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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2015 May 8 at 17^h 00^m
in the Geological Society Lecture Theatre, Burlington House

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

The President. Welcome back everybody. This is probably a unique day in that every single meeting today has ended early [laughter]! I have a few prizes to announce. The 2014 Michael Penston thesis prize goes to Dr. Guido Pettinari of Portsmouth University for his thesis entitled 'Intrinsic bispectrum of the cosmic microwave background'. The joint runners-up are Dr. David Harvey of Edinburgh University and Dr. Boris Leistedt of University College London. The Keith Runcorn prize goes to Dr. Hannah Christensen of Oxford University for her thesis entitled 'Stochastic parameterisations and model uncertainty'. The runner-up is Dr. Grace Nield of Newcastle University.

On to the programme for the afternoon. I will hand over to Steve Miller for the announcement of the details of the consortia who will be receiving funding to support outreach and education programmes awarded under the RAS200 Sky and Earth programme. This is the major public-engagement project for the Society and marks the lead-up to its 200th anniversary.

Professor S. Miller. It's my great pleasure to be able to announce the winners of the first round of this competition, in which RAS members have decided to use a million pounds from the RAS reserves to support outreach and engagement with groups that we don't normally engage with. We are doing this because we want the RAS, by 2020, to be well and truly embedded in the wider society in which we find ourselves.

To give a quick background as to the timeline of the process: in February, the awards panel was faced with 92 outline proposals. We shortlisted 15, for which 14 full proposals were received. In April, the panel chose five full proposals, and one 'partial' proposal. All the winning organizations have been notified and nobody has said "no thanks". [Laughter.]

I am going to go through the winning proposals one by one to give a small amount of detail on what they are proposing. Firstly, 'The Planets 360', by the

National Space Centre (NSC), Leicester, in partnership with the Philharmonia Orchestra. I love Holst's *Planets* suite and the NSC are going to use this as the centrepiece with a new recording from the Philharmonia Orchestra for a full-dome experience, adapted not only for large venues, but also available for small, mobile planetaria.

The second award is to the Workers' Educational Association (WEA) for 'Open your eyes — look up to the sky'. The WEA is partnering with Liverpool John Moores University to start astronomy courses to improve the lives of the unemployed, the disabled, and people with low literacy. This will begin in North-West England and then be rolled out across the country. We hope this will help people who missed the chance for education when they were younger.

The next award is to the Prince's Trust, for 'Stars and Space': we wanted the RAS to work with some of the toughest groups to reach, and the Prince's Trust provides support for young people who are not in education, employment, or training. The Prince's Trust will include astronomy and geophysics activities in their 'outward-bound' residential courses for young people, as well as their well-established 'Get Started' programme.

Our next winner is — does one of the Welsh-speakers want to make this announcement for me? [laughter] — 'Astronomy and Geophysics through the traditional culture of Wales'. This is RAS scientists working with professional artists and performers and musicians in order to bring astronomy, geophysics, and space science into an artistic format in the Great Eistedfodds of Wales.

Then we have 'Stepping out: astronomy short breaks for carers': this is from Care4Carers, a Scottish-based organization — I don't think anybody has been able to make it down here today, but let's give them a round of applause. [Applause.] Being a carer is an incredibly tough job, and Care4Carers is a very important support organization in Scotland for such people. They are going to be working with the Royal Observatory Edinburgh.

Finally, we have what I call a 'meta-project', because it is going to work alongside the other winners. 'Space, Earth and Autism' is a proposal from the National Autistic Society (NAS), who will run a pilot project to work alongside the other RAS200 projects, to ensure that they include people on the autistic spectrum. This is certainly a challenging and interesting aspect to the whole RAS200 programme.

We introduced the RAS200 programme this time last year, and held a stakeholder meeting here at Burlington House in 2014 July, attracting nearly 100 people; we also held 13 Town Hall meetings in all parts of UK, and the Isle of Man. I have found out that my Freedom Pass does not work in the Isle of Man, as it is not in the UK! [Laughter.] I'd really like us to express our thanks to those who carried the load of this effort, including RAS staff, in particular Robert Massey and Sheila Kanani, and the members of the steering group and awards panel. [Applause.]

Initially, we had intended to dive straight back in with identifying a second round of projects, but we have decided to give it longer, and delay by 6–9 months to get our funded projects up and running, and set up evaluation and monitoring. We will therefore probably be looking to announce second-round winners by late 2016 or early 2017.

The President. I think we should give our thanks to Steve for running this project. [Applause.] After all the fun, it's now time for my Presidential Address. [A summary of the Presidential Address has appeared in *Astronomy & Geophysics* 56, 4.31, 2015.]

The President. I'd be happy to take a few questions in the last five minutes or so.

Mr. H. Regnart. Of course we need the new instruments but is there any possibility that the EU, Russia, or China might have a vehicle capable of servicing *Hubble* if its life can be extended?

The President. I think that's an interesting possibility. The US now has its new capsule, *Orion*, that is supposed to go beyond low Earth orbit. I cannot see a good reason why one could not go and service the *Hubble* again. There is just no intent. I could imagine that, in a few years' time if things start to degrade, there might well be another servicing mission.

Dr. J. G. Morgan. Is there a possibility of a fully robotic mission?

The President. I would like to think that robotic servicing was possible, but in reality I think we're a long way from that. That was one of the drivers that caused NASA to rethink. I went out to Goddard Spaceflight Center to watch the prototype robot unscrew a screw. It took three hours. On the particular box that they would have had to open up there were 132 screws. It became fairly obvious that although robotic technology is quite attractive in a lot of ways there are some things that it can't achieve in the near future. I don't know if you remember the 2009 servicing mission — there was a stuck bolt and an astronaut had to take a decision whether to turn it and allow it to break. In the end it didn't and they could put in the new equipment. I can't imagine a robot coping with that situation.

Dr. G. Q. G. Stanley. Considering the social impact *Hubble* has, how are we going to handle it when it fails?

The President. Fortunately, I won't have to handle it. NASA will have to, and it may lead them to decide to service it and keep it going until we can replace its capability. While I think *JWST* will be a wonderful facility, I don't think it will provide the iconic images that *Hubble* has provided. *JWST* is not going to provide such detailed images because it will be looking at very distant 'fuzzy blobs'. The detail in those images will tell us a huge amount about the history of the Universe but won't produce as many pretty pictures.

Professor D. W. Kurtz. It needs a boost as well as a servicing. The Shuttle could have done that fairly easily, but what about *Orion*?

The President. *Hubble* has a docking station installed because if they don't go back to service it they either have to bring it down or boost it anyway. There is a place to install a booster. You take one up and stick it on, I guess, is one way of doing it.

I see we have just passed 6 pm so I'll have to end it there. I remind you of the drinks reception in the RAS Library following the meeting. I give notice that the next A&G meeting of the society will be on Friday, 2015 October 9th. I hope to see you all there. [Applause.]

THE SMOULDERING OF THE HERTZSPRUNG–RUSSELL DIAGRAM

By Graeme H. Smith

University of California Observatories/Lick Observatory

“The issue of the naming of the diagram smoldered for years.”
 — DeVorkin¹ and Gingerich² in reference to the naming of the Hertzprung–Russell diagram.

Introduction

Within a period of two years, two diagrams appeared in the astronomical literature that would have a profound influence on the study of stellar astrophysics. In 1911 Ejnar Hertzprung³ published a diagram that plotted apparent visual magnitude *versus* a photographically-determined equivalent or effective wavelength for stars in the Pleiades and Hyades clusters. (The first appearance of such a diagram for the Pleiades had actually been published by Rosenberg⁴, a research assistant to Hertzprung.) In 1913 Henry Norris Russell reported⁵ upon the fruits of a long-term parallax programme that enabled him to produce a plot of absolute magnitude *versus* spectral class for bright field stars. Russell’s plot is referred to in the minutes of the 1913 June meeting of the Royal Astronomical Society published in *The Observatory*⁵, but is not included as part of those minutes. It appears in press in several of Russell’s papers of 1914^{6,7}. In addition, Eddington included a reproduction of it as early as 1914 in his book on *Stellar Movements and the Structure of the Universe*⁸.

Those early diagrams of Hertzprung and Russell formed the cornerstones of what eventually came to be known as the Hertzprung–Russell diagram, in which some measure of intrinsic stellar brightness (such as absolute magnitude, bolometric luminosity, or even apparent magnitude for a star cluster) is plotted *versus* a parameter that correlates with stellar photospheric temperature (such as spectral type, photometric colour, or effective temperature) for an ensemble of stars. As the above quotation states, it took some time for the term ‘Hertzprung–Russell diagram’ to gain common consensus in the astronomical literature. In consequence of the large number of records that can be accessed through the SAO/NASA ADS data-base it is possible to track how the naming of the diagram evolved with time. The history of the astronomy behind the development of the diagram has been discussed by Hoffleit⁹, Nielsen¹⁰, DeVorkin^{11,12}, and Gingerich², while DeVorkin¹ provides an insightful recounting of the subtext behind how the diagram got its currently accepted name.

Statistics from the ADS data-base

A list of publications that use one or more of the terms ‘Hertzprung–Russell diagram’, ‘Russell diagram’, or ‘Russell–Hertzprung diagram’, was compiled by using the SAO/NASA Astrophysics Data System (ADS) search engine. The ADS Labs Integrated search interface was employed (as opposed to the ‘Classic Search’ interface) since it allowed a search of the full-text content of a paper. The current search is only as complete as the data-base indexed in the ADS, which was quoted as being on the order of 11 million records at the time the searches were conducted. The ADS system searches not only scholarly and popular journals but also a variety of official observatory publications that did not necessarily appear in refereed form. There were only a small number of books returned by the ADS search, and as such the compilation that forms

the basis for this article does not track the terminology used in textbooks or standard reference books on astronomy of the early-to-middle 20th Century.

By 1948 the usage of the term ‘Hertzsprung–Russell diagram’ was becoming sufficiently frequent within some papers on stellar astronomy that the abbreviation ‘H–R diagram’ was being introduced into the literature. Consequently it was also necessary to use the ADS to search for instances of that abbreviated designation.

The results of the ADS search are presented graphically in Fig. 1, which is divided into two panels. In both panels the number of ADS records per year in which the term ‘Russell diagram’ appears is shown by a histogram. Those are to be contrasted with the solid line in the lower panel which represents the number of records in which the term ‘Hertzsprung–Russell diagram’ occurs each year, while the solid line in the upper panel tracks the publications in which ‘Russell–Hertzsprung diagram’ is found.

