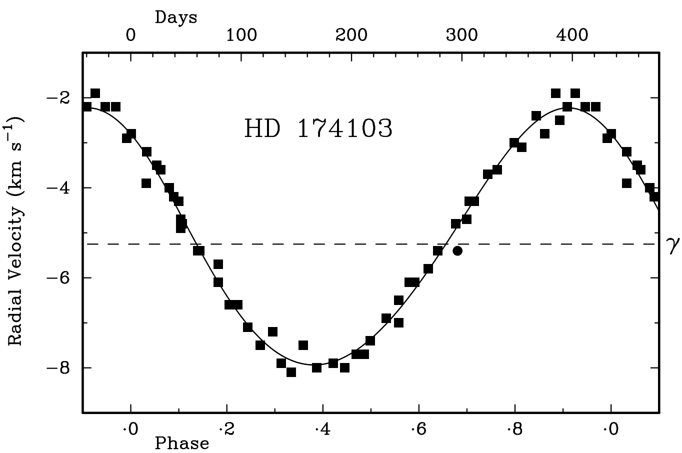

FIG. 2

The observed radial velocities of HD 174103 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All but one of the observations were made with the Cambridge *Coravel* and are plotted as filled squares; there is a single measurement (chronologically the first one) that was obtained at Haute-Provence and is represented by a filled circle.

If we were concerned to demonstrate the non-zero character of the eccentricity by a method which would enable us to quote chapter and verse, we could use the second statistical test explained by Bassett[38] in 1978, in his kind correction of a misapprehension displayed by the writer in some of the early papers in this series. Whereas the sum of the squares of the residuals of the 55 radial velocities from the adopted solution of the orbit is 2·66 $(km\ s^{-1})^2$, the corresponding quantity for the orbit upon which zero eccentricity is forced is 3·82. The 2·66 represent the 49 degrees of freedom left after six orbital elements were fitted, so they cost about 0·054 $(km\ s^{-1})^2$ per degree, whereas in the circular solution we gained two degrees of freedom by not fitting e and ω, but only at a cost of (3·82 – 2·66) $(km\ s^{-1})^2$ (0·58 per degree). The quotient 0·58/0·054 — about 10·7 — is the statisticians' F ratio with 2 and 49 degrees of freedom, whose significance can be appreciated by recourse to tables such as those of Lindley & Miller[39]. We find there (by interpolation) that to be significant at the 1% level $F(2,49)$

has to exceed 5·07, and even the 0·1% level is reached at close to 8, so 10·7 is literally 'off the charts'! If we were keen to get a rather exact numerical probability of the observed occurrence we could turn to the Web and (if we could find where[40] to look!) obtain the value 0·014%.

The mass function is very small and, if the primary star is arbitrarily (but plausibly) estimated to have a mass of 2 $M_{\odot}$, its companion does not need to be more than 0·4 $M_{\odot}$, corresponding to the mass of an M2 main-sequence star, so it is no surprise that the secondary has not been apparent in the radial-velocity traces. The rotational velocity is small — the formal mean value from the 54 Cambridge traces is 1·9 km s^{-1}, with a standard error of 0·25 km s^{-1}, but so as not to over-state the reliability of a rotational velocity determined just from dip widths the result should best be stated as 2 ± 1 km s^{-1}.

HD 182563

HD 182563, a star somewhat fainter than the ninth magnitude, is located only about 25′ north of the 4½^{m} star π Dra, which is itself only 2° south-following the relatively bright object δ Dra that was used above as the reference point for HD 174103. It was observed by *Tycho* but not by *Hipparcos*; the *Tycho 2* magnitudes, transformed by *Simbad* to the *UBV* system, are V = 9^{m}·20, $(B - V)$ = 0^{m}·52. Although (like all the stars in the 'Clube' programme) the *HD* type is K0, the colour index would suggest a type of about F8 V: although there is no direct information about the distance or luminosity, the fact is that there *aren't* any normal stars that are *that* blue on the giant branch of the H–R Diagram, so a star of that colour is almost *obliged* to be on or near the main sequence. The radial-velocity traces show an unusually shallow dip, with an equivalent width (defined just as for spectroscopy but in terms of km s^{-1}, those being the abscissae of the radial-velocity scan in the same way as wavelengths are for spectra) of about 2·6 km s^{-1} — only about half the strength of the dips given by HD 174103 (5·1 km s^{-1}) and (despite its rather early type) by HR 1884 (5·6 km s^{-1}). HD 182563 actually emphasizes its rather early type by exhibiting quite significant rotation: the mean $v \sin i$ found from the Cambridge traces is 10·7 km s^{-1}, with a formal standard error of only 0·23 km s^{-1}, so the dips on radial-velocity traces are somewhat smeared out and are even shallower in comparison with those of most other stars than the smallness of their equivalent widths might suggest. Near the nodes of the orbit, there have been occasions when the observer has thought that the trace exhibited an asymmetry such as would be expected if there were a weak secondary dip, but that has not happened sufficiently systematically to warrant a claim that the object is double-lined.

Simbad retrieves only one paper for HD 182563, one concerning the interstellar absorption band near λ 2200 Å. That paper does not actually say anything about HD 182563, which is neither bright enough nor hot enough to show anything of a 2200-Å feature to the instrument that provided the source material for the paper. The 422 stars listed in the paper are bright O and B stars; only six are as 'faint' as 7^{m}, and the great majority feature in the *Bright Star Catalogue*. They include HD 18256*8* (HR 7372; 2 Cyg), a 5^{m} star of type B3, which one might think has been dyslexically entered in *Simbad* as if it were the star of present interest here. To avoid unnecessarily exacerbating the mistake (if that is what it is), and because the matter is of no actual relevance to HD 182563 apart from the correction of the perceived mistake, we refrain from giving any references in this paragraph.

The first radial-velocity observation of HD 182563 was made in 2002; the second, a year later, was very discordant with it. That prompted the scheduling of

TABLE III

Radial-velocity observations of HD 182563

All the observations were made with the Cambridge Coravel.

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2002 Sept. 10·05	52527·05	−17·8	0·415	−0·5
2003 Sept. 19·98	52901·98	−5·0	0·740	+1·1
23·89	905·89	−6·6	·743	−0·5
Oct. 18·79	930·79	−6·0	·764	−0·1
Nov. 3·78	946·78	−6·8	·778	−0·9
27·86	970·86	−6·5	·799	−0·4
Dec. 28·76	53001·76	−6·8	·826	−0·4
2004 Apr. 15·15	53110·15	−9·8	0·920	+0·1
May 23·08	148·08	−11·3	·952	+0·4
June 22·08	178·08	−13·9	·978	−0·6
Aug. 12·97	229·97	−16·8	1·023	−0·7
Sept. 5·97	253·97	−17·4	·044	−0·1
Oct. 5·89	283·89	−18·4	·070	+0·3
25·85	303·85	−20·1	·087	−0·5
Nov. 19·79	328·79	−21·0	·109	−0·5
Dec. 6·83	345·83	−21·1	·124	−0·1
2005 Jan. 8·73	53378·73	−21·3	1·152	+0·5
May 8·12	498·12	−21·7	·255	+0·4
June 11·06	532·06	−21·1	·285	+0·5
July 17·03	568·03	−20·5	·316	+0·3
Aug. 15·03	597·03	−21·1	·341	−1·0
Sept. 12·97	625·97	−19·3	·366	−0·1
Oct. 25·86	668·86	−17·8	·403	0·0
Nov. 29·84	703·84	−16·9	·433	−0·3
2006 Mar. 23·20	53817·20	−12·5	1·531	−0·1
Apr. 26·09	851·09	−11·7	·561	−0·5
May 30·09	885·09	−9·1	·590	+0·9
June 27·98	913·98	−8·9	·615	+0·2
July 29·07	945·07	−7·4	·642	+0·8
Aug. 28·96	975·96	−8·5	·669	−1·1
Sept. 22·96	54000·96	−6·5	·691	+0·4
Oct. 24·88	032·88	−6·8	·718	−0·4
2007 Mar. 27·17	54186·17	−7·4	1·851	−0·4
May 1·13	221·13	−7·9	·881	+0·2
June 1·08	252·08	−8·2	·908	+1·1
July 7·07	288·07	−10·3	·939	+0·6
Aug. 10·94	322·94	−12·9	·969	−0·2
Sept. 22·92	365·92	−15·1	2·006	−0·1
Oct. 13·89	386·89	−15·5	·024	+0·7
2008 Apr. 24·14	54580·14	−22·3	2·192	+0·1
May 22·07	608·07	−22·2	·216	+0·2
Oct. 21·87	760·87	−19·6	·348	+0·2
Dec. 9·74	809·74	−18·6	·390	−0·3
2009 May 24·10	54975·10	−11·7	2·533	+0·6
Aug. 8·00	55051·00	−9·6	·599	+0·1
Sept. 25·90	099·90	−7·9	·641	+0·3
2010 July 30·02	55407·02	−8·8	2·907	+0·4
2011 Sept. 27·95	55831·95	−21·8	3·275	0·0

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2011 Dec. 10·72	55905·72	−19·5	3·339	+0·6
2012 Apr. 30·13	56047·13	−15·4	3·461	0·0
June 29·05	107·05	−13·8	·513	−0·6
Sept. 13·93	183·93	−10·0	·579	+0·4
2013 May 14·08	56426·08	−5·9	3·789	+0·1
July 12·06	485·06	−6·8	·840	−0·1
Dec. 9·78	635·78	−13·2	·970	−0·4
2014 July 31·01	56869·01	−22·3	4·172	−0·1
Aug. 3·01	872·01	−21·9	·175	+0·3
Oct. 7·86	937·86	−22·8	·232	−0·4

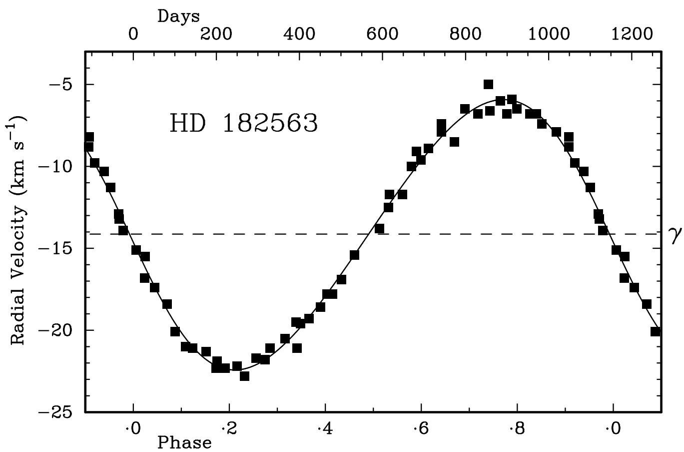

FIG. 3

As Fig. 2, but for HD 182563. In this case *all* of the observations were made with the Cambridge *Coravel*.

further observations on a quasi-monthly basis, a routine that was helped by the high declination of the star, which has been observed in every calendar month except February. The 58 radial velocities, all obtained with the Cambridge *Coravel*, are listed in Table III and yield the elements that are included in Table V; the orbit is plotted in Fig. 3.