Introduction of the term ‘Russell diagram’

References to ‘Russell’s diagram’ appeared as early as 1915 in *The Observatory* within a report¹³ entitled ‘Spectral Classification of Stars and the Order of Stellar Evolution’, which contains the minutes of a discussion at the 1915 Manchester meeting of the British Association. Comments by Sir F. W. Dyson refer to Russell’s diagram in regard to the figure that appeared in Russell’s

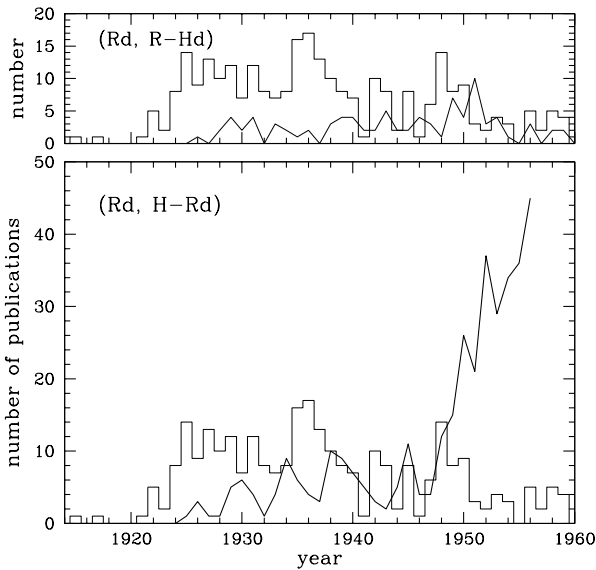


FIG. 1

(Top panel) Number of papers per year in which the term Russell diagram is used (histogram) or the term Russell–Hertzsprung diagram (solid line). (Bottom panel) Number of papers per year in which the term Russell diagram is used (histogram) or the term Hertzsprung–Russell diagram (solid line).

1914 paper⁷ in *Nature*. Similarly, in a 1917 issue of *Monthly Notices of the Royal Astronomical Society* (*MNRAS*), A. S. Eddington¹⁴ referred to Russell's diagram from the same *Nature* paper*. In a 1921 address on the occasion of the award to Russell of the Gold Medal of the Royal Astronomical Society, A. Fowler¹⁵ drew attention to the importance of Russell's diagram. Thus, the association between Russell's name and the diagram itself appears to have begun in English journals, one of which was *The Observatory*, although initially it was perhaps just a convenient way of referring to one specific instance of the diagram in one of Russell's papers, as opposed to an attempt to give the diagram a distinctive or generic name.

During 1922–1924 the slightly different term 'Russell diagram' as an actual name for a luminosity *versus* spectral class plot was being introduced by Brill¹⁶, Lundmark & Luyten^{17,18}, Seares¹⁹, Kienle²⁰, Kobold²¹, Milne²², ten Bruggencate²³, and Vogt²⁴ into the *Astronomical Journal* (*AJ*), the *Astrophysical Journal* (*ApJ*), *MNRAS*, *Astronomische Nachrichten* (*AN*), *Die Naturwissenschaften* (*NW*), and *Publications of the Astronomical Society of the Pacific* (*PASP*). As such, the 'Russell-diagram' terminology initially came to appear in North American, English, and European journals at around the same time. By 1925–1928 both the 'Russell diagram' and 'Russell's diagram' were being picked up by other observers and theoreticians such as Bottlinger²⁵, Emden²⁶, Jeans²⁷, Heinemann²⁸, King²⁹, McLaughlin³⁰, Menzel³¹, Plaskett³², Rabe³³, Redman³⁴ (who used the term in the title of his paper), Rudolph³⁵, Sandford³⁶, Shajn³⁷, Struve³⁸, Trumpler³⁹, and van Rhijn⁴⁰, on both sides of the Atlantic Ocean. Throughout those years the *MNRAS* was a particular forum for sightings of that terminology, which also frequented the official minutes of various meetings of the Royal Astronomical Society as published in *The Observatory*. Eddington⁴¹ and Jeans⁴² were referring in 1925 to 'Russell's compass diagram'. Thus, the record shows that British and mainland European astronomers were as quick to adopt the 'Russell-diagram' appellation as their North American counterparts, if not more so.

Over the period 1928–1938 the term 'Russell diagram' enjoyed popular usage in the astronomical literature, as can be seen from Fig. 1. However, during those years two alternative ways of referring to the diagram started to crop up in the form of the terms 'Hertzsprung–Russell diagram' and 'Russell–Hertzsprung diagram'. Throughout much of the remainder of this article the abbreviations RD, HRD, and RHD, respectively, are used as a convenient way to refer to those alternative names. Eventually, the abbreviation 'H–R diagram' also came to be introduced. Over a thirty-year period astronomers drew freely from among those options, until eventually the forms 'Hertzsprung–Russell diagram', and the more convenient equivalent 'H–R diagram', gained the ascendancy.

DeVorkin¹ has noted the circumstances by which the term HRD became standardized in the style guide of the *ApJ* by Chandrasekhar in the 1940s, with the last vestiges of 'Russell diagram' appearing in the *ApJ* around 1949⁴³. Ironically, that meant that during the decade following 1949, when it was becoming quite infrequent in US publications, the name held on to some extent in European and Soviet journals up until 1959^{44,45}.

*Although Eddington published a redrawn version of the diagram in *Stellar Movements and the Structure of the Universe*⁸, he did not in that 1914 book give the figure a name, but referenced Russell as the originator of both the data and the diagram.

The Trumpler paper of 1925

On the basis of an ADS search the earliest appearance of the term ‘Hertzsprung–Russell diagram’ in an astronomical journal was in the 1925 paper by Trumpler⁴⁶ entitled ‘Spectral Types in Open Clusters’. That paper would be a fitting one for introducing the title HRD into the astronomical journals.* Figure 2 of that paper is an iconic one, the caption for which reads ‘Magnitude–Spectral Class Diagrams of four Cluster Types’. The ordinates used were the same as in Russell’s original diagram for field stars, namely stellar absolute visual magnitude *versus* spectral class, while the stars that Trumpler was investigating were members of open clusters, as in the studies of Hertzsprung³ and Rosenberg⁴. Trumpler divided a sample of 52 clusters into four types, based on the morphology of the magnitude–spectral–class diagram (MSCD). Clusters were classified according to whether their dwarf sequence extends to absolute magnitudes brighter than $M_V = 0$, whether they contain giant stars in addition to dwarf stars, and whether the giant and dwarf regions were separated by a gap in the MSCD.

A key sentence in Trumpler’s⁴⁶ paper is “... the magnitude–spectral class diagrams of clusters show a marked crowding of points along lines which can be identified with parts of the Hertzsprung–Russell diagram of giants and dwarf stars.” Over-plotted in Trumpler’s Fig. 2 are solid lines that depict “the axes of the giant and dwarf branches (Hertzsprung–Russell diagram)”. The concept of a HRD was thereby being interwoven into the discoveries by Hertzsprung^{48–50} and Russell^{5,51} of so-called dwarf and giant stars, which Russell, Eddington⁵², and others were trying to connect through a giant-and-dwarf theory of stellar evolution. Trumpler’s paper drew to an intersection the studies of the Russell diagram of field stars and the Hertzsprung diagram of open-cluster stars.

In 1925 Robert Julius Trumpler⁵³ was working at Lick Observatory. Born in Zurich, Switzerland, he had obtained a PhD degree from Gottingen in 1910. He moved to the United States in 1915 to become an assistant at Allegheny Observatory, where he continued his work on the relative proper motions of Pleiades cluster stars. After arriving at Lick Observatory in 1919, and being appointed an assistant astronomer there in 1920, Trumpler became a citizen of the United States in 1921. He may have been the first in a tradition of US immigrants of European descent to use the term ‘Hertzsprung–Russell diagram’ in the astronomical research literature.

Trumpler did have a professional relationship with Hertzsprung. His 1921 paper⁵⁴ presents a plot of spectral class *versus* absolute visual magnitude for the Pleiades group. Colour information was provided to him in advance of his publication by Hertzsprung. At that time he referred to this plot as the “relation between magnitude and spectral type or color”, but he did not then call it either a HRD or a RD, terms that he would use in subsequent papers.

Prior to 1925 Frederick C. Leonard^{55,56}, in reporting observations obtained at Lick Observatory while he was a University of California Berkeley PhD student, had coined the terms “Hertzsprung–Russell configuration” and “Hertzsprung–Russell arrangement” to denote the distribution of the components of visual

*Hearnshaw⁴⁷ has noted that early usage by Trumpler. By contrast, in another paper³⁹ of 1925, Trumpler referred to the “well known Russell diagram of giant and dwarf stars”.

double stars within a plot of absolute magnitude against spectral class. Leonard⁵⁶ published such a diagram: “Fig. 4 presents the absolute magnitudes of the components of the eighty-five stars of Table IV plotted as a function of the spectral classes of the components. The figure is a modification of the giant–dwarf diagram of H. N. Russell, applied to double, instead of to single stars.” Thus, Leonard actually seems to have come closer to calling the MSCD a ‘Russell diagram’ than a HRD, and in fact he did use the RD terminology in a 1928 paper⁵⁷.

Trumpler was to repeat the term HRD in a paper⁵⁸ of 1926, and both he and P. Doig referred to a “Hertzsprung–Russell configuration” as they exchanged correspondence in the published literature^{59,60}. A student of Trumpler’s, A. D. Maxwell, would use the HRD attribution in a 1927 Lick Observatory publication⁶¹. In reviewing Maxwell’s work, W.E.H.⁶² (William Edmund Harper) also referred to the “Hertzsprung–Russell diagram”.

The Hertzsprung–Russell diagram after 1928

From 1929 the term ‘Hertzsprung–Russell diagram’ started to appear more frequently in the literature. The term gained a place within writings by Kreiken^{63,64}, Krieger⁶⁵, Milne⁶⁶, and Wallenquist⁶⁷ in 1929, Eddington⁶⁸, Shajn⁶⁹, ‘W.M.S.’⁷⁰, Trumpler⁷¹, and Wallenquist^{72,73} in 1930, Kienle^{74,75}, Trumpler⁷⁶, and Slocum⁷⁷ in 1931, and Sticker⁷⁸ in 1932. In two of those papers, by Wallenquist in 1929 and Trumpler in 1930, reference was made to the “well known Hertzsprung–Russell diagram”, both authors then being in the practice of using that term routinely. Many of those astronomers were European or British, writing in European and British publications. The preceding list of papers includes the earliest usage of the term HRD by Milne, Eddington, and Shajn in a refereed publication, although previously they had referred to the RD in their writings. W.M.S. (William M. Smart) was reporting upon a Council meeting of the Royal Astronomical Society on proper motions. By contrast, L. T. Slocum was a student under the tutelage of Trumpler, and C. J. Krieger was another Lick Observatory writer. Thus, within the United States during the period 1925–1931 there is a coincidence between Lick Observatory and use of the term HRD in the astronomical literature, which was a consequence of the habits of Trumpler and his students*.

Despite those earliest appearances of HRD in the literature, the popularization of the term is often attributed^{1,2,47} to Gerard Kuiper or Bengt Strömgren after 1932. In 1933 those articles that refer to a HRD were largely by European astronomers, and there are two particular instances worth highlighting. The term gained a place in a report written by J. H. Oort⁷⁹ that described the Leiden PhD thesis of Gerard P. Kuiper, whose supervisor had been Hertzsprung himself[†]. Of even more particular note is the first appearance of HRD in the title of a paper, the 1933 study by Strömgren⁸⁰ entitled ‘On the Interpretation of the Hertzsprung–Russell Diagram’. This paper is significant not only for the title, but for also introducing the abbreviated form ‘H.–R.-diagram’ into the literature.

Following his thesis work Kuiper moved to Lick Observatory, and in 1934–1935 he included the term HRD in a number of papers^{81–84} that appeared in the *PASP*. Among the records accessed by our ADS search, the first use of HRD in

*Hertzsprung himself went on to be a visiting astronomer at Lick Observatory in 1937, using the 36-inch refractor for double-star work.

†At that time Hertzsprung had been at Leiden for some 14 years.

the *Apf* occurred in 1934 in a paper by Baade⁸⁵ shortly after he had moved from Germany to the United States. Chandrasekhar commenced referencing the term in 1935 and 1936 with papers^{86,87} published in the *MNRAS*. Throughout the period of 1934–1936 the other deployments of the HRD nomenclature were largely restricted to European astronomers writing in European or British publications, for example, Bernheimer⁸⁸, Kopal⁸⁹, and ten Bruggencate⁹⁰ (who had previously preferred the RD).

In the first two decades following the arrival of the term HRD in the astronomical literature, its usage can be traced to European and British astronomers writing in European and English journals or observatory publications, combined with appearances within papers in North American journals written by astronomers of European birth then residing in the United States (e.g., Baade, Gamow⁹¹ from 1938 onwards, Kuiper, Schwarzschild⁹², Struve⁹³, Strömgren, and Trumpler). That was a trend that largely continued until around 1945, with a few notable exceptions. Cuffey & Shapley⁹⁴ were among the first US-born astronomers to concede to the HRD naming in 1937, with Hogg⁹⁵ in 1939, and Titus & Morgan⁹⁶ in 1940. Adams⁹⁷ and Seares⁹⁸ followed in 1944 and 1945 respectively. Across the Atlantic Ocean the terminology was gaining acceptance in papers published in the *MNRAS* and *The Observatory* by astronomers and theorists such as Smart^{99,100} (1939), Greaves¹⁰¹, Hoyle¹⁰², and McVittie¹⁰³ (all in 1945). On mainland Europe the Second World War saw a decline in the astronomical literature from 1939–1945 and the appearance of HRD in European journals was scarce, with a few exceptions such as van Rhijn¹⁰⁴, Wurm¹⁰⁵, and Ramberg¹⁰⁶.

DeVorkin¹ has argued that the struggle to accept the term ‘Hertzsprung–Russell diagram’ in preference to ‘Russell diagram’ had significant cultural overtones, and was driven in part by forceful European astronomers such as Kuiper who had immigrated to the United States. The published record noted above complements DeVorkin’s argument. As an additional example, although the term HRD entered the *Apf* as early as 1934, as noted above, between 1935 and 1945 its occurrence in that journal can be largely traced to Kuiper¹⁰⁷ (1937), Gamow⁹¹ (1938), Strömgren¹⁰⁸ (1938), Chandrasekhar¹⁰⁹ (1939) (who was then in the United States at the University of Chicago), and van Maanen¹¹⁰ (1944). The landmark 1944 *Apf* paper of Baade¹¹¹ reporting on the resolution of Messier 32, NGC 205, and the central region of the Andromeda Nebula into stars, also takes a place within that group, as it contains both the full and abbreviated HRD terminology. Thus, it was scientists who had immigrated to the United States who led the race to introduce the term HRD into the *Apf*, while Titus & Morgan⁹⁶ and Nassau & Hynek¹¹² were among the earliest US-born astronomers to do so (in 1940 and 1942, respectively).

The period 1933–1949 was one of considerable diversity in what to call the diagram, during which astronomers chose to use their own preference among the three options. Throughout the years 1938–1948 the terms ‘Russell diagram’ and ‘Hertzsprung–Russell diagram’ were employed with comparable frequency, as is illustrated by Fig. 1. Usage of the former term went through a maximum around 1936 after which it began to decline, while the latter option increased in popularity. By 1948 the HRD alternative began to predominate.

Not only did different authors employ different terminology, some astronomers evolved in their preference, or did not at first feel the need to be consistent. Writers such as Milne^{113,114}, Strömgren^{80,115}, Kopal^{89,116}, and Baade^{85,117}, chose to use RD in one paper and HRD in another work published less than three years apart, as did Trumpler^{39,46} in 1925 (the year in which he introduced HRD to the literature). Some authors reverted to the RD even after HRD

had found a place in one of their earlier publications. Furthermore, there are instances^{63,74,75,86,118,119} where the competing terms both appeared in the same paper.

After World War 2, US-born astronomers increasingly came to adopt the term HRD, such as Bowen¹²⁰ and Weaver¹²¹ in 1946, Roman¹²² (1948), Code¹²³ and Greenstein¹²⁴ (1949), Eggen¹²⁵ and Shane¹²⁶ (1950). Hoffleit⁹ (1950) in reviewing the luminosity *versus* spectral-class diagram used all three variants (HRD, RD, and RHD) in tracing the historical development of the plot. DeVorkin¹ notes that one key event in codifying the use of HRD in the refereed literature was the formal introduction by Chandrasekhar of the term into the *ApJ* style manual*. The years 1951 and 1952 saw the term in widespread use in the three US journals *ApJ*, *AJ*, and *PASP*, by authors such as Chamberlain & Aller¹²⁸, Baum¹²⁹, H. L. Johnson¹³⁰, Sandage¹³¹, Sharpless¹³², and Walker¹³³. A significant reinforcement came in 1952 with the publication by the *AJ* of papers that had been presented at the first conference to be explicitly dedicated to the diagram. A symposium entitled ‘The Hertzsprung–Russell Diagram’ was held on 1951 December 28 during the 86th meeting of the American Astronomical Society in Cleveland, Ohio. The abbreviated term ‘H–R diagram’ was used rather than the full term in the published title of the proceedings. Introductory remarks by Russell¹³⁴ were published which employ the term ‘H–R diagram’ in the abbreviated form. From that time on there was no going back in the tussle between the HRD and the RD, and even the abbreviated form ‘H–R diagram’ can be said then to have come of age.

The Russell–Hertzsprung diagram

Interwoven into the astronomical literature between appearances of the HRD and RD was also the alternative form ‘Russell–Hertzsprung diagram’. As indicated by Fig. 1, that variation did at no time rise to compete with the initially more popular RD or the up-surging HRD. Among the earliest appearances of the term were in papers by Shajn¹³⁵ (1928, he would later switch to HRD), Kreiken⁶³ (1929; who was also using HRD in the same year), Atkinson¹³⁶ and Strömberg¹³⁷ (1931), and Morgan¹³⁸ (1933). Between 1928 and 1951 the term tended to crop up mostly in North American or British publications, and was less often seen in the journals of mainland Europe. The RHD variant appeared only infrequently after 1953, although it is found in the literature up until around 1972. By 1964 Tucker¹³⁹ could remark that the use of the term in the textbook by Motz¹⁴⁰ was notably atypical.

Concluding remarks

By 1952, some forty years after Hertzsprung’s original diagram for the Pleiades and Hyades open clusters and Russell’s diagram for field stars, the term ‘Hertzsprung–Russell diagram’ was being used synonymously for the colour–magnitude diagrams (CMDs) that were just starting to reach the main sequences of globular clusters^{141–143}. It is fitting that the names both of Hertzsprung and Russell were being associated with those CMDs. Hertzsprung’s early diagrams were for open clusters that contained mostly dwarf stars, and few red giants of the type that showed up in Russell’s 1913 field-star diagram. Now the globular-cluster CMDs were revealing the top of the dwarf sequence that had dominated

* Chandrasekhar’s role in furthering the acceptance of the term preceded his appointment to the editorial board of the *ApJ*, and his use of the term in his 1939 book on stellar structure¹²⁷.

Hertzsprung's diagrams for open clusters. Russell knew that the disposition of giants and dwarfs in his original field diagram was an important clue to stellar evolution, although the 'reversed-7' appearance fostered a misreading of the order of evolution as being from the giant region of the diagram to the dwarf region. It was with the arrival of deep photographic photometry of globular clusters that the potential of the HRD for studying the consecutive stages in the evolution of low-mass stars could be more fully realized. Thus, the publication of the first CMDs to show the dwarf branch of a globular cluster, coinciding with the 1951 American Astronomical Society symposium on the HRD, does seem to demarcate a conclusion to the 'smouldering' phase in the history of the 'Hertzsprung–Russell diagram'.

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SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 245: HD 26083, HD 26441, HD 51001, AND HD 85843

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The four stars came to attention in different ways. HD 26083, a 9^m Ko star just south of the celestial equator in Eridanus, features in an as-yet-unpublished extension of the 'Clube Selected Areas' programme. Its extraordinarily high (for a late-type star) $v \sin i$ of some 30 km s⁻¹ marked it out as a probable short-period binary at the very first observation, which was made as comparatively recently as late 2013. It quickly proved to have a circular orbit

with a period of about 18 days; it was already known to show photometric variations with half that period, which no doubt arise from non-sphericity of the star, whose parallax shows it to be a giant.