As in the case of HD 174103, the orbital eccentricity is small, but in this case it is eight times its standard error and so is incontrovertibly non-zero; the uncertainty of the longitude of periastron is correspondingly about an eighth of a radian. If we have correctly divined that the spectral type of HD 182563 is about F8 V, its mass must be very near to 1·2 $M_{\odot}$, and to satisfy the mass function the companion star must have a mass not less than 0·6 $M_{\odot}$. That is appropriate to a star whose type is about K7 V, with an absolute magnitude of about 8 — four magnitudes fainter than the primary. Although it could be brighter than that, to any degree, depending only on the orbital inclination, at the high inclinations that are most probable statistically the increase in the mass above the lower limit is not great. It is, therefore, not a matter for surprise that the secondary has not been seen with any certainty in the radial-velocity traces and does not appear to distort the radial-velocity curve seen in Fig. 3, as would happen if the measured 'dips' were at all significantly blended.

HR 8442 (HD 210220)

HR 8442 is a sixth-magnitude star in Cepheus, less than 1° north-preceding the late-type supergiant ζ Cep*; it is about 3° following, and in an almost identical declination to, μ Cep, Sir William Herschel's[43] 'Garnet Star'. Just as in the case of HR 1884 described above, HR 8442 was on the early Cambridge programmes of narrow-band spectrometry, of which the first was the writer's investigation[1], carried out with the help and oversight of his mentor Redman, of the violet CN bands in more than 700 late-type stars. Again like HR 1884, HR 8442 lacked modern photometry at that time, and that omission was rectified in the same way by Argue[3], whose results seem still to be the only photometry available for the star. He found $V = 6^{m}\cdot32$, $(B-V) = 0^{m}\cdot88$, $(U-B) = 0^{m}\cdot63$. The fact that HR 8442 is a giant star appears first to have been recognized at Mount Wilson, where Adams *et al.*[44], who had developed there the method of 'spectroscopic parallaxes', found an M_V of $+0^{m}\cdot6$. The parallax of 5·82 arc-milliseconds originally found by *Hipparcos* is equivalent to a distance modulus of $6^{m}\cdot17$, which would give HR 8442 an absolute magnitude a little fainter than zero, but the revised[19] value of 4·80 ± 0·46 milliseconds puts it rather further from the Mount Wilson value, at about $-0^{m}\cdot3 \pm 0^{m}\cdot2$.

The star was classified as G5 in the *Henry Draper Catalogue*[45], and then at Mount Wilson, first[46] as G6 and then[44] as G4. It was given on the MK system as G6 III by Bidelman[47], and that is the type that is shown for it in the *Bright Star Catalogue*[48]. Nassau & van Albada[49] subsequently gave its type as K1 III, extraordinarily different from Bidelman's — and, for that matter, from all three of the previous types. Although the K1 classification was made from an objective-prism plate, and Bidelman's reputation as a spectroscopic expert *par excellence* would take some beating, it may be noted that the colour index[3] of HR 8442 corresponds on average to about type K0 among giant stars[50], so it would seem unwise to dismiss the Nassau & van Albada classification out of hand.

The CN strength[1] proved to be a little strong for type G6, but would be just average if the type is really G8 to K0; too much significance should not be read into that, however, as (rather disappointingly) CN strength seemed in that work[1] not to be uniquely correlated with other then-known properties of the stars concerned. Gray[51] has used the rapid variation of the depth of the V I (*i.e.*, neutral vanadium) line at λ 6251·83 Å in comparison with the adjacent Fe I line at λ 6252·57 Å to interpolate temperature (and thereby spectral) types for a lot of stars on the basis of the run of relative depths of those lines in a large number of spectral-type standards. The strength of the vanadium line varies very rapidly with temperature, such that the V/Fe depth ratio ranges from about 0·17 at G3 III to 0·72 at K0 III; in Gray's work the ratio was measured to about

*Suggested in the past[41,42], seemingly incorrectly, to be itself a spectroscopic binary — see concluding section below.

0·01, corresponding therefore to about an eighth of a sub-type in spectral class, although it is doubtless optimistic to suppose that the ratio is not influenced by any other factor. Gray[51] asserted, however, that "This particular line depth ratio has the disadvantage of invoking two elements, but V and Fe show no obvious differential abundance effects" — so that appears to dispose of the most obvious potential complicating factor. Later, however, he[52] did recognize and discuss factors that slightly impair the uniqueness of the relationship between the line ratio and temperature. For the case of interest here, HR 8442, he derived a type of G8·6 — which we notice is very close to the median between Bidelman's G6 and Nassau & van Albada's K1. Over the same (G3–K0) range of type, the mean trend of $(B - V)$ varies only from about $0^{m}\!\cdot\!8$ to $1^{m}\!\cdot\!0$.

Casting one's eye down Gray's[51] list of 86 stars, one could notice two cases where the $(B - V)$ value is badly out of line — much too blue to correspond to Gray's inferred spectral type, though not so bad for the actually classified one. The two stars are HR 3112 and HR 8059 (12 Aqr). They are both known to have 'visual' companions, and HR 3112 is also a 97-day spectroscopic binary that was documented[53] in Paper 189 of the series of papers of which *this* one is a member. Both objects could usefully be discussed anew — but preferably not here in a section supposedly devoted to HR 8442!

The radial velocity of HR 8442 was first measured at Mt. Wilson in 1915, and then twice more in 1916 on dates less than a month apart. The results were given[54] (the star is identified as Boss[55] 5694) only as a mean velocity, which had a 'probable error' of 2·5 km s^{-1}, the third-largest among the 45 such entries on that page of the paper, but evidently was not considered to demonstrate any real variation. The three velocities were long afterwards published individually in a public-spirited enterprise by Abt[56], where it is seen that one of them departs by about 10 km s^{-1} from the other two. They are listed at the head of Table IV here, where all three are seen to give residuals of about 5 km s^{-1} from the orbit derived below. The star seems not to have been re-observed for radial velocity until it was measured 70 years later by de Medeiros & Mayor[57], who made just two observations with the Haute-Provence *Coravel* and found them to differ by 7·03 km s^{-1}, decisively demonstrating a change. They also listed a $v \sin i$ of 1·8 ± 1·5 km s^{-1} that is more or less compatible with Gray's[51] 3·4 ± 0·7 km s^{-1}. Later, de Medeiros *et al.*[58] published exactly the same information, with the curious exception of the mean velocity, in a different journal. The individual velocities whose means were published by de Medeiros & Mayor[57] were lodged with the Centre de Données Stellaires at the end of 2001, and it was from that list that HR 8442 (with a number of other stars) was adopted for radial-velocity measurement in Cambridge. Fifty measurements have been made with the Cambridge *Coravel* and are set out in Table IV. They yield the orbit whose elements are given in the last column of Table V and is illustrated in Fig. 4. The two small gaps in phase coverage arise from the orbital period's being just a week longer than exactly two years; the disadvantage is moderated by the high declination of the star, which allows it to be observed at substantial hour angles — it has in fact been measured in every calendar month of the year.

The orbit is of moderate eccentricity; the mass function is small, so it is far from surprising that no sign has been noticed of the secondary star in any of the radial-velocity traces. In fact, the very last Cambridge observation, obtained right at the more favourable node when the velocity separation between the components would be maximal, was taken with a suitably wide scan and

TABLE IV

Radial-velocity observations of HR 8442

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

Date (UT)	*MJD*	*Velocity* *km s*$^{-1}$	*Phase*	*(O – C)* *km s*$^{-1}$
1915 Nov. 24·15*	20825·15	−7·0	$\overline{43}$·856	−5·5
1916 Oct. 15·21*	21151·21	−16·3	$\overline{42}$·298	−4·5
Nov. 9·21*	176·21	−6·9	·332	+4·6
1986 Aug. 18·02†	46660·02	−0·8	$\overline{8}$·892	+0·3
1987 Sept. 3·05†	47041·05	−10·7	$\overline{7}$·409	−0·1
2002 July 15·09	52470·09	−3·3	0·772	+0·1
Sept. 1·99	518·99	−1·8	·838	0·0
Oct. 4·92	551·92	−1·2	·882	−0·1
Nov. 12·92	590·92	−1·1	·935	+0·2
Dec. 18·82	626·82	−2·9	·984	+0·1
2003 Jan. 16·79	52655·79	−5·4	1·023	0·0
Feb. 14·77	684·77	−8·0	·063	0·0
May 26·12	785·12	−12·0	·199	0·0
June 25·10	815·10	−12·4	·239	−0·3
Aug. 4·05	855·05	−11·8	·294	+0·1
Sept. 11·00	893·00	−11·3	·345	+0·1
Oct. 11·95	923·95	−10·6	·387	+0·3
Nov. 12·92	955·92	−10·1	·430	+0·2
Dec. 15·83	988·83	−9·5	·475	+0·1
2004 Jan. 9·76	53013·76	−9·0	1·509	0·0
Apr. 23·14	118·14	−6·2	·650	0·0
May 17·10	142·10	−5·8	·683	−0·3
June 22·09	178·09	−4·5	·732	−0·1
Aug. 7·09	224·09	−2·6	·794	+0·2
Dec. 5·76	344·76	−2·0	·958	−0·1
2005 Jan. 12·73	53382·73	−4·4	2·009	+0·1
Feb. 8·78	409·78	−7·2	·046	−0·3
May 5·08	495·08	−11·7	·162	−0·1
2006 Mar. 23·21	53817·21	−7·3	2·598	0·0
July 12·07	928·07	−3·8	·749	+0·1
2007 Apr. 12·18	54202·18	−10·4	3·120	+0·2
May 2·15	222·15	−11·0	·148	+0·3
June 21·08	272·08	−12·4	·215	−0·3
July 25·10	306·10	−11·9	·261	+0·1
Sept. 7·99	350·99	−11·8	·322	−0·2
Oct. 13·95	386·95	−11·0	·371	+0·1
2008 Feb. 11·78	54507·78	−8·9	3·535	−0·3
May 19·11	605·11	−5·9	·667	0·0
Oct. 9·02	748·02	−1·7	·861	−0·3
27·96	766·96	−1·1	·886	0·0
Nov. 21·93	791·93	−1·2	·920	−0·1
Dec. 17·76	817·76	−1·7	·955	+0·1
2009 Jan. 2·81	54833·81	−2·9	3·977	−0·2
18·73	849·73	−3·8	·999	+0·1
May 24·11	975·11	−11·8	4·169	−0·1
Dec. 20·80	55185·80	−10·0	·454	−0·1

TABLE IV (*concluded*)

Date (UT)		MJD	Velocity km s^{-1}	Phase	(O – C) km s^{-1}
2010	May 23·11	55339·11	–6·0	4·662	0·0
	June 27·10	374·10	–5·0	·710	–0·1
	Sept. 12·99	451·99	–2·4	·815	–0·1
2011	Jan. 18·77	55579·77	–3·3	4·989	0·0
	Aug. 10·04	783·04	–11·8	5·264	+0·2
2012	Apr. 30·15	56047·15	–6·6	5·623	+0·2
2013	Sept. 14·97	56549·97	–12·0	6·304	–0·2
2014	Sept. 10·99	56910·99	–2·7	6·794	+0·1
	Nov. 23·92	984·92	–1·1	·894	–0·1

*Mt. Wilson photographic observation[54,56]; wt. 0
†Haute-Provence *Coravel* observation[57]; weight 1

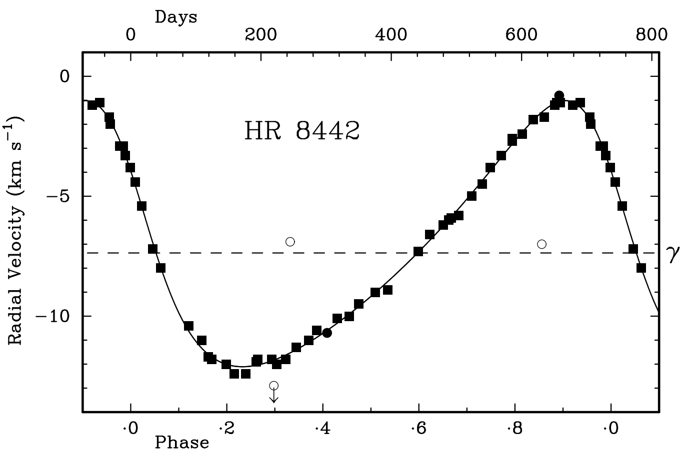

FIG. 4

The observed radial velocities of HR 8442 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. Most of the radial velocities were obtained with the Cambridge *Coravel*, but there are two (filled circles) that were made by others[57] and have been retrieved *via* the *CDS*, and three (open circles, one of them well off the bottom of the plot where indicated by the arrow) that were made almost 100 years ago at Mount Wilson and published as a mean value by Adams & Joy[54] in 1923 and much later (1976) in detail by Abt[56]. The two small gaps in the phase coverage of the observations arise because the orbital period differs by only a week from exactly two years, so any given phase takes about a century to migrate around the calendar. The situation is, however, largely retrieved by the high declination of the star, which has allowed it to be observed in every calendar month.

integrated for much longer than would ordinarily be necessary to measure the velocity, just in order to look for any sign in the trace of the secondary component — but there was none.

The rotational velocity is small: the formal mean value is 1·21 ± 0·15 km s^{-1}, but 13 of the 50 values that went into that average are zero, and lower values are of course not permissible although no doubt there would be a small tail of the distribution extending into such values if they were allowed, so the mean obtained here must be regarded as probably a slight over-estimate. In any case, since no greater accuracy than ±1 km s^{-1} is claimed for rotational velocities derived in a rather rough-and-ready way from dip widths in radial-velocity traces, the mean is probably best expressed as 1 ± 1 km s^{-1}. In comparison with the other values quoted above, it is in reasonable agreement with that given by de Medeiros & Mayor[57] from only two observations, but sits less comfortably with that of Gray[51], which was determined in a more sophisticated manner and might on that account be considered preferable.

TABLE V

Orbital elements for the four stars

Element	*HR 1884*	*HD 174103*	*HD 182563*	*HR 8442*
P (days)	7829·3 ± 1·4	435·1 ± 0·4	1155·6 ± 2·4	737·4 ± 0·4
T (MJD)	54417·21 ± 0·22	54207 ± 14	54359 ± 23	54113·3 ± 2·1
γ (km s^{-1})	−12·06 ± 0·03	−5·25 ± 0·03	−14·13 ± 0·07	−7·37 ± 0·03
K_1 (km s^{-1})	21·77 ± 0·04	2·85 ± 0·05	8·25 ± 0·10	5·55 ± 0·04
e	0·8867 ± 0·0006	0·075 ± 0·016	0·095 ± 0·012	0·308 ± 0·007
ω (degrees)	250·61 ± 0·20	37 ± 12	93 ± 7	61·9 ± 1·3
$a_1 \sin i$ (Gm)	1084 ± 4	17·03 ± 0·29	130·5 ± 1·6	53·5 ± 0·4
$f(m)$ ($M_\odot$)	0·829 ± 0·008	0·00104 ± 0·00005	0·0665 ± 0·0024	0·01126 ± 0·00023
R.m.s. residual (wt. 1) (km s^{-1})	0·21	0·22	0·49	0·17

ζ Cephei (HR 8465, HD 210745)

This section is in the nature of an addendum regarding a star that became of oblique interest during the writer's radial-velocity programme, partly on account of its proximity in the sky to HR 8442, but mostly because it had been asserted, as long ago as 1907, to be a spectroscopic binary. *Simbad* records no fewer than 246 papers that refer to ζ Cep; it is not the writer's purpose to summarize the whole literature but only to refer to that (relatively small) part of it that is germane to the interest here in radial velocities, after giving a brief description of the nature of the star.

Zeta Cep is an early-K supergiant, for which *Simbad* reports numerous classifications, seemingly wherever it saw a type quoted for it; almost all the types listed are in fact quotations, including two[1,59] that are attributed to *me*! We could accept as authoritative Keenan's last classification of it, in the '*Perkins Catalogue*'[60] of 1989, as type K1·5 Ib. There are several truly independent determinations of its *UBV* photometry, which are in reasonable but not very precise agreement, all close to $V = 3^{m}{\cdot}35$, $(B-V) = 1^{m}{\cdot}57$, $(U-B) = 1^{m}{\cdot}74$. *Hipparcos* lists it (as HIP 109492) as being an 'unsolved' variable star, giving its maximum and minimum brightness (on the '*Hp*' scale, close to V) as $3^{m}{\cdot}50$ and $3^{m}{\cdot}54$ — so there is certainly not very much amplitude to underpin any positive assignment of a type of variability. All the same, *Simbad*'s main heading for the star says boldly, "**zet Cep** — Eclipsing binary". The extraordinarily accurate parallax of 3·90 ± 0·10 arc-milliseconds[19] corresponds to a distance modulus of $7^{m}{\cdot}05$ and thus to an absolute magnitude of $-3^{m}{\cdot}70$, with an uncertainty of only about $0^{m}{\cdot}05$. A late-type star of such high luminosity (and correspondingly low surface gravity) is only too liable to have a rather unstable and 'macroturbulent' atmosphere whose character will manifest itself in slight ($\lesssim$ 1 km s^{-1}) radial-velocity instability.

The 1907 proposal of the variability of the radial velocity of ζ Cep was made by Moore[41,61], of the Lick Observatory. The assertion is implicitly reaffirmed by its quotation in the final catalogue[62] of the great Lick survey, conducted throughout the first quarter of the 20th Century, of all the bright stars. Misgivings, however, were later entertained by Moore himself, who noted them in an early general catalogue of known radial velocities compiled under his own direction[63]. But the variability was actually reaffirmed from Lick by Katherine Gordon[64] some years later, and was asserted again in the *Radial Velocity Catalogue*[42]. De Medeiros *et al.*[23] included the star in a survey made with the Haute-Provence *Coravel* of a lot of late-type supergiants; they found a mean radial velocity of −17·56 ± 0·14 km s^{-1}, with an r.m.s. dispersion of 0·41 km s^{-1} among nine measurements. Tremko *et al.*[65] found a mean of −17·08 with an r.m.s. spread of 0·42 km s^{-1} from 27 photographic spectrograms of 12 Å mm^{-1} taken at the David Dunlap 74-inch Cassegrain. Eaton & Williamson[66] made 34 measurements with their Tennessee radial-velocity instrument, finding a mean velocity of −18·05 and an r.m.s. spread of 0·34 km s^{-1}. Hekker *et al.*[67], who used the very precise Lick 'planet-finding' system which they said gave a velocity precision of 5 to 8 m s^{-1}, found that 80% of late-type giant and supergiant stars exhibited velocity instability at a level of $\gtrsim$ 20 m s^{-1}, including ζ Cep; they note for that star an apparent periodicity of 533 days, but give no information about the amplitude or the faithfulness of velocity variations to the noted period. It should perhaps be remarked that the mean values mentioned for the various sets of velocities are not to be expected to be comparable at the level that might be deduced by assuming that those mean values were more accurate than the individual observations by factors of their respective $\sqrt{N}$, as velocity zero-points are notoriously difficult to establish with any accuracy.