The 8^m object HD 26441 (ADS 3041), about 3° south of HD 26083, is a very close visual binary, known for 101 years, consisting of an unequal pair of G dwarfs in an orbit for which periods of about 20 and 40 years have been proposed; the radial velocities demonstrate that the shorter one is the correct choice, and the very high eccentricity of about 0.84 enables them to define it within a standard error of only 4 days.

The radial velocity of HD 51001, an 8^m very red star in southern Gemini, was observed on the recommendation of Filiz Ak after the star was found by *ASAS-3* to be a photometric variable with a period of 138.2 days; its radial velocity proves to vary in the same period. The orbit is almost circular but appears to deviate from a sine wave in a systematic fashion.

Attention was drawn to the 7^m F8 V star HD 85843 by Suchkov, who found it to be ‘over-luminous’; there is in fact a very weak secondary component in the spectrum, but it cannot be responsible for much of the apparent excess luminosity. The orbit is of modest eccentricity (0.17) and has a period of 1387 days that is determined to 1 day. The secondary must surely be a lower-main-sequence object, but the mass ratio from the orbital elements does not agree with the luminosity ratio estimated from the radial-velocity traces.

HD 26083

HD 26083 is a star in a region of sky rather barren to the naked eye, in the same declination as Orion’s Belt but about 20° preceding. It is about 1½ degrees directly following the fifth-magnitude B star 35 Eri*. The summary above notes that HD 26083 qualified for inclusion in an extension of the ‘Clube Selected Areas’^{1,2} investigation. The character of that extension is only an increase in the size of the areas from which the stars are drawn (to increase the sample numbers) — not to alter the selection criteria in any other manner. Those criteria are (a) spectral type Ko in the *Henry Draper Catalogue*, and (b) $m_V = 9^m.0 \pm 0^m.5$. HD 26083 qualifies for the programme by having $V = 9^m.07$ (as tabulated in the *Hipparcos Catalogue*) or $9^m.09$ (as listed by *Simbad* from *Tycho 2*, which also provides a $(B - V)$ of $1^m.11$).

The *Hipparcos Catalogue* itself³ flags the star as an ‘unsolved variable’, and gives the photometric range as $0^m.04$ in its own *H_p* magnitude scale, which is,

*35 Eri is portrayed in *Uranometria 2000.0* as an encircled dot, indicating that it is a variable star. It is noted in the *Hipparcos Catalogue* as an ‘unsolved variable’, and the ‘epoch photometry’ does exhibit a scatter that is a bit larger than is warranted by the indicated uncertainties of the individual points; but the whole range is less than $0^m.05$, and is not likely to trouble users of the *Uranometria*. Nor does it seem to have attracted interest or comment subsequently, so the matter could not be pursued here even if it were germane to this paper.

TABLE I
Radial-velocity observations of HD 26083

All the observations were made with the Cambridge Coravel

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O - C) km s ⁻¹
2013 Oct. 30.16	56595.16	+12.9	0.109	-0.7
Nov. 8.09	604.09	-32.5	.606	-0.8
9.08	605.08	-25.1	.661	-0.9
10.95	606.95	-5.4	.766	+0.7
13.09	609.09	+12.6	.885	-0.2
20.04	616.04	-11.8	1.272	+1.1
Dec. 1.07	627.07	+14.0	.887	+0.9
5.02	631.02	+13.8	2.107	0.0
10.05	636.05	-30.6	.387	+0.3
20.02	646.02	+17.5	.943	-0.7
27.94	653.94	-29.2	3.384	+1.4
28.92	654.92	-35.3	.439	+0.5
2014 Jan. 3.92	56660.92	-2.6	3.773	+2.2
4.98	661.98	+4.8	.832	-0.6
11.90	668.90	-4.6	4.218	-1.5
17.93	674.93	-37.1	.554	-0.8
20.85	677.85	-14.2	.716	+0.8
Feb. 12.83	700.83	+19.5	5.997	-0.6
13.80	701.80	+19.1	6.051	+0.5
27.81	715.81	+4.0	.832	-1.3
Oct. 10.13	940.13	-24.0	19.332	-0.7
Nov. 4.08	965.08	-15.0	20.723	-1.1
6.09	967.09	+6.4	.835	+0.6
8.10	969.10	+17.1	.946	-1.4
10.05	971.05	+19.0	21.055	+0.6
24.02	985.02	+7.2	.834	+1.5
Dec. 8.97	999.97	-23.5	22.667	-0.2
2015 Jan. 6.92	57028.92	-13.9	24.280	+0.4
10.91	032.91	-39.2	.502	-1.3
22.91	044.91	+5.5	25.171	+0.7
30.83	052.83	-30.1	.613	+0.8

however, close to *V. Koen & Eyer*⁴ subsequently managed to divine a period, of 8.981 days (they actually expressed it as a frequency, of 0.11135 day⁻¹), in the *Hipparcos* ‘epoch photometry’, with an amplitude of 0^m.0248. The (revised⁵) *Hipparcos* parallax is 1.32 ± 1.12 arc-milliseconds, which is hardly prescriptive but may be said, in round terms, to indicate that the distance must be more than about 400 pc (corresponding to $\pi = 2.5$ ms)*, and accordingly that the distance modulus must be more than about 7 magnitudes. So the absolute magnitude is brighter than +2, and the star is evidently a giant.

The first of 31 radial-velocity observations of HD 26083 was made at Cambridge in 2013 October. The radial-velocity traces of the star are quite unusual, being greatly smeared out, no doubt by rotation; the mean value

*That parallax has been inverted, with a near-millionfold enhancement of apparent precision, into a distance of 757.580 pc by McDonald *et al.*⁶. An analogous appearance of lack of comprehension is exhibited by de Bruinje & Eilers⁷, who find that, in order not to spoil the precision of the hoped-for *Gaia* proper motion, we will need to determine the radial velocity of the star to within 38681.80 km s⁻¹. We do that below.

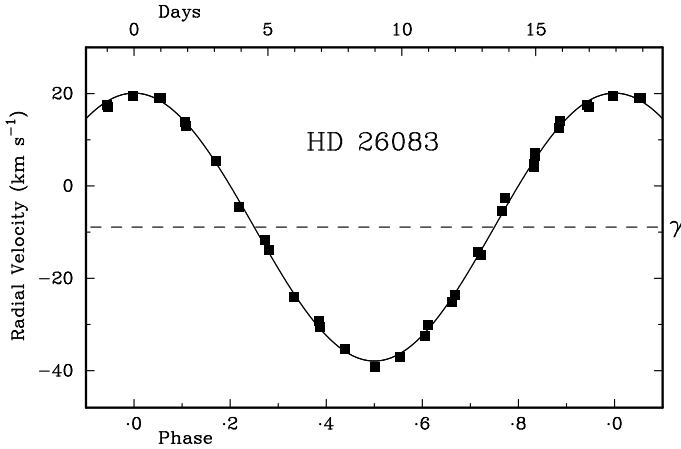


FIG. 1

The observed radial velocities of HD 26083 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*.

of $v \sin i$ is 32.3 km s^{-1} , with a formal standard error of only 0.3 km s^{-1} . The velocities are set out in Table I and readily lead to a circular orbit which is plotted in Fig. 1 and whose elements are as follows:

$$\begin{array}{ll}
 P = 17.9450 \pm 0.0025 \text{ days} & T_0 = \text{MJD } 56772.662 \pm 0.025 \\
 \gamma = -8.90 \pm 0.19 \text{ km s}^{-1} & a_1 \sin i = 7.16 \pm 0.07 \text{ Gm} \\
 K = 29.01 \pm 0.27 \text{ km s}^{-1} & f(m) = 0.0455 \pm 0.0013 M_{\odot}
 \end{array}$$

$$\text{R.m.s. residual (wt. 1)} = 0.95 \text{ km s}^{-1}$$

When the requirement $e = 0$ is relaxed, the eccentricity takes the value 0.014 ± 0.009 and the sum of the squares of the deviations of the 31 data points falls from 28.04 to $25.51 \text{ (km s}^{-1})^2$. That reduction is far from being statistically significant: it leads to an F value of 1.24 with 2 and 25 degrees of freedom, for which even the 10% point is 2.53 .

The photometric period found by Koen & Eyer is almost exactly half (it is actually 0.50046 times) the orbital period, clearly indicating that the cause of the variation is ellipticity of the star. Unfortunately those authors give only a frequency and not an epoch, so we do not know the phasing of the photometric wave with respect to the orbit. The projected circumference of the star, in kilometres, is equal to the product of $v \sin i$ and the period expressed in seconds, so its projected radius must be $32.3P/2\pi$, or very nearly 8 Gm (about 12% greater than the value of $a_1 \sin i$ in the informal table above, reflecting the ratio of $v \sin i$ to K and showing that the centre of gravity of the binary system is actually within the primary star). That projected radius is about 0.7 times the radius (of 15 or 16 R_{\odot}) given in tables for a typical KoIII star, which may indicate that the axial inclination is something like $\sin^{-1}(0.7)$, or about 45° . In a system

that is so close that the primary star is distorted into an ellipsoid, the orbital and rotational poles can be expected to coincide, so the orbital inclination, too, is probably near 45° . If we suppose the mass of the primary star to be $2 M_\odot$, the mass function sets a minimum mass of about $0.7 M_\odot$ for the secondary. If we also suppose the orbital inclination to be 45° , the mass required of the secondary would be about $1.1 M_\odot$, corresponding to that of a main-sequence star with a spectral type of about G0 and an absolute magnitude near +4. That would be sufficiently fainter than the primary (and give a dip of much smaller equivalent width) that it would pass undetected in radial-velocity traces, and would not compromise the colour indices, certainly not in the *UBV* range, to an extent that would draw attention to the hybrid nature of the system. The orbit of such a secondary star would be larger than that of the primary, by the inverse of the ratio of their masses, at about $13/\sin i$ Gm, so the (constant) separation of the stars in their circular orbit should be about $2\frac{1}{2}$ times the radius of the large evolved primary.

The tentative model of the system, then, which admittedly owes a good deal to an *ex cathedra* estimate of the mass of the primary star, has a Ko III primary and a G0 V secondary, with masses of 2.0 and $1.1 M_\odot$, respectively, the secondary being sufficiently close and massive to produce ellipsoidal distortion of the primary significant enough to be observable despite the moderate orbital inclination of about 45° . The centre of mass is just within the primary star, and the centre-to-centre separation of the components is about $2\frac{1}{2}$ times the radius of the primary.

HD 26441 (*ADS 3041*)

HD 26441, an object long known as a very close visual-binary system, appears as a 7^m star in Eridanus, about 3° further south than, and slightly following, the one (HD 26083) treated above; more helpfully for finding it, it is just 2° north of the wide visual double star 37/38 Eri. *Hipparcos* reports ground-based photometry rather than its own, and it seems as if neither it nor *Simbad* provides any easy means of retrieving the source of it, though *Simbad* attributes it generically to Eggen. A look at the contents pages of the *RGO Bulletins* covering the epoch of Eggen's activity there locates the original publication of the photometry as ref. 8, giving $V = 7^m.36$, $(B - V) = 0^m.65$, $(U - B) = 0^m.22$. The $(B - V)$ colour index is that of a solar-type star, while the $(U - B)$ one is distinctly redder. The (revised⁵) *Hipparcos* parallax of 16.09 ± 0.65 milliseconds puts the distance modulus very close to four magnitudes, so the joint absolute magnitude of the pair is about $3^m.4$, about $1^m.3$ brighter than a single main-sequence star of the same $(B - V)$ colour index. The duplicity of the star cannot account for all of the enhancement, which therefore must be partly laid at the door of evolution. We return to that matter after estimating the Δm of the system and attempting to divine its evolutionary status, below.

The object was first recognized as a double star by Aitken in 1914. Aitken, observing with the Lick 36-inch refractor, discovered double stars on a wholesale scale and routinely announced his discoveries in frequent papers that listed 100 new doubles at a time. HD 26441, which took his 'discovery number' A 2801, was the first entry in one⁹ such paper; it is listed with components each of magnitude 8.0, with an angular separation of only $0''.19$. That was a hard act to follow, and in fact more than 20 years passed before other observers managed to measure the system. The first orbit for it was presented by Muller¹⁰ (in French) in the *Journal des Observateurs* in 1954; he was able to call on only 12 observations of the pair (they are listed in his paper), of which four were not

actual measurements but simply reported the pair as unresolved. (He could be seen as sailing very close to the wind, in deriving seven orbital elements from eight real observations plus four negative ones! All the same, he obtained an orbit that is quite similar to what is now believed to be the true one, which is more than can be said for some subsequent authors.) He found the orbit to have a period of 20 years and the very high eccentricity of 0.76. Muller remarked that the system was resolvable only over a small sector of about 60° of the orbit. He specifically referred to a trial of a solution with double the period and a low eccentricity, but found it less satisfactory; he did not close the door on it altogether, though, but concluded his remarks with (this is a free translation from his French), “It will be at least ten years before the choice of period can be considered definitive.” Muller listed exactly the same elements* in a paper¹¹ in the *Astronomical Journal*, though without any listing of data and residuals such as are normally included in any such publication and do appear in his *JO* paper. It may be speculated that the logic behind the partial duplication of publication was that the *AJ*, with its English language, would reach a wider clientele than the *JO*, but that the heavy page charges in the former mandated brevity.

Zulevic¹² put forward in 1972 an orbit with a period of 40 years and zero eccentricity — an orbit of just the sort that Muller had rejected in 1954. As late as 1986 Baize¹³ gave a similar one; in a note he referred to the Muller and Zulevic orbits, and said, “Calcul repris pour départager les deux orbites”, (i.e., “Calculation repeated to distinguish between the two orbits”) — but despite having 30 years’ more data than Muller, and deliberately setting out to decide between the competing solutions, he distinguished the wrong one! Astrometry was at last restored to reality with the 20.42-year orbit recently published by Tokovinin, Mason & Hartkopf¹⁴.

Many of the papers retrieved for HD 26441 by *Simbad* refer to measurements of the system as a visual binary, at first literally visually and later often by speckle interferometry, but some offer other information that is mostly summarized in this present paragraph. The *HD* lists the spectral type of HD 26441 as G0; the first MK classification was made by Christy & Walker¹⁵, who also attempted to assess the individual types of stars involved in visual binaries, simply from the joint classification plus the *Am*. For HD 26441 they classified the system jointly as G2 IV, and then on the basis that *Am* = 0 they concluded that the individual types were G2 IV + G2 IV! Stephenson & Sanwal¹⁶ gave the joint spectral type as G3 IV–V. It is interesting that both sets of authors recognized the system as being slightly above the main sequence. The radial velocity was first given by Heintz¹⁷, who saw the system as single-lined and gave two measurements, of +27.8 and +27.4 km s⁻¹, obtained only six days apart. Tokovinin & Smekhov¹⁸, too, measured it as single-lined and gave a mean of two observations as +20.63 km s⁻¹. The large ‘Geneva–Copenhagen survey’¹⁹ of F and G dwarf stars appears to say that 50 radial-velocity observations were made over an interval of 6334 days, and that the system is double-lined and has a mean velocity of +25.4 km s⁻¹. The individual observations are believed not to be generally available; but it seems surprising that, with so many data covering a time interval much longer than the orbital period, no orbit was offered. Although visual observers have usually not been able to distinguish between the magnitudes of the stars in such a close pair, some of the speckle experts have offered non-zero *Ams*. Horch *et al.*²⁰ gave the difference as 0^m.52, while what we must admit to be a powerful syndicate of Tokovinin, Mason & Hartkopf¹⁴ has recently noted it at as much as 0^m.7.

*apart from the epoch of periastron being given as 1911.5 instead of 1951.5 — those times are equivalent to one another, since the period was 20 years.

New radial-velocity observations, and an effort to model the system

HD 26441 was placed on the observing programme of the original radial-velocity spectrometer²¹ at Cambridge in 1986, and was found then to give single-lined traces. Since it seemed certain that the traces really should exhibit two ‘dips’, although much of the time they could be expected to be blended together and appear single, every opportunity was taken to observe the system with other spectrometers with digital outputs that would facilitate the splitting of close blends. In 1991 the traces became very asymmetrical, and in early 1993 the two dips were more or less completely resolved, with a velocity separation in excess of 30 km s^{-1} . At that epoch the writer had no operational instrument at Cambridge and was entirely dependent on guest-investigator privileges at other observatories; he was able to obtain only two observations in the critical season of nodal passage. By the next season, however, the system had reverted to single-lined. For a long time after the commissioning of the Cambridge *Coravel* in 1999, HD 26441 appeared single-lined, but eventually, in 2013, the brief double-lined phase recurred and was attentively observed. A trace taken at that epoch is illustrated in Fig. 2.

The traces that were obtained while the system was overtly double-lined in 2013 enabled the relative strengths of the two ‘dips’ to be established as 1 to 0.56, a ratio that, expressed in magnitude terms, is $0^{\text{m}}.63$. Both dips are of minimal width and indicate quasi-zero rotational velocities for both stars. In cases where both components of a binary system can be assumed to be main-sequence stars, the fainter one, being of later type, has spectral lines that are mostly stronger than those of its hotter companion, and an empirical ‘rule of thumb’ asserts that the actual magnitude difference of the pair is 15% greater than the difference, expressed in terms of stellar magnitudes, corresponding directly to the relative dip strengths in the radial-velocity traces. For HD 26441, that ‘rule’ would give $\Delta V \sim 0^{\text{m}}.72$. But the fact that that binary system, jointly, stands more than a magnitude above the main sequence indicates that at least the primary star is significantly evolved; the initial evolution of a solar-type star is towards higher luminosity at more or less constant colour index (vertically upwards in the H–R diagram), so the ‘rule of thumb’ does not apply, at least not directly, in this instance.

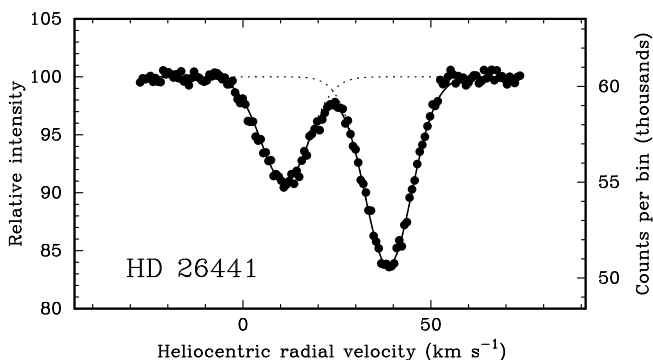


FIG. 2

Radial-velocity trace of HD 26441, obtained with the Cambridge *Coravel* on 2013 December 4, showing the unequal dips at nearly their maximum separation.

Assessment of the degree of evolution is not straightforward, because even if it were only the primary star that had evolved significantly, it must have started already brighter than the secondary, and the two stars together would obviously constitute an object that would appear brighter than a main-sequence object of the same colour; therefore evolution would have contributed only part of the present enhancement of luminosity in comparison with the main sequence. A pair of stars with a ΔV of $0^m.63$ — or $0^m.72$, it does not make a major difference for present purposes — is jointly nearly half a magnitude brighter than the primary alone. But HD 26441 is about $1^m.3$ brighter than a main-sequence star of its colour index, and it is in a part of the sky where such a comparatively nearby system cannot plausibly be suspected of suffering significant interstellar reddening. Anticipating a result from the orbit determined below, we find that the components of HD 26441 differ in mass by nearly 8%, (logarithmically by just over 0.03), an amount corresponding to some two sub-types or a luminosity difference of about $0^m.3$ on the main sequence. We could therefore suppose the original spectral types, on the main sequence, to have been very close to G2 and G4, with M_{VS} of $4^m.7$ and $5^m.0$, respectively, and a combined absolute magnitude of $4^m.1$; the present combined value of $3^m.4$, divided between the components whose ΔV we have found above to be about $0^m.7$, must be very nearly $3^m.9$ and $4^m.6$, implying evolutionary increases of luminosity of $0^m.8$ and $0^m.4$ above the ZAMS, respectively.