Although it might now appear somewhat superfluous to offer another set of velocities for ζ Cep, it seems a pity not to do so, since the writer's interest in the star started more than 20 years ago when it was not a matter of such popularity as it seems to have become in recent years. Table VI presents 28 measurements made with the Haute-Provence and Cambridge *Coravel*s. The star gives the most magnificent 'dips' in radial-velocity traces, deeper *and* wider than the great majority of late-type stars. The dips occupy pretty well the whole scan width of Haute-Provence traces, and there is a liability to appreciable error from any slope that the trace may have, because there is not enough continuum at the ends of the scan to recognize a slope. The Cambridge scans can be made arbitrarily wide, and are routinely levelled in the reduction procedure if they exhibit any slope between the sections of continuum at the ends of the traces. (Slight slopes on the traces are an occupational hazard for users of radial-velocity spectrometers!) The result of the *Coravel* observations is analogous to the findings of the investigations outlined above: there sometimes appear to be variations that are larger than the typical measuring errors found for other stars. In the absence of the corroborating evidence from independent work, however, the writer would not care to assert that there were real variations, since the appreciably increased width of the radial-velocity 'dips' in the *Coravel* traces, in comparison with most stars, impairs the precision with which they can be bisected. The width would correspond to a rotational velocity ($v \sin i$) of about 12 km s^{-1} if it were seen in a star of more normal luminosity, but the turbulence in the supergiant's atmosphere is likely to be responsible for some, possibly even all, of the broadening of the dip.

TABLE VI

Coravel radial velocities of ζ Cephei

Date (UT)			RV ($km\ s^{-1}$)
1993	Feb.	11·74*	−17·6
	Mar.	23·15*	−17·6
	July	9·09*	−16·5
	Sept.	12·01*	−16·4
	Dec.	26·81*	−15·8
1994	Feb.	11·74*	−15·8
	Apr.	30·16*	−16·6
	Aug.	2·06*	−18·3
	Dec.	10·78*	−17·8
1995	Jan.	1·81*	−17·9
	June	6·13*	−16·6
	Dec.	31·78*	−17·9
1996	Nov.	18·85†	−18·0
	Dec.	16·80*	−17·6
1997	Jan.	26·75*	−17·8
	May	10·09†	−17·2
	July	21·05*	−17·3
	Sept.	9·93*	−17·5
	Dec.	20·82*	−17·2
1998	May	3·14*	−17·4
	July	9·05*	−17·1
1999	Dec.	28·81†	−17·4
2000	Apr.	10·15†	−17·6
	July	20·09†	−17·1
	Sept.	20·99†	−17·2
2001	Jan.	11·71†	−17·8
2004	Sept.	6·04†	−17·0
2014	Nov.	23·92†	−17·1

*Haute-Provence *Coravel* †Cambridge *Coravel*

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CORRESPONDENCE

To the Editors of 'The Observatory'

On Cosmogony and Cosmology

In a recent book review in this *Magazine*, David Hughes[1] defines cosmogony as "the study of the origin and evolution of planetary systems" and cosmology as "the study of the origin and evolution of the Universe as a whole". In so doing, he neatly encapsulates the current usage of the words among astronomers, but it is not the usage of other people and has not always been the usage of our profession.

First, the usage of other people: in the *Oxford Companion to Philosophy*[2] there are entries for both words which make them almost synonymous with each other. "A cosmogony" this source says "is an account of the origin or creation of the universe" whereas we are told in the entry for "cosmology" that "since the advent of Einstein's general theory of relativity, the term has almost exclusively referred to the endeavours of physicists to understand the large-scale space-time structure of the universe on the basis of that theory." Both entries go on to mention the "big bang" and the entry for cosmogony makes no mention of the origin and evolution of planetary systems.

Consider now the historical usage among astronomers. Somewhat over sixty years ago, as a first-year undergraduate, I was taught that 'cosmology' referred to the philosophical speculation about the origin and meaning of the Universe, and that the scientific study of the origin and evolution of the Universe was 'cosmogony'. I admit that, even then, this seemed a strange distinction to me, although it gains some support not only from the entries in the *Companion to Philosophy* already cited but also from the much earlier *Oxford English Dictionary*[3] which records the usage of the early 20th Century. On the other hand, I was aware that Hubble had written a book with the title *The Observational Approach to Cosmology*[4] and that Hoyle, Bondi, and Gold were at that time strongly advocating their steady-state cosmologies. Hoyle, indeed, in his 1950 book *The Nature of the Universe* speaks of the "New Cosmology" and even says at one point "The origin of the planets is one of the high points of the New Cosmology"[5], thus further blurring the distinction between cosmology and cosmogony. Two years later, Bondi published a book with the simple title *Cosmology*[6] — consistent with Hughes' definitions, but not with the distinction that I had been taught in the previous year!

Sir James Jeans published four books with the word "cosmogony" in the title, all within the decade 1919–1929. The first, the classic *Problems of Cosmogony and Stellar Dynamics*[7] was concerned with the stability of rotating fluid masses and the possibilities of applying the results of those studies to five topics: the origin of the Solar System (in which the tidal hypothesis of Chamberlin and Moulton was preferred to the Kant–Laplace theory), the origin of binary stars by fission of a rapidly rotating single star, the formation of spiral nebulae, recognized as probably external galaxies, the formation of planetary and ring nebulae, and the formation of star clusters. The second book, the Halley lecture for 1922, *The Nebular Hypothesis and Modern Cosmogony*[8] covers much of the same ground and again concludes that the tidal theory of the origin of the Solar System is to be preferred over the nebular hypothesis of Kant and Laplace. The other two books both appeared in 1929; they were: *Eos or the Wider Aspects of Cosmogony*[9] and *Astronomy and Cosmogony*[10]. The former was based on a semi-popular lecture to the Royal Society of Arts. In an 88-page book, four pages were devoted to 'The birth of the planetary system', again favouring the tidal theory. The rest of the book was devoted to a discussion of what we would call cosmology. The latter dealt with much the same topics as Jeans' 1919 book. It is clear that Jeans used one word, *cosmogony*, to cover topics that we now use two words for, in the way described by Hughes. It is of interest that Jeans' contemporary and rival, Eddington, tended to avoid the use of either word.

The terms seem to have had fluid meanings, even among astronomers. The last two syllables of 'cosmogony' come from the same Greek root as the word *genesis*, so my teacher (T. B. Slebarski, at St. Andrews) may well have been partly right in suggesting that those who now call themselves 'cosmologists' ought to style themselves 'cosmogonists', but I do not think they are very likely to change their self-description now. That cosmogony has now come to mean (to astronomers) the study of the origin and evolution of planetary systems simply reflects the time when the only planetary system then known was considered to be "the cosmos" — the fixed stars being supposed all to be located in a thin outer shell at the boundary of the system. Just when astronomers settled on the present division of the meaning of the two terms is unclear; perhaps Hubble's book was influential in leading to the choice of cosmology rather than cosmogony for "the study of the origin and evolution of the Universe as a whole".

Yours faithfully,
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2014 October 13

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REVIEWS

Gravity: Newtonian, Post-Newtonian, Relativistic, by E. Poisson & C. M. Will (Cambridge University Press), 2014. Pp. 780, 25 × 19·5 cm. Price £50/$85 (hardbound; ISBN 978 1 107 03286 6).

This is a monumental volume on Newtonian and Einsteinian gravity which, within powerful approximation methods, treats realistic problems like planetary motions, the timing of pulsars, and gravitational waves from astrophysical systems. It is written by two leading experts in the field, with one of them, Cliff Will, being a pioneer and, for many years, leading expert in research on the border-line between General Relativity and experiment; the other, Eric Poisson, belonging to a younger generation of prominent relativists involved in research on equations of motion, self-force, black holes, *etc.* Both are the authors of influential books: *Theory and Experiment in Gravitational Physics* (revised edition; Cambridge University Press, 1993) by Cliff Will, and *A Relativist's Toolkit — The Mathematics of Black-Hole Mechanics* (Cambridge University Press, 2004) by Eric Poisson.

In contrast to numerous well-known textbooks on General Relativity, the authors devote the first three chapters (almost 200 pages) to a detailed and very useful treatment of its first approximation: Newton's gravity! The exposition is finely pedagogical, quite elementary when needed though written from a broad perspective, and even with a sense of humour ("Poisson's equation, known after its originator Simeon Denis Poisson, who unfortunately is not related to either author of this book ..."). Not only are the basic concepts associated directly with gravity, like mass, momentum, centre-of-mass, and virial theorems, first introduced, but also basic laws of thermodynamics, including equations of state of hot and degenerate matter, properties of polytropes, isothermal spheres, *etc.*, are analyzed. More sophisticated topics come soon after: the usual tools such as spherical harmonics and symmetric trace-free tensors are well explained and employed in the treatment of the equations of motion for isolated bodies, in particular for binaries, including spin dynamics. Parts on rotating bodies cover Jacobi ellipsoids and Maclaurin spheroids before going on to more general cases of the theory of tidal deformation. In the last 'Newtonian' chapter one learns about orbital dynamics, a perturbed Kepler problem, the Kozai mechanism, and tidal interactions. In a brief discussion of the N-body problem we encounter also "the dark matter hypothesis" in connection with the binding of star clusters.

The following two chapters, on Minkowski and curved space–time, contain, on 100 pages, the basic wisdom of Special and General Relativity, covered in most books on relativity in a greater detail but here explained with a remarkable simplicity and lucidity.

The next five chapters (245 pages) represent the core, and are, when compared with other recent books on relativistic gravity, the most unique aspects of the volume. They start with the post-Minkowskian theory and its implementation in near- and wave-zones of general asymptotically flat systems, and go over to a systematic development of the post-Newtonian theory valid in the near-zone, including a detailed exposition of the post-Newtonian fluid hydrodynamics. This is then employed in the treatments of configurations of moving isolated bodies, their structure, inter-body metric, and the equations of motion, in particular, for the case of binaries. As in previous chapters in their corresponding contexts, conservation laws, virial identities/theorems are formulated and the theory is extended to bodies with spins. Finally, post-Newtonian gravity is applied to the problems of celestial mechanics, astrometry, and navigation.

Two extensive chapters are devoted to gravitational waves. The basic properties (polarization, appropriate gauges, effects on test particles) are first discussed, and the quadrupole formula derived and applied for binary systems, for a 'mountain' on a rotating (neutron) star, and for stellar encounters. Corrections to the quadrupole formula of higher orders in (v/c) are calculated in great detail. Radiation from higher time-dependent multipole moments is also analyzed and evaluation of integrals representing the wave tails is provided. In the second chapter on waves a delicate issue of radiation reaction is treated, first in electromagnetism and then in gravity. Radiative losses from moving systems are demonstrated by examples of binary pulsars and in-spiralling compact binaries, including even a brief remark on a 'kick' (recoil) a resulting black hole may gain due to the radiation of linear momentum. The issue of an appropriate gauge to express the radiation reaction force is emphasized and the discussion of orbital evolution under radiation reaction over long time-scales concludes the chapters on gravitational waves.