Orbit of HD 26441

Once a system has been seen double-lined, as HD 26441 was in Fig. 2, the profiles of the ‘dips’ of the individual components are known and can be imposed on the reductions of other traces, so even seemingly single-lined ones can usually be reduced to give the twin velocities. That has enabled the observations to give a tolerably well-covered double-lined orbit, which is portrayed in Fig. 3. In addition to the writer’s own observations made on a guest-investigator basis with the Geneva Observatory’s ‘Coravel’ spectrometers (17 at Haute-Provence (OHP) and two at ESO), and 27 obtained subsequently with the Cambridge *Coravel*, it has been possible to include in the list of measurements (Table II) as many as 37 other observations made by other users of the OHP system. They have been kindly subscribed by the observers and reduced and forwarded, in part long ago by the late Dr. A. Duquennoy, and more recently by Dr. S. Udry. The initial measurements made with the original spectrometer at Cambridge of the unresolved blends are not of utility for the orbit and have not been included in the table. The OHP/ESO measures have been increased by $+0.8 \text{ km s}^{-1}$ to take account of the zero-point offset that has often been noted between OHP and Cambridge; the Cambridge ones have been adjusted by -0.6 km s^{-1} to bring them into approximate systematic accord with the OHP ones. (Cambridge velocities have been found to have a colour-dependent zero-point, and the amount of -0.6 km s^{-1} is more or less in keeping with extrapolation to bluer colours of an explicit comparison of OHP–Cambridge offsets for the ‘Redman K stars’²².) The OHP velocities have been half-weighted in comparison with those made with the Cambridge *Coravel* in the solution of the orbit; four early OHP ones that were particularly severely blended near the cross-over of the components’ velocities near phase .6 are not very reliably split between the components and are zero-weighted. The orbital elements are as follows:

P	$= 7527 \pm 4$ days (20.61 years)	$(T)_1$	$= \text{MJD } 49144 \pm 4$ (1993.43)
γ	$= +26.55 \pm 0.03 \text{ km s}^{-1}$	$a_1 \sin i$	$= 656 \pm 4 \text{ Gm}$
K_1	$= 11.59 \pm 0.06 \text{ km s}^{-1}$	$a_2 \sin i$	$= 708 \pm 6 \text{ Gm}$
K_2	$= 12.51 \pm 0.10 \text{ km s}^{-1}$	$f(m_1)$	$= 0.199 \pm 0.004 M_\odot$
q	$= 1.079 \pm 0.010 (= m_1/m_2)$	$f(m_2)$	$= 0.250 \pm 0.007 M_\odot$
e	$= 0.8372 \pm 0.0015$	$m_1 \sin^3 i$	$= 0.929 \pm 0.021 M_\odot$
ω	$= 69.0 \pm 0.4$ degrees	$m_2 \sin^3 i$	$= 0.860 \pm 0.016 M_\odot$

R.m.s. residual (wt. 1) = 0.24 km s⁻¹

The colour index of the system demonstrates that the primary star must be — or must *have* been, before it evolved so far — very similar to the Sun, so we might expect its mass to have been, and still to be, just one solar mass. We might then be optimistic enough to insert the value $m_1 = 1$ in the equation involving $m_1 \sin^3 i$, seen above among the orbital elements, and regard it as an equation yielding the orbital inclination, i . The calculation is readily executed merely by taking the cube root of the value of $m_1 \sin^3 i$, to wit 0.976 ± 0.007 , to obtain the value of $\sin i$. That gives i as $77^\circ.4$ with a 1- σ uncertainty of about 2° , a result in none too close agreement with Tokovinin *et al.*'s astrometric value¹⁴ of $71^\circ.5 \pm 2^\circ.2$. Looking at the same equation another way, instead of inserting a value for m_1 we could insert Tokovinin *et al.*'s value of i and thereby obtain a mass for the primary star, of about $1.09 \pm 0.04 M_\odot$. To conclude this section, Table III offers a succinct comparison between the astrometric orbits (those of acceptable character only) and the spectroscopic one determined here.

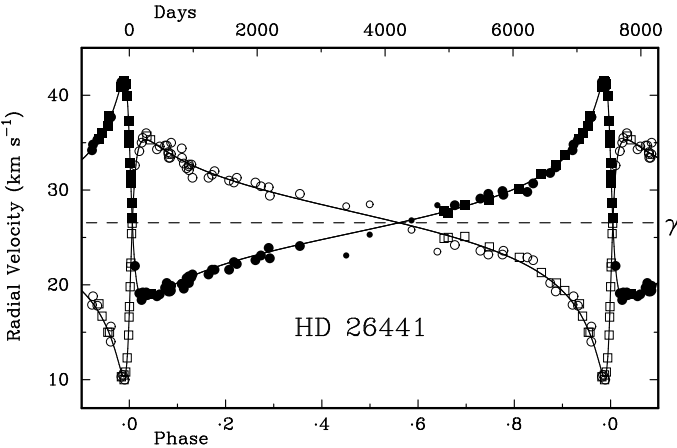


FIG. 3

The observed radial velocities of HD 26441 (ADS 3041) plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. Observations of the primary are plotted with filled symbols, those of the secondary (weighted 0.3 in the solution of the orbit) with open ones. Cambridge measurements are shown as squares, Haute-Provence ones (all half-weighted in the solution) as circles. Four observations obtained near phase .5, when the components' velocities were almost equal and the numerical decomposition of the dips between the components was unreliable, have been rejected from the orbit and are plotted with smaller symbols.

TABLE II

Radial-velocity observations of HD 26441

*Except as noted, the sources of the observations are as follows:
 1978–1999 — Haute-Provence Coravel (weighted ½ in orbital solution);
 2006–2014 — Cambridge Coravel (weight 1)*

Date (UT)	MJD	Vélocity		Phase	(O–C)	
		Prim. km s ⁻¹	Sec. km s ⁻¹		Prim. km s ⁻¹	Sec. km s ⁻¹
1978 Oct. 13:13 31:06	43794.13 812.06	+23.9 22.8	+30.3 29.4	0.289 .292	+0.4 -0.7	+0.4 -0.4
1980 Feb. 14:79	44283.79	24.1	29.6	0.354	-0.2	+0.6
1982 Feb. 8:79	45008.79	23.1	28.3	0.451	-2.2	+0.4
1983 Feb. 11:76	45376.76	25.3	28.5	0.499	-0.6	+1.2
1984 Nov. 27:05	46031.05	26.8	25.8	0.586	-0.1	-0.4
1986 Jan. 6:82 Oct. 7:12	46436.82 710.12	28.4 28.4	23.5 24.2	0.640 .677	+0.9 +0.4	-2.0 -0.8
1987 Nov. 10:99	47109.99	29.1	23.6	0.730	+0.3	-0.5
1988 Mar. 14:79 Oct. 23:12 Nov. 6:07	47234.79 457.12 471.07	29.6 29.9 29.5	23.2 23.2 23.6	0.746 .776 .778	+0.5 +0.3 -0.1	-0.6 -0.1 +0.4
1989 Nov. 15:28	47845.28	29.8	22.9	0.827	-0.9	+0.9
1990 Feb. 15:09* Nov. 8:05	47937.09 48203.05	30.7 31.8	22.6 20.2	0.840 .875	-0.4 -0.4	+0.9 -0.3
1991 Feb. 3:81 Oct. 25:08 Nov. 9:00	48290.81 554.08 569.00	32.4 34.2 34.8	19.3 17.9 18.8	0.887 .922 .924	-0.2 -0.1 +0.3	-0.7 -0.3 +0.8
1992 Jan. 16:90 Aug. 13:14 15:14	48637.90 847.14 849.14	35.3 37.7 37.7	17.8 14.0 15.6	0.933 .961 .961	+0.2 -0.1 -0.1	+0.5 -0.5 +1.2
1993 Jan. 26:78 Feb. 14:81 Mar. 19:79 Aug. 28:16 Nov. 3:38* Dec. 10:03 27:87	49013.78 032.81 065.79 227.16 294.38 331.03 348.87	41.4 41.2 40.8 22.0 19.1 18.4 19.2	10.3 10.5 10.0 32.6 34.1 35.0 35.5	0.983 .985 .990 1.011 .020 .025 .027	+0.3 -0.3 -0.8 +0.1 0.0 -0.2 +0.7	-0.5 0.0 -0.4 +1.0 -0.5 -0.1 +0.3
1994 Feb. 19:81 Aug. 7:13 Sept. 19:18 Dec. 11:95	49402.81 571.13 614.18 697.95	19.2 18.8 19.0 19.7	36.0 34.3 34.6 34.7	1.034 .057 .062 .074	+0.8 -0.2 -0.1 +0.2	+0.7 -0.4 +0.1 +0.5
1995 Jan. 7:92 31:90 Feb. 1:81 7:81 26:81 Mar. 12:82 16:81 Aug. 30:13	49724.92 748.90 749.81 755.81 774.81 788.82 792.81 959.13	20.2 19.6 19.3 19.5 19.3 19.4 19.9 +20.1	34.7 34.0 33.8 33.4 33.4 33.8 35.0 +34.4	1.077 .080 .080 .081 .084 .086 .086 1.08	+0.6 -0.1 -0.4 -0.2 -0.5 -0.4 0.0 -0.3	+0.6 0.0 -0.2 -0.5 -0.5 0.0 +1.2 +1.2

TABLE II (concluded)