The last 60 pages contain a brief overview of alternative theories of gravity, again more popular at present, inspired by the 'stringy' developments in theoretical physics and dark matter and energy observations in astronomy. The authors introduce the well-known framework of 'parameterized post-Newtonian metric' (in the development of which Cliff Will played a major role) and treat experimental tests of gravitational theories within this framework. The properties of gravitational waves are specified for a given theory as well as their effects on a laser interferometer. Finally, a more detailed discussion (involving the post-Minkowskian formulation, the near- and wave-zone solutions) is dedicated to the general scalar–tensor theories which include the Brans–Dicke theory as a special case.

The approach of the authors bears the seal of their main goal — to present Einstein's theory as a 'practical' and efficient, albeit not simple, framework for dealing with real problems of Nature. Sometimes, in my view, this attitude leads to somewhat misleading statements. In the bibliographical notes on p. 692, for example, we read: "An alternative approach to the description of radiative losses in general relativity, widely considered to be more rigorous and convincing than the Landau–Lifshitz approach adopted here, was formulated by Bondi and his colleagues … . Though different, the Bondi and Landau–Lifshitz approaches yield identical results." However, it is not just a question of rigour. In an interview for the *Czechoslovak Journal of Physics A*, published in 1969, Roger Penrose qualified the paper by Bondi *et al.* as the most important paper in new developments of General Relativity. Indeed, it led to a deeper understanding of gravitational waves, it inspired the geometrical, gauge-independent approach, the use of global techniques in relativity, and indirectly it led to the discovery of the Kerr metric, *etc.* In the end it will give the same result for the quadrupole formula but it gives and inspires much more. (It is perhaps worth noting that, nevertheless, the first author describes and employs the Bondi–Sachs mass formula in his *Relativist's Toolkit*.)

With some reservations about statements like that, I repeat what I wrote at the beginning of this review: this is a monumental work on gravity. The text is interwoven with 61 individual 'boxes' (following the example of the 'biblical' *Gravitation* by Misner, Thorne & Wheeler); for example, there are boxes on spherical harmonics, on the Clairaut–Radau equation and Love numbers, on Post-Newtonian transformations, *etc.* Each chapter starts with an extensive abstract which puts the chapter into the context of other parts of the book. These introductory remarks are often written with interesting historical connections.

I found the exercises at the end of each chapter extremely useful. The solutions are quite explicitly indicated (though not given), in contrast to, say, problems included in Jackson's *Classical Electrodynamics*. A simple example: for spherical static stellar configurations one can write down the exact relativistic equations of equilibrium. One can, of course, also treat this problem within post-Newtonian theory. In the exercise 8.6 (p. 412) it is shown that it is a non-trivial task to relate those two approaches.

This 'Gravity' book should be on the shelf of not only those relativists applying Einstein's theory to astrophysical and astrometric phenomena, but also of the 'Classical and Quantum Gravity' pure theorists so that they may see the muddy currents flowing in the deep relativity river. — JIŘÍ BIČÁK.

The Weight of the Vacuum: A Scientific History of Dark Energy, by Helge S. Kragh & James M. Overduin (Springer, Heidelberg), 2014. Pp. 113, 23·5 × 15·5 cm. £44·99/$54·99 (paperback; ISBN 978 3 642 55089 8).

This is an exceptionally good, short guide to the history of physicists' understanding of the energy of empty space. The dark energy of this book is 'dark' not simply in the sense that it does not interact with electromagnetic radiation, but in the deeper philosophical sense that we are in the dark in more ways than one: its properties are in the realm of unknown unknowns. During the 20th Century the concepts of 'ether' and 'vacuum energy' underwent several phase changes. They are documented clearly in this account, which is arranged in two halves: before and after 1964.

The advent of quantum theory transformed the debate on the nature of the vacuum. The zero-point energies of field theories swept aside the *pneuma* of the Stoics as well as the ethereal world-view of the Victorian era. In 1911 Max Planck introduced zero-point energy, which he admitted was a ghostly entity, outside the scope of classical physics. Others from the German school of quantum theory, notably Walther Nernst, Wilhelm Lenz, and Emil Weichert, developed hypotheses in which a medium remained after the removal of all matter from space. Everything changed in the 1930s with Einstein, Lemaître, and the cosmological constant Λ, as well as Hubble's discovery of a linear correlation between the recession velocities and the distances of galaxies. Einstein began to speak of "empty space" having physical properties. Lemaître always regarded Λ as a vacuum energy, in part inspired by his former mentor Eddington. However, Lemaître's insight attracted little following.

The second half of the account opens with the accidental discovery of the cosmic microwave background in 1964, which was immediately interpreted as fossil radiation from the earliest Universe. Historians of science have established that the concept of an inflation era in the early Universe begins in the USSR in the late 1960s. An explosion of interest followed in the 1980s when inflation and the false vacuum became mainstream. But at the same time cosmologists tended to dismiss the Λ term. The great shock, and it was a shock, that convulsed cosmology in 1998 was the discovery of the accelerating Universe. That led to the emergence of a concordance cosmology in which the values of the fundamental parameters are known with exquisite precision. This is an excellent brief history of cosmology. I expect to cite it many times in my academic papers and books. — Simon Mitton.

Springer Handbook of Spacetime, edited by A. Ashtekar & V. Petkov (Springer, Heidelberg), 2014. Pp. 950, 25 × 20 cm. Price £314·50/$499 (hardbound; ISBN 978 3 642 41991 1).

At approaching 1000 pages, this is a splendid and very comprehensive review of the special and general theories of relativity and their applications, in a collection of about 40 articles by experts in the field. It begins with some fascinating historical development (including interesting notions such as the Lorentz contraction being the result of direct interaction with the ether), and covers mathematical foundations, applications in physics and astronomy, and status reports on research to unify General Relativity with quantum physics. The articles are varied in nature, from essays, some with a philosophical perspective, to very mathematical papers, with the result that the book will appeal to a wide variety of readers, from advanced undergraduates to experts in the field.

With a volume of this size, there is space for some excursions away from the consensus view, so there are some articles that are to some extent speculative, and this adds to the interest of those for whom the mainstream material is well known. However, it is such a rich and varied volume that provides a view of relativity from so many familiar and unfamiliar angles that I doubt that there is any physicist who would not find something new and interesting here. — ALAN HEAVENS.

Particles and Astrophysics: A Multi-Messenger Approach, by Maurizio Spurio (Springer, Berlin), 2015. Pp. 491, 24 × 16 cm. Price £67·99/$99 (hardback; ISBN 978 3 319 08050 5).

Author Spurio has based this book on a lecture course given at the University of Bologna on astroparticle physics and acknowledges significant input from colleagues and students. It is aimed at PhD students, postdocs, and particle physicists who have developed an interest in the Universe, and correspondingly Spurio has made an effort to derive radiation and other processes to first order and to provide back-of-envelope checks on complex calculations.

Particles and Astrophysics is an enormously likeable book. It uses colour just where needed, to distinguish curves and points that are close together in graphs, to point to specific parts of apparatus, and to colour-code more-than-two-dimensional data. The units are cgs throughout (a brave choice for a European these days!), and most items are up to date as of about 2013. One exception is the discovery rate of supernovae, which now greatly exceeds his 10–30 per year. The book deliberately does not cover much of anything softer than a GeV and excludes gravitational waves and dark energy. Among the "messengers", cosmic rays, GeV and PeV photons, muons, and neutrinos receive significant attention, and active galactic nuclei, supernovae and their remnants, and the Big Bang and dark matter among the "senders". Receivers, variously called experiments, telescopes, and so forth, are also featured.

Lots of "aha!" items appear: the word "shower" for cosmic-ray secondaries came from Patrick Blackett as a translation of the Italian "*sciame*" of Bruno Rossi. And I, at least, needed to be reminded that the outer Van Allen belt contains energetic electrons and the inner one both electrons and protons. History gets brief, but I think, fair attention within the framework of citing mostly review articles and not original papers.

The English is not quite idiomatic ("In fact, the HiRes spectrum was compatible with the existence of a UHECR suppression, while the AGASA spectrum did not."). Some items are missing, though not critical to the main discussions (for instance, that the commonest sort of galaxy is the dwarfs). A glossary of acronyms is badly needed. The knee and ankle of the CR spectrum in Fig. 3.8 don't object to the *Auger Observatory* and *Telescope Array* points dripping from her toes, so why should we?

A few of the typos are classics. John Simpson is credited in the caption of Fig. 3.7 for a plot of relative abundances of the nuclides, but in the figure itself his points are called Sympson. One wishes this represented a 'Sympsonium' in his honour, but probably not. My favourite, however, is Table 9.1 of classes of sources known to emit TeV photons. These are three sorts of blazars (meaning AGNs with their jets pointed right at us). There are HIBL Lac type of blazar (typical examples Mkn 421 and Mkn 501), IBL Lac type of blazar (typical example BL Lac and W Comae), and LBL Lac type of blazar (with no examples at all, like Zwicky's type-VI supernovae).

Trimble is surely the last person you would think of in this context. And indeed she is the last person Maurizio thought of. The references are separated and alphabetized by chapter, so the very last citation (though Zatsepin, Zeeman, and Zwicky appear elsewhere in the book) is Trimble (1987) on dark matter. It is chapter 13, on top of everything else. — VIRGINIA TRIMBLE.

Annual Review of Astronomy and Astrophysics, Volume 52, 2014, edited by S. M. Faber, E. van Dishoeck & J. Kormendy (Annual Reviews, Palo Alto), 2014. Pp. 705, 24 × 19·5 cm. Price $246 (print only for institutions; about £152), $96 (print and on-line for individuals; about £60) (hardbound; ISBN 978 0 8243 0952 7).

"Wondering about things" is something that surely all astronomers do, and the first chapter in the 2014 *Annual Review* tells us what theoretician George Field has been contemplating — and doing — for the last 60 years or so. It's a lively account in two parts, the first directed at practising astronomers and the second towards a more lay readership, although in truth both parts are eminently readable by all.

The Sun's influence on planet Earth is of vital concern to us all (astronomers and laymen alike), so the chapter by Paul Charbonneau on 'Solar dynamo theory' is an important contribution to the climate debate as well as to stellar physics. Perhaps the only other contribution this year to a local theme is by Gordon Ogilvie on 'Tidal dissipation in stars and giant planets', which is of relevance to the satellites of the giant planets in the Solar System.

'Cosmic star-formation history' is reviewed by Piero Madau & Mark Dickinson in a wide-ranging article which will be of especial interest to cosmic chemists, while the discussion of 'Observational clues to the progenitors of type-Ia supernovae', considered by Dan Maoz *et al.*, confirms what Steve Fossey said in his talk at the RAS on the supernova in M 82 about the likelihood of a double-degenerate origin (see **134**, 310, 2014). For SNe created by more massive stars, the paper by Nathan Smith on 'Mass loss: its effect on the evolution and fate of high-mass stars' will be required reading.

With γ-ray astronomy now an established element of the observational tool-box, articles on 'Short-duration gamma-ray bursts' (most probably from compact-object mergers) by Edo Berger, and on the 'Gamma-ray pulsar revolution' by Patrizia Caraveo will be compulsory reading for members of the high-energy astrophysics community.

Moving on to the grander scale, the evolution of galaxies occupies a major part of the present volume, with a review on 'Evolution of galaxy structure over cosmic time' by Chris Conselice, and one on 'Far-infrared surveys of galaxy evolution' by Dieter Lutz; and for those objects with black holes at their hearts we have 'The coevolution of galaxies and supermassive black holes' by Timothy Heckman & Philip Best, and 'Hot accretion flows around black holes' from Feng Yuan and Ramesh Narayan, both of which demonstrate how firmly embedded in the presently accepted scheme of things are those invisible entities.

Finally there are two papers of more general interest: one is by Mark Reid & Mareki Honma on 'Microarcsecond radio astrometry', which takes our measurements of distance way beyond the Milky Way, and the other, by Luis Lehner & Frans Pretorius, on 'Numerical relativity and astrophysics', where numerical methods are required to model extreme effects of gravity.

So once again we have a treasure trove of knowledge to bring newcomers and more established astronomers up to date and to the cutting edge of research. — DAVID STICKLAND.

Advanced Interferometers and the Search for Gravitational Waves, edited by M. Bassan (Springer, Heidelberg), 2014. Pp. 387, 24 × 16 cm. Price £90/$129 (hardbound; ISBN 978 3 319 03791 2).

This is an interesting book which bridges the gap between popular articles and treatises on gravitational waves, and the highly specialized articles on aspects of potential gravitational-wave sources and the detectors being designed to search for them. It quickly corrects the impression that the laser interferometers — *LIGO*, *Virgo*, and *GEO 600* — were expected to detect sources in their initial configurations, and explains very clearly the rationale for the upgrades to those detectors currently underway. Further, it emphasizes the progress in the field, in that collaboration between research groups has replaced competition, and that the field as a whole has become much more out-going and sees the importance of gravitational-wave detection in the context of multi-messenger astronomy.

Given its origin as a write-up of subjects dealt with at a summer school associated with the French–Italian *Virgo* collaboration, it is not surprising that this is really a book aimed at young keen researchers starting out in the field of gravitational-wave detection.

However, it will also be of interest to experimental physicists working on the limitations to fundamental measurement. While the reader will need to know the basics of sources and detectors of gravitational waves using laser interferometry before delving into it, there is much to be learned from the book. This ranges from the well-established noise sources which limit such interferometers, such as thermal noise, photoelectron shot noise, and seismic noise, through the more practical problems that are encountered due to the presence of scattering of light in the interferometers, to the optical methods which may be used to help bypass the apparent limitations set by the Heisenberg Uncertainty Principle. The possibility of instabilities at high laser power due to parametric coupling between the modes of the light field and the mechanical modes of the masses is an interesting challenge for the experimenters to tackle.

The section on advanced detectors is expanded by discussion of a potential future detector in Europe (the *Einstein Telescope*), which is likely to combine systems of different design for low- and high-frequency sources, and cryogenic temperature for reduction in thermal noise, which is being considered for the lower end of the spectrum. This couples well into planning for further upgrading the new advanced detectors in the USA and Europe.

Reading this book will convey the excitement of the field, which is underpinned by real experimental progress! — JIM HOUGH.

Opacity, by W. F. Huebner & W. D. Barfield (Springer, Heidelberg), 2014. Pp. 572, 24 × 16 cm. Price £153/$229 (hardbound; ISBN 978 1 4614 8796 8).

We all know something about opacity; smoke, dust, and moisture constantly change our view of the world around us. In astronomy, opacity has a profound effect on physical structures and so its correct evaluation is crucial to the construction of realistic models of, for example, gas clouds, stars, planets, supernovae, quasars, the intergalactic medium, and even emerging structures in the early Universe. Remarkably, until now, one would have found no single textbook dealing with the topic. Heubner & Barfield's *Opacity* changes that.

My formal introduction to opacity began with the commencement of a PhD under the guidance of Dick Carson, one of the true pioneers of opacity calculation; it has continued in one way or another to the present. The phenomenal advances in theory and computation made since the 1980s have changed the subject beyond recognition, so that Huebner & Barfield have had the luxury of reviewing a mature and robust science, yet one that is highly contemporary. The authors approach their subject in a comprehensive and thorough manner. They set the historical context, and cite all of the major milestones. A refresher on radiative transfer and the definitions of opacity, which to use, and when, follows. Various models for atomic and molecular structure and for the equation of state are introduced. Since the variety of these is perplexing, the authors have taken care to describe the choices available, and explain when each may be used, rather than to repeat detail which can be obtained elsewhere. A thorough and well-researched bibliography identifies the requisites to proceed. The guts of the book (240 pp.) are concerned with the computation of radiative cross-sections and their contribution and use in opacity calculations. Molecular opacities are given substantial attention (good for cool stars and planets), and electron conduction is included (good for red giants, white dwarfs, and also planets). Five short closing chapters deal with practical considerations, such as mixtures, useful approximations and interpolation schemes, uncertainties, experiment (always good to check!), and special cases. Five appendices demonstrate the overall care which has been taken with the preparation of the text, and provide starting points for the entrepreneur wishing to generate their own opacity data.

As the authors note in their introduction, reviews of opacity calculations have been thinly spread since the birth of the subject. Here, the authors have grasped an enormous and not particularly glamorous topic. They have laid out the necessary components in a well-organized and carefully prepared manuscript which is pleasing to the eye, and relatively easy to read (though familiarity certainly helps). If the authors aimed to emulate the clarity of Mihalas' masterpiece on *Stellar Atmospheres* (1978), they have been largely successful.

Opacity acknowledges the many producers of atomic data. As a user, it is easy to forget the effort required to generate just one oscillator strength, let alone an entire ensemble for an opacity calculation. *Opacity* is a superb testament to that effort. It is a 'must buy' for any library concerned with atomic and molecular physics and astronomy theory, and a highly-recommended textbook for all students concerned with radiative processes. — SIMON JEFFERY.

Outstanding Problems in Heliophysics: From Coronal Heating to the Edge of the Heliosphere (ASP Conference Series, Vol. 484), edited by Q. Hu & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 272, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 852 7).

This volume is a collection of 40 articles contributed at the 12th Annual International Astrophysics Conference held in 2013 April at Myrtle Beach, South Carolina, USA. The range of topics is truly enormous, almost intimidatingly so, with papers on such subjects as solar energetic particles, solar-wind heating, and element abundances, but with most of them concentrating on the outer heliosphere and its boundary with the interstellar medium. Unfortunately, there is no categorization of contributions and there are no keynote review articles, which must have been difficult for the conference participants having to change gear as each paper was presented. It certainly did not help with the reading of the proceedings where the papers are given in alphabetical order of the first author's name. This is not to say that the contributions were uninteresting —

there are some that are well worth scrutinizing. Thus, there is still apparently controversy on whether the *Voyager-1* spacecraft, for example, has actually entered the interstellar medium in its journey through the Solar System, as was widely reported in late 2013. The contribution by Gloeckler & Fisk suggests that the spacecraft has instead entered the intermediate heliosheath region. The article by McKenna-Lawlor *et al.* summarizing the observations that the ESA *Rosetta* spacecraft has made on its convoluted journey to the nucleus of Comet 67P/Churyumov–Gerasimenko deserves attention.

There is a mixture of papers giving measurements of various parts of the heliosphere and those modelling the measurements, so observers and theoreticians are catered for. It is, however, a little hard to see how these proceedings will be an attractive buy for libraries, even though the publication has appeared only a year after the conference. Many of the more important papers (such as the Gloeckler & Fisk article mentioned above) have now appeared in refereed journals and are readily available. Moreover, the on-line editions of the journal papers have figures which are in colour, whereas in the present volume only very few figures are. There are a few misprints but nothing that detracts from the quality. — KEN PHILLIPS.

Introducing Astronomy: A Guide to the Universe, by I. Nicolson (Dunedin Academic Press, Edinburgh), 2014. Pp. 166, 19·5 × 16·5 cm. Price £9·99 (paperback; ISBN 978 1 78046 025 3).

Back in 1999, Iain Nicolson published *Unfolding Our Universe*, an introductory astronomy book that really was almost as good as the blurb on the back cover said it was. "Probably the most concise of the astronomy books you might ask a non-science student to read, *Unfolding* does an excellent job of introducing the vocabulary and ideas that will enable the reader to ask for more information and be taken seriously", said that well-known expert Trimble. Two other American and two British astronomers were equally complimentary, mentioning the excellent illustrations, clear style, and so forth. I had hopes that *Introducing Astronomy* would be a very similar, modernized successor. It is not.

The present volume is indeed colourfully illustrated and considerably updated, but it tries to cover more territory (the Universe has aged by only 10^{-9} of the Hubble time, but modern astronomy is at least 15% older) in much less space (fewer pages, and much smaller ones). Some things are very well done, for instance, the fuzziness of the habitable zone around stars. There is a fine glossary, but no index, and no multiverse, which, along with the proliferation of exoplanets is, I think, the most exciting now-respectable concept to arise in the last 15 years.