Date (UT)	MJD	Vélocity		Phase	(O-C)	
		Prim. km s ⁻¹	Sec. km s ⁻¹		Prim. km s ⁻¹	Sec. km s ⁻¹
1995 Sept. 2·13	49962·13	+20·1	+33·4	·109	-0·3	+0·3
Oct. 1·17	991·17	19·6	32·8	·113	-0·9	-0·2
Nov. 15·05	50036·05	20·7	32·3	·119	0·0	-0·6
Dec. 18·98	069·98	20·9	32·5	·123	+0·1	-0·3
20·91	071·91	20·2	32·1	·123	-0·6	-0·7
21·91	072·91	20·5	32·7	·123	-0·3	-0·1
1996 Jan. 27·81	50109·81	20·8	32·7	1·128	-0·1	0·0
Feb. 14·14	127·14	21·1	31·3	·131	+0·2	-1·3
Oct. 22·14	378·14	21·1	31·3	·164	-0·5	-0·6
Dec. 16·87	433·87	21·6	31·6	·171	-0·1	-0·1
1997 Jan. 24·90	50472·90	21·6	32·0	1·177	-0·2	+0·4
Sept. 10·10	701·10	21·6	31·0	·207	-0·7	-0·1
Nov. 24·14	776·14	22·4	30·8	·217	-0·1	-0·1
1998 Jan. 8·86	50821·86	22·2	31·3	1·223	-0·4	+0·5
Oct. 27·09	51113·09	22·6	30·8	·262	-0·5	+0·5
1999 Jan. 20·84	51198·84	23·1	30·4	1·273	-0·2	+0·3
2006 Nov. 29·06	54068·06	27·8	24·9	1·654	+0·1	-0·4
2007 Feb. 1·85	54132·85	27·6	25·0	1·663	-0·2	-0·2
Oct. 20·11	393·11	28·4	25·1	·697	+0·1	+0·4
2008 Nov. 8·09	54778·09	29·0	24·0	1·749	-0·1	+0·2
2010 Feb. 20·79	55247·79	30·1	22·9	1·811	-0·2	+0·4
2011 Jan. 31·86	55592·86	31·7	21·3	1·857	+0·1	+0·1
Sept. 24·18	828·18	32·7	20·2	·888	0·0	+0·2
2012 Feb. 1·84	55958·84	33·7	19·4	1·905	+0·3	+0·3
Sept. 19·19	56189·19	35·4	18·0	·936	+0·1	+0·9
Nov. 6·06	237·06	36·0	16·7	·942	+0·1	+0·2
2013 Feb. 4·84	56327·84	36·8	15·0	1·954	-0·2	-0·2
Mar. 2·79	353·79	37·8	15·0	·958	+0·4	+0·2
Sept. 5·18	540·18	40·9	10·3	·983	-0·2	-0·5
Oct. 16·09	581·09	41·5	10·5	·988	-0·1	+0·2
17·14	582·14	41·4	10·0	·988	-0·2	-0·3
Nov. 13·04	609·04	41·2	10·8	·992	0·0	0·0
Dec. 4·99	630·99	39·9	12·3	·995	+0·1	+0·1
27·92	653·92	37·3	14·7	·998	+0·3	-0·5
2014 Jan. 3·93	56660·93	35·8	16·6	1·999	-0·1	+0·1
9·89	666·89	35·0	17·7	2·000	+0·1	+0·1
20·82	677·82	32·9	19·8	2·001	+0·1	0·0
27·80	684·80	31·6	21·9	·002	+0·2	+0·6
Feb. 1·81	689·81	30·7	22·3	·003	+0·2	0·0
11·79	699·79	28·6	25·4	·004	0·0	+1·1
20·81	708·81	27·0	26·5	·005	-0·1	+0·6
Oct. 10·15	940·15	19·0	35·7	·036	+0·5	+0·4
Dec. 6·02	997·02	+19·0	+35·3	·043	+0·4	+0·2

*Observed with ESO *Coravel*

TABLE III
Comparison of astrometric and spectroscopic orbital elements
for HD 26441 (A2801, ADS 3041)

Author	P (years)	T	e	i (deg.)	ω (deg.)
Muller ^{10,11}	20	1951.5	0.76	62.3	42.0
Tokovinin <i>et al.</i> ¹⁴	20.42 0.12	1952.61 .36	0.887 .031	71.5 2.2	70.4 3.8
This 'Paper 245'	20.608 0.011	1993.430 .011	0.8372 .0015	77.4 2.	69.0 .4

HD 51001

This star has been so neglected as to have *no* bibliography retrieved by *Simbad*, so the summary of its literature will not detain the reader here for long. The star is to be found in the southern part of Gemini, about 2° north of the $4^m.7$ star 38 Gem. It is classified K5 in the *Henry Draper Catalogue*, and indeed it is very red — we are indebted to *Tycho 2* for its V and B magnitudes, $V = 8^m.20$, $(B - V) = 1^m.60$. It features also in the great Dearborn *Catalog [sic] of faint red stars*²³, as no. 12660*, whose type is given as M4. That whole catalogue is of stars whose spectra exhibit TiO bands; the *HD* cannot be faulted for overlooking them in the spectrum of HD 51001, however, because the description of K5 spectra in the introductory text to each volume of the *HD* does refer to the incipient appearance at K5 of the (then-unidentified) bands that become so strong in the M types. *Simbad* seems not to alert an interested party to the presence of HD 51001 in the *ASAS-3* catalogue[†], to which my attention was drawn by Miss Filiz Ak. The catalogue records no fewer than 350 photometric observations of HD 51001, covering seven observing seasons and demonstrating a repeating light-curve with a peak-to-peak amplitude of $0^m.17$ and a period of 138.2 days. There is no interval of constancy: in the 138-day period there are two nearly-equal maxima and two noticeably unequal minima — a β -Lyrae-type light-curve.

Soon after the star was drawn to my attention it was placed on the observing programme of the Cambridge *Coravel*, and 69 measurements have been made of its radial velocity in 2010–15. They are set out in Table IV and define an orbit that is not quite circular; its formal solution has the elements set out below and is illustrated in Fig. 4. No zero-point adjustment is needed for this red star: the Cambridge zero-point was originally set up²⁴ on stars almost the same colour as HD 51001.

$$\begin{aligned}
 P &= 138.07 \pm 0.09 \text{ days} & (T)_8 &= \text{MJD } 56199 \pm 6 \\
 \gamma &= +84.02 \pm 0.06 \text{ km s}^{-1} & T_0 &= \text{MJD } 56165.90 \pm 0.31 \\
 K &= 6.42 \pm 0.09 \text{ km s}^{-1} & a_1 \sin i &= 12.17 \pm 0.17 \text{ Gm} \\
 e &= 0.049 \pm 0.015 & f(m) &= 0.00378 \pm 0.00016 M_\odot \\
 \omega &= 87 \pm 16 \text{ degrees} & \text{R.m.s. residual} &= 0.50 \text{ km s}^{-1}
 \end{aligned}$$

* Caution! — the three leading figures of the serial number are given only at the head of each column (the column contains 70 entries), and where (as necessarily happens in most columns) they change part-way down the column, they very confusingly refer *not to the stars near the head of the column but to those at the foot*.

[†] It would be appropriate to give a reference here, but there seems to be none that could usefully be given. To refer to *ASAS-3*, the only thing to do seems to be to enter its name in a 'search' window on the Web.

TABLE IV

*Radial-velocity observations of HD 51001**All the observations were made with the Cambridge Coravel*

<i>Date (UT)</i>	<i>MJD</i>	<i>Velocity km s⁻¹</i>	<i>Phase</i>	<i>(O - C) km s⁻¹</i>
2009 Oct. 25.23	55129.23	+77.3	0.249	-0.3
2010 Mar. 4.94	55259.94	78.1	1.196	+0.2
6.92	261.92	77.4	.210	-0.3
22.91	277.91	78.4	.326	0.0
Nov. 24.13	524.13	80.0	3.109	+0.1
Dec. 19.10	549.10	77.9	.290	0.0
2011 Jan. 10.03	55571.03	81.5	3.449	-0.4
19.07	580.07	83.7	.514	-0.6
Apr. 6.85	657.85	80.7	4.078	-0.3
Oct. 16.22	850.22	82.8	5.471	+0.1
Nov. 18.16	883.16	90.1	.709	+0.1
23.14	888.14	89.3	.745	-1.1
28.13	893.13	89.9	.782	-0.5
30.08	895.08	89.5	.796	-0.9
Dec. 5.15	900.15	89.7	.832	-0.3
6.12	901.12	89.8	.839	-0.1
8.09	903.09	89.7	.854	+0.1
10.12	905.12	89.4	.868	+0.2
15.13	910.13	88.2	.905	0.0
18.10	913.10	88.5	.926	+1.1
2012 Jan. 2.10	55928.10	82.8	6.035	0.0
4.07	930.07	80.7	.049	-1.5
6.08	932.08	80.9	.064	-0.7
11.04	937.04	79.5	.100	-0.7
17.06	943.06	78.8	.143	-0.1
26.97	952.97	78.4	.215	+0.7
Feb. 2.00	959.00	78.2	.259	+0.5
7.91	964.91	78.8	.301	+0.8
10.98	967.98	79.0	.324	+0.6
18.90	975.90	79.7	.381	-0.1
19.93	976.93	79.9	.388	-0.1
24.96	981.96	80.3	.425	-0.8
Mar. 1.86	987.86	82.7	.468	+0.1
3.90	989.90	82.6	.482	-0.5
7.94	993.94	84.6	.512	+0.4
9.96	995.96	84.8	.526	+0.1
11.92	997.92	85.0	.541	-0.2
14.92	56000.92	85.7	.562	-0.3
Apr. 1.85	018.85	89.2	.692	-0.5
5.87	022.87	90.4	.721	+0.3
10.86	027.86	90.0	.757	-0.4
Nov. 18.16	249.16	79.5	8.360	+0.3
Dec. 26.10	287.10	88.2	.635	-0.1
2013 Feb. 7.00	56330.00	86.6	8.946	-0.1
Apr. 16.84	398.84	81.3	9.444	-0.5
Oct. 24.21	589.21	89.6	10.823	-0.5
Nov. 9.19	605.19	87.6	.939	+0.7
13.18	609.18	85.9	.968	+0.2
19.16	615.16	84.6	11.011	+0.8
Dec. 28.06	654.06	+77.6	.293	-0.3

TABLE IV (concluded)

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O-C) km s ⁻¹
2014 Jan. 27·02	56684·02	+84·1	11·510	0·0
Feb. 11·05	699·05	88·5	·619	+0·6
11·94	699·94	88·9	·625	+0·9
12·94	700·94	88·8	·632	+0·5
15·94	703·94	90·0	·654	+1·2
25·86	713·86	89·8	·726	-0·4
Mar. 3·82	719·82	90·7	·769	+0·2
7·87	723·87	90·0	·798	-0·4
19·84	735·84	89·3	·885	+0·5
Oct. 28·20	958·20	83·1	13·495	-0·5
Nov. 4·18	965·18	85·4	·546	0·0
6·16	967·16	86·2	·560	+0·3
8·21	969·21	86·5	·575	+0·1
10·16	971·16	87·4	·589	+0·5
24·12	985·12	89·8	·690	+0·2
Dec. 20·05	57011·05	89·7	·878	+0·7
30·04	021·04	87·0	·951	+0·5
2015 Jan. 11·04	57033·04	82·4	14·037	-0·3
22·98	044·98	79·4	·124	0·0
30·96	052·96	+78·2	·182	+0·1

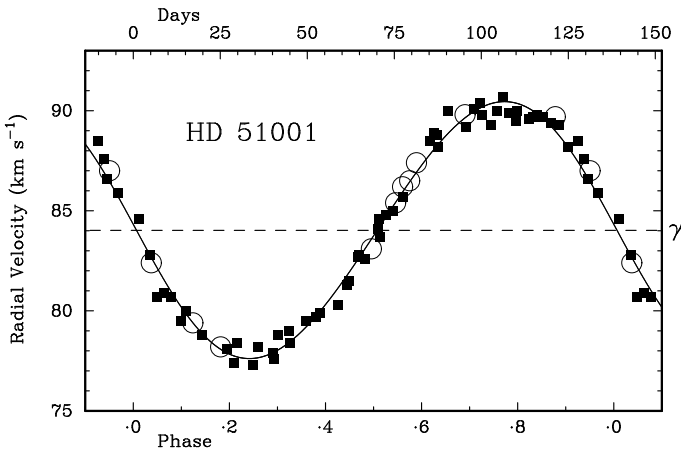


FIG. 4

As Fig. 1, but for HD 51001. Although all the observations were made with the Cambridge *Coravel*, those made in the last (2013/14) season are distinguished by being plotted with rather large open circles instead of the usual filled squares. The observer did not like the orbit delineated by the filled squares, which is nearly a sine wave but seemed to be too steep on the sides and too blunt at the extrema, so in the last season he took especially long-integrated radial-velocity traces, which certainly confirm the reality of the feature that he did not like. The feature may (in fact it seems that it *must*) be explicable in terms of ellipsoidal distortion and/or non-uniform surface brightness of the star. The secondary is not apparent in the traces and is probably too faint to register perceptibly.