If the departmental CEO again decrees that I have to teach a short, non-major astronomy class, I will very probably ask the students to read *Introducing*, but with less pleasure and confidence than I invited an earlier generation to read *Unfolding*. — VIRGINIA TRIMBLE.

Introducing the Planets and Their Moons, by P. Cattermole (Dunedin Academic Press, Edinburgh), 2014. Pp. 142, 19·5 × 16·5 cm. Price £9·95 (paperback; ISBN 978 1 78046 029 1).

As its title suggests, this almost-pocket-sized book seeks (in the author's own words) "to give a flavour of what the larger members of our planetary system are like", and in so doing to inspire the reader to delve deeper. Of the ten published to date in the Dunedin series of *Introductions*, one already handles Astronomy, the others being concerned with the surface or atmosphere of Planet Earth. The concept of the *Planets* volume is noble and its attempt brave, since the price certainly brings these distillations of expert knowledge well within an affordable range. It would have been nice to assert unequivocally that the concept pays off in terms of descriptive content too, but unfortunately such a statement does have to be qualified rather heavily.

Rather than dedicating a separate chapter to each Solar System body, the author adopts the less-usual and certainly attractive scheme of comparing different properties — origins, orbits, magnetic fields, atmospheres, *etc.* — of the planets and their moons in separate chapters, thus enhancing their similarities and differences instead of merely cataloguing their properties planet by planet. The scheme has obvious benefits, but at the same time it lacks a place to tabulate the physical properties, and it takes a lot of hunting to ferret out even basic ones such as the mass of Mars. The author may (as he states) have attempted to minimize the use of technical terms, and a glossary defines some of those used, but his background in geology and rock chemistry has somewhat clouded his judgement of what the less-well initiated would regard as a technical term, with the result that undefined terminology involving rock chemistry is a little too present.

The contents of the book are both factual and speculative, but while some speculation cannot be avoided when describing the origins of the Solar System bodies, there is a tendency to present speculation as proven fact. The text dates from early 2014 so is well current. Illustrations are plentiful and fall into two categories: reproduced mission-based ones that are generally attractive, and figures created to illustrate points in the text, though several of the latter kind contain errors or lack adequate descriptions of ordinates or abscissæ.

The book is not unattractive, though the publication format is rather mean and I would have welcomed an extra inch of border around each page. The author clearly loves his subject, pursues it thoroughly, and is well attuned to its many details. My main criticism is levelled at a lack of care over proof-reading the text and in checking various statements, particularly numbers and units. Units oscillate between kilometres and miles, km/s and kps, m/s, m s^{-1} and m/s^{-1} [*sic*]; ages vary from Gya to Ga (and are expressed as a chronological age rather than as years 'ago'); some quantities lack definition, a few explanations are muddled or wrongly stated, some figures cited are not there, and the occasional unintentionally humorous typo has slipped in. The style of writing is clumsy; some sentences are contorted, and far too many commence with "This". A course in 'Writing scientific prose and proof-reading' could have taught the author how to rectify most of those shortcomings and produce a book that was as truly useful as he intended. — Elizabeth Griffin.

Planetary Rings: A Post-Equinox View, by Larry W. Esposito (Cambridge University Press), 2014. Pp. 246, 25 × 18 cm. Price £75/$120 (hardbound; ISBN 978 1 107 02882 1).

Our knowledge of the ring systems of the Solar System's gas-giant planets has increased by leaps and bounds recently due to the extended *Cassini* orbiter mission to Saturn and the *New Horizons* flyby of Jupiter. The *Cassini* spacecraft went into orbit around Saturn in 2004 July, and the mission was extended by two years in 2008 (this extension being called *Equinox*) and by a further four years in 2010 (called *Solstice*). *New Horizons* received a gravitational assist from

Jupiter, setting it on its way to Pluto. In early 2007 images were taken of the Jovian rings from a range of distances and under different lighting conditions.

The first edition of this excellent overview of planetary ring characteristics was published in 2006. It has been thoroughly updated by its author Larry Esposito, a professor at the University of Colorado, a principal investigator on the *Cassini* mission, and a world expert on rings (he also discovered Saturn's F ring in 1979). The book is superbly and colourfully illustrated, beautifully produced, rigorous, well referenced, clearly written, up to date, and a key resource in planetary science for any advanced university student or researcher.

The main thrust of the book is the structure and dynamics of all the known ring systems and the way they are affected by gravitational interactions with nearby moons. Much care is also spent explaining the interactions between the planet's magnetosphere and extended ionosphere and the electrostatically charged ring particles. The thickness and particle-size distribution in the rings is discussed in detail.

Planetary rings are important. All planets have or had them. They also provide an accessible laboratory for the phenomena that occur in stellar protoplanetary discs and thus are a vital clue as to the mechanisms responsible for planetary formation. Esposito delicately balances what we know about rings with the large list of still-to-be-solved mysteries. We might know what is on the surface of the ring particles but what is inside is still hidden from view. We might have a clue to the mass of Saturn's rings (around 6×10^{-6} the mass of Earth), but when it comes to the other planetary rings we are still guessing. Then there is the delicate problem of ring age, origin, and evolution. Here we have lots of theories but little certainty.

This excellent book provides huge encouragement to keep studying these fascinating astronomical phenomena. — DAVID W. HUGHES.

God's Planet, by Owen Gingerich (Harvard University Press, London), 2014. Pp. 170, 19 × 12 cm. Price £14·97/$19·95 (hardbound; ISBN 978 0 674 41710 6).

You will notice that the title of this book comes without a question mark. And this strongly reflects the opinion of the author, who is the emeritus professor of astronomy and the history of science at the Harvard Smithsonian Centre for Astrophysics. The question is simple. Is planet Earth just any old planet, a random collection of atoms and molecules that happens by chance to have the right physical and chemical characteristics conducive to sentient life, or does the development of homo sapiens, and the Universe (or multiverse) around us, point to design and purpose and fine tuning?

Gingerich's book is based on the three Herrmann lectures that he gave at Gordon College in Wenham, Massachusetts, in 2013 October. Those lectures concentrate on the relationship between science and religion. Some folk, of course, are convinced that this relationship does not exist, that science and religion occupy completely non-overlapping territories, and those folk go further in encouraging the participants in each endeavour to keep their respective noses out of each other's business. Gingerich strongly disagrees. He bases his argument on three things — the works and thoughts of Nicolaus Copernicus, and his introduction of heliocentricism in 1543, Charles Darwin and the influence of his 1859 masterpiece, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, and Fred Hoyle's startlingly intuitive breakthrough prediction of the correct resonance level of carbon, a characteristic necessary to explain carbon's required high abundance in today's Universe.

This book wonders why we live in a Universe that seems to be specifically and congenially designed and fine-tuned for intelligent, self-reflective life. Why is the cosmological Big Bang so similar to Genesis 1:3, “Let there be light”? Why is the Universe understandable? Gingerich is convinced that when we look through our telescopes and do our experiments and calculations we see a purposive Universe that seems to know that we are coming. He is convinced that what we see is indisputable proof of the planning and intentions of a Creator–God. To Gingerich the idea of a Universe without God is an oxymoron, a self-contradiction.

This is a deeply thought-provoking book written clearly and convincingly by a man who wears his erudition lightly. It is an absolute delight. — DAVID W. HUGHES.

Elephants in Space: The Past, Present and Future of Life and the Universe, by B. Moore (Springer, Heidelberg), 2014, Pp. 189, 23·5 × 15·5 cm. Price £19·99/$34·99 (paperback; ISBN 978 3 319 05671 5).

In context and scope, this book provides a mix of scientific information and personal views of the author. The mixture is open to cutting science with introspective thoughts bearing on life and modes of human existence. The author, Ben Moore, blends all this with astrophysics and does so in a chatty way.

Packed with information, the chapters include ‘What we know and how we know it’, as well as thoughts about the Big Bang, and the emergence of galaxies, stars, and planets. Moore believes there could exist thinking creatures like elephants on some extrasolar planets, which accounts for the title of the book. Elephants have huge brains and are non-technological. That extra-terrestrial intelligent or wise non-technological species exist is conceivable, as Dyson[1] pointed out a long time ago. This touches upon Factor Six of the Drake Equation and the relevant issue of whether such beings are typical of extra-terrestrial species or not.

Back to our planet. The author tackles the fog of circumstances around 30 000 BC, when human skills arose. He calls into question what it was that led to the transition from apes to human intelligence. Clearly, as he states, that is one of the great questions not yet answered. He then goes on to discuss events around 10 000 BC — the end of the Ice Age — with the emergence of organized, agricultural societies, which led to the appearance of megaliths and Neolithic astronomy at Nabla Playa in Nubia. Moore does not enter into the interpretation of the megalithic stellar alignments there. The site at Nabla Playa is subsequent to Göbekli Tepe, placed between 10 000 and 9000 BC and where the central pillar includes a pictogram of the Half Moon.

The author also refers to Babylonia and Buddhism. He seems to not know of *Enuma Elish* — Babylonian literature about the reorganization of the cosmos, by Marduk, and the establishing of stars. Moore only mentions Marduk as creator of dry land. He also passes over Buddhist cosmologies, which include the Cakravala or Single World System and Sahasra cosmology of countless numbers of worlds.

The author seems more at home with some of the cosmological speculations of Thales of Miletus and Democritus of Abdera. In different places of the book, but spread out, he cites other philosophers — Parmenides and Zeno, for example. All this builds up to a mass of interspersed information that brings into focus Copernicus, Bruno, Galileo, Newton, and successive astronomers and astrophysicists. From those sources the author steps to the circumstances of contemporary times, stating that there is no grand meaning being human beings. He juxtaposes this with the laws of the Universe and life as we know it. His opinion hinges on whether our species is freak or not.

In summary, it's a rather unusual book that has a number of catchy viewpoints. — P. CHAPMAN-RIETSCHI.

Reference

(1) F. Dyson, *Disturbing the Universe* (Harper & Row, New York), 1979.

The Protos Mandate, by N. Kanas (Springer, Heidelberg), 2014. Pp. 142, 23·5 × 15·5 cm. Price £15/$19·99 (paperback; ISBN 978 3 319 07901 1).