In an orbit so nearly circular, the epoch of periastron inevitably has a large uncertainty, which is not reflected in the uncertainty of the positioning of the orbit graph along the time axis. In such a case, it is useful also to give the epoch T_0 , the time of maximum velocity; its uncertainty is seen to be about

$1/20$ of that of T , perhaps reflecting the value of e itself. A very noticeable (and observationally adverse) characteristic of the ‘dips’ in radial-velocity traces of HD 51001 is their great width: they are smeared out, no doubt by stellar rotation, to a degree corresponding to a $v \sin i$ of about 18 km s^{-1} — formally, the mean value from all the traces is $18.21 \pm 0.13 \text{ km s}^{-1}$, but the true value is not claimed to be accurate to better than an integer number of kilometres per second. What with the very small apparent eccentricity of the orbit, and a rotational velocity nearly three times the orbital velocity amplitude, there is every likelihood that the orbit truly is circular and that the apparent deviations from circularity arise from non-uniformities of surface brightness and radius of the star itself, as is suggested by the substantial photometric variations.

The phasing of those variations is such that the deeper of the two dips in the light-curve occurs very nearly at zero phase or half-way down the descending side of the orbital velocity curve — a time when the star is at the near side of its orbit. It might be imagined that the light minimum is exacerbated by eclipse of the secondary, but the mass function is very small, and does not encourage the idea that the minimum could be due to, or much enhanced by, any eclipse of the companion star. Despite the smallness of the mass function, the primary star may be supposed to be so large and tenuous as to be significantly distorted into an ellipsoid with its major axis aligned with its companion; that would explain the phasing of the minima as occurring when the area of the stellar disc, as we see it projected on the sky, is a minimum. The greater depth of the minimum at zero phase might be due to our seeing then the ‘cooler’ end of the star, the other end being heated somewhat by the nearby companion.

The non-adherence of the radial velocities to the sine wave expected of a circular orbit is manifested not only in the probably spurious non-zero value of e but also in velocity changes that are too small in the vicinities of the nodes and too steep near the conjunctions. The writer was so bothered by that appearance that he took longer integrations on the star in the final observing season (the 11 observations in 2014–15, distinguished in Fig. 4): those velocities, though not given extra weight in the solution, have mean-square residuals less than half those of the other 58, and reinforce the observed deviations from a Keplerian orbit.

It seems troublesome that HD 51001, though plenty bright enough to have been on the *Hipparcos* programme, was not in fact observed by *Hipparcos*, and in the absence of a parallax and even of an MK type there is nothing to give us a luminosity estimate apart from one that we might try to make for ourselves. A star that rotates at 18 km s^{-1} and yet takes 138 days to turn (if that is really what it does, with the rotation synchronized to the orbit) must be a large star — the numbers lead to a projected radius of about 35 Gm or about $50 R_{\odot}$, which is 25% greater than most tabulations give for the radius of a star of type Mo III. Since, on the same hypothesis, the star’s own radius must be nearly three times that of its orbit around the centre of gravity of the system, the companion star must have a mass less than half that of the primary just in order to be separated from it, but there is little else known that could serve as the basis of speculation on the nature of the secondary star.

HD 85843

HD 85843 is a 7^{m} star in the ‘head’ of Leo; it is nearly $1\frac{1}{2}$ degrees south-following $\mu \text{ Leo}$, a 4^{m} star and one whose designation as ‘super-metal-rich’ by Spinrad & Taylor²⁵ the best part of half a century ago sparked a major controversy. For HD 85843, we are indebted once more to *Tycho 2* for its photometry, $V = 7^{\text{m}}.07$, $(B - V) = 0^{\text{m}}.57$. It has been classified by Harlan²⁶

as F8V (the *Henry Draper* type, too, is F8). Eggen at one time²⁷ included it as a member of his ‘Hyades–Sirius supercluster’ but, if he had really been committed to that idea, it would have been typical of him to have repeated it in several other papers, something that did not happen. HD 85843 has a recently recognized²⁸ common-proper-motion companion some 7' distant.

Hipparcos recognized the star as double, not by seeing it as double but owing to slight variation of its position in the sky — the *Hipparcos* position resulted from an ‘acceleration solution’. The parallax⁵ is 15.50 ± 1.37 arc-milliseconds, which inverts to a distance of about 64 ± 5 parsecs (not 64.520 parsecs, as specified with such unwarranted precision by McDonald *et al.*⁶); the distance modulus is $3^{\text{m}}.02 \pm 0^{\text{m}}.20$, yielding an absolute magnitude of $4^{\text{m}}.05$ with practically the same uncertainty — pretty well spot on the absolute magnitude of $4^{\text{m}}.0$ that tabulations usually give against the type of F8V.

In 1999 Suchkov & McMaster²⁹ developed a criterion of what they called ‘over-luminosity’ for F-type stars, where the absolute magnitude, M_V , found from the *Hipparcos* parallax was significantly brighter than what was, in effect, a spectroscopic estimate of luminosity, M_{c_0} , yielded by the c_0 index formed from photometry in the Strömgren *uvby* photometric system. Those authors found that, for bright single F stars within 25 pc, the scatter of $\Delta M_{c_0} = |M_V - M_{c_0}|$, was about $0^{\text{m}}.15$. Stars with significantly large values of ΔM_{c_0} were likely to be binary systems; in some cases that was already known to be so, but in many others it was not. At the invitation of the present author, who offered to make systematic radial-velocity measurements of stars thus suggested to be binary systems, Suchkov supplied a list of 118 stars that had $\Delta M_{c_0} > 0^{\text{m}}.5$, were at declinations accessible to the Cambridge telescope, and satisfied certain other constraints that are noted by Griffin & Suchkov in ref. 30, where the results of the survey of the 118 stars were published. Orbits were in fact already known for 27 of the objects (six of them were the work of the present writer); after only about two years’ observations the paper³⁰ was able to present orbits for another 28, most of them not previously recognized as binaries and mostly SB2 and thereby accounting for their large values of ΔM_{c_0} . Six additional binary systems were discovered that had periods longer than the project had been running and whose orbits therefore could not be given at that time*; one of them was HD 85843.

The initial radial-velocity observation of HD 85843 was made (with the then-new Cambridge ‘*Coravel*’ spectrometer, like all subsequent ones) in 2000 February. The star was not observed again until the succeeding observing season, in November of that year, when it was found to have changed its velocity by more than 10 km s⁻¹. Subsequent measurements, up to the closing date for the compilation of the paper³⁰ at the beginning of 2002, brought the total number to 14 and covered a minimum of radial velocity, illustrated in Fig. 4 of that paper. The measurements were of course continued, normally on a monthly basis during the season when the star was accessible. For a long time the star was regarded as single-lined, and was so designated in the Griffin/Suchkov paper³⁰ of 2003. It was not until the 38th observation, made in 2005 April, that a very weak secondary ‘dip’ was recognized in the radial-velocity trace; thereafter, the traces were reduced as double-lined when the system was near a node of the orbit, though the weakness of the secondary made it impossible, or at least unprofitable to attempt, to disentangle the traces at other

*Three of the six (HD 103613, 212754, and 215977) have since had their orbits published in the series of papers of which *this* one is a member, and HD 85843 now makes a fourth. The remaining ones are HD 46781 (23 Gem, an observationally troublesome double-lined star whose components are always blended together in radial-velocity traces) and HD 54451, apparently single-lined, with an orbit of about 18 years’ period that has not yet been seen round a full cycle.

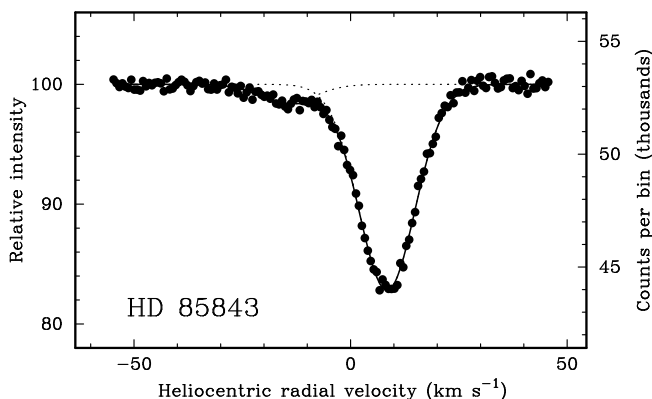


FIG. 5

Radial-velocity trace of HD 85843, obtained with the Cambridge *Coravel* on 2007 March 22, showing the very unequal dips at practically their maximum separation.

phases. To obtain somewhat meaningful velocities for the secondary, even near the node, it has been necessary to integrate the traces to considerably higher photon-count levels than was considered needful before the system was found to be double-lined. The amplitudes of the velocity changes are too small for the dips ever to appear separated. Fig. 5 illustrates a trace obtained close to the more favourable (greater ΔV) node of the orbit; it