This book is one of the *Science and Fiction* series being published by Springer which aims to combine a reasonably plausible science-fiction story with an extended account of the science behind it. Here, the latter (Part II) begins with a thorough survey of the treatment of space travel in fiction, from Kepler's *Somnium via* early Solar System journeys to many accounts of interstellar travel, the last including different ways of coping with the long durations of the journeys to even nearby stars. Most popular were the multigenerational narratives, in which there were enough crew to produce descendants who would keep the spacecraft systems going and eventually reach the destination; but some authors preferred to put their travellers into suspended animation for most of the journey. Kanas also surveys the psychological and sociological issues presented by multigenerational voyages, including the social engineering to keep the population at the right level and maintenance of genetic variation. There are the questions of the retention of the objective of the mission through multiple generations by descendants who may not feel bound by the commitments made by their ancestors at the beginning of the mission. Other topics surveyed include propulsion systems for interstellar flight, of which there has been a lot of work over the years, and suspended animation, which is still beyond us. All the surveys are well referenced in a comprehensive bibliography.

The story itself (Part I) is a straightforward account from shortly before take-off to initial settlement on a distant planet. Despite references to global warming, and the inclusion of a small number of women in senior positions, the book is rather old fashioned with good guys, bad guys, and laser pistols. The people in charge are wise and benevolent. For complex characters having mixed motives, or a story line with twists and turns, you should look elsewhere. The most interesting problem is the one we are left with at the end: how the new colonists will interact with the dominant native life-form, something rather like slime mold — to which we are introduced in Part II. — PEREDUR WILLIAMS.

Incoming Asteroid! What Could We Do About It?, by Duncan Lunan (Springer, Heidelberg), 2014. Pp. 390, 23·5 × 15·5 cm. Price £35·99/$39·99 (paperback; ISBN 978 1 4614 8748 7).

I am not sure whether our near future is more in danger from impacting asteroids or comets, or from the effects of chopping down trees to produce the flood of books on the subject.

Duncan Lunan, the Scottish astronomy, spaceflight, and science-fiction writer is clearly very worried about the consequences of the next big impact. He starts by reviewing past damage. Much is made of well-known hits such as Tunguska, Chicxulub, Barringer, Chelyabinsk, Giordano Bruno, and Manicouagan. This is followed by a progress report on our attempts to assess the present asteroidal and cometary population and specifically recognize those that are 'on their way in'. The main thrust of the book, however, concentrates on what we do when we recognize our nemesis.

When the body that has our name on it is known, and the date of the hit is fixed, we clearly have three potential actions. We can, ostrich like, stick our heads in the sand, and hope it will go away; we can be gung-ho and zoom out there into near space and blow the offending object to bits; or we can be more gentle and divert its path so that it sails by. Lunan then discusses the obvious main problem. We have very little knowledge of the interior makeup and strength of our enemy. So we have no idea what would happen if we detonated a nuclear device in its vicinity. Maybe the best plan is diversion. Here we read of 'the red line', the track of the predicted impact point across the Earth's surface, and we revel in details concerning mass drivers, gravity tractors, mirror evaporators, solar sails, asteroid tugs, and manned missions.

The book ends with politics. And this is a huge problem. It is clear that being prepared to eradicate a known threatening impactor would be hugely expensive. And the time-scale is all wrong for governments. The politician's outlook is extremely short term and is completely ineffective when it comes to problems that might crop up every 10 000 years or so. So we are left with the inexpensive, and nugatory, plan B. We fund a few astronomers (for example IWAN, an International Asteroid Warning Network), we research the impactors by establishing a 'Near Earth Object Threat Mitigation, Mission Planning and Operation Group' (MPOG), and we cosily discuss the 'politics of survival'. Then most of us follow the ostrich and put our heads back into the sand.

Lunan writes engagingly and has produced a well-illustrated, well-referenced, and highly readable tome. What I enjoyed especially was the multiple references to the works of science-fiction writers on the subject. — DAVID W. HUGHES.

Holy Sci-Fi: Where Science Fiction and Religion Intersect, by P. J. Nahin (Springer, Heidelberg), 2014. Pp. 224, 23·5 × 15·5 cm. Price £15/$19·99 (paperback; ISBN 978 1 4939 0617 8).

Neither science fiction nor religion is particularly easy to define. The author begins by saying that he is not a religious person, "in the sense of believing in a supreme being who is the ultimate cause of the world we immediately live in, or of the universe at large in which our world is but an extremely small part". As for science fiction, we are given a quote from John R. Pierce* who wrote "Science fiction bears the same relation to the world of science and technology that legends of the saints do to the Christian religion." My favourite, hard-core, definition came from Isaac Asimov, who wrote (somewhere) that science fiction explores the consequences of some technology that we do not have, but might one day, and that its primary purpose is to accustom us to the idea that everything is going to change.

Most of the 78 short stories and dozens of novels mentioned in *Holy Sci-Fi* have at least some aspects of both, though there are stretches into fantasy (very crudely, the technology could never exist, unassisted thought transference, for instance, though I would not rule out the breeding of dragons), and also into

*A 1936 Caltech PhD in Electrical Engineering, later a director of research at Bell Labs, who wrote science fiction. That he did so under the pen-name jj coupling and that Simon Ramo received an EE PhD from Caltech the same year are part of the 'extra value' of this review not contained in Nahin's book. Ditto for the factoids that Pierce was occasionally to be found, later in life, lunching at Caltech, that he was very bad at recognizing faces, and had been very near-sighted as a child.

sorts of philosophy that you might not think of as religious (what to do about first contact with aliens). Lots of the people you (already a sci-fi fan) expect to find are here, from Poul Anderson to John Wyndham (whose *Midwich Cuckoos* is the focus of a good Marilyn Monroe story), and quite a few you might not expect, from Alighieri (whose *Inferno* is suggested as the first theological SF story) to William Wordsworth (an admirer of rainbows), *via* English biologist, J. B. S. Haldane, said to have been the model for the evil physicist Weston in C. S. Lewis' *Out of the Silent Planet* (first volume in a multi-genred trilogy otherwise unmentioned by Nahin).

Is there some astronomy in *Holy Sci-Fi*? A bit, since Camille Flammarion is criticized for putting Capella 71 LY away from us instead of 42 LY in his 1887 novel *Lumen*, "You'd think an astronomer wouldn't make a mistake like that!". It is left as an exercise for the reader to find out the best parallax known for Capella in 1887, and also to calculate the brightness of the night sky for an observer at the centre of a globular cluster (in connection with Asimov's story *Nightfall*).

Topics tackled much more thoroughly include religious robots, computers as gods, and time travel, especially time travelling to Jesus (who, or which, invariably turns out to be quite different from what the various protagonists expected). Nestling somewhere between time travel and cosmology live multiverses, especially the many worlds of Hugh Everett III. None of my favourite short stories in which people (*etc.*) move between adjacent spokes of the divergent fan appear, but in connection with some of the others, Nahin says that worlds split "at every decision by every sentient being in the universe." In fact, splits also occur every time a radioactive atom decays, even if there is no cat there to observe it.

Nahin's target reader is probably someone who has read more science fiction, and perhaps more theology, than I have, but even at the beginner level one finds bits one wants to fix. Herewith four examples: (*i*) Merton Mansky in Asimov's *Bicentennial Man* was undoubtedly named, as the author says, in honour of Marvin Minsky, but I suspect input also from Robert Merton. (*ii*) "The project is called 'His Master's Voice' (HMV) because the name is ambiguous as to which master we are to listen to, the one from the stars, or the one in Washington" (*re* a Stansiław Lem story). But HMV is also a gramophone slogan that goes with the picture of the dog sitting, ear cocked, by an exponential horn. (*iii*) "Caesar Augustus once said he'd rather be a pig than a child in the House of Herod". The story is better told by Robert Graves in *I, Claudius*, where the remark is he would rather be Herod's pig than Herod's son, because, as a Jew, Herod wouldn't kill the pig for food, while he had just killed his son (for other reasons). (*iv*) Frank Drake's Project Ozma, said to have been named "after the imaginary land of Oz", rather than after Princess Ozma, the rightful ruler, temporarily displaced by the Wizard.

Conflicts of interest? Well, I just missed meeting Paul Nahin, now Professor Emeritus of Electrical Engineering at the Universities of New Hampshire and Virginia, because he left Caltech with an MS in 1963, the year before I arrived there. The editorial board that selects the volumes for this Springer *Science and Fiction* series included Gregory Benford, Professor Emeritus of Physics here at UC Irvine, two of whose very short stories appear as appendices to the present volume. Both originally appeared on the back page of *Nature* and deal with coded messages found in the cosmic microwave background radiation and a data stream from *LIGO* (the *Laser Interferometric Gravitational Observatory*). — VIRGINIA TRIMBLE.

Other Books Received

Ensuring STEM Literacy: A National Conference on STEM Education and Public Outreach (ASP Conference Series, Vol. 483), edited by J. G. Manning, M. K. Hemenway, J. B. Jensen & M. G. Gibbs (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 461, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 850 3).

This is the proceedings of a conference held in San Jose, California, in 2013 July, aimed at bringing together American workers in science education and outreach to discuss the promotion of 'literacy' in science, technology, education, and mathematics (STEM). With the close involvement of the Astronomical Society of the Pacific, the meeting highlighted astronomical themes and will be of interest to those with a similar agenda outside the USA.

Numerical Modeling of Space Plasma Flows ASTRONUM–2013 (ASP Conference Series, Vol. 488), edited by N. V. Pogorelov, E. Audit & G. P. Zank (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 292, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 860 2).

The latest in the annual series of ASTRONUM conference proceedings contains the usual eclectic mix of about 40 summary papers on computational modelling of astrophysical and space-plasma systems, presented in Biarritz in 2013 July. Topics include plasma turbulence and particle acceleration, astrophysical plasmas including discs and jets, space-plasma simulations related to the solar corona, heliosphere, and planetary magnetospheres, and numerical methods and algorithms.

Here and There

A TIGHT SQUEEZE

… and a series of public lectures will be held inside the Plaskett telescope. — *Victoria Times-Colonist*, 2014 September 3.

POETIC LICENCE

I'd driven ten hours from Dubrovnik to learn about James Joyce's formative years living in a city light-years away from his native Dublin. — *Victoria Times-Colonist*, 2014 September 20, p. D10.

NOT REALLY THE WAY TO GO

Design flaws were carefully noted and incorporated into later vessels. — *Orange County Register*, 2014 September 30, p. News 6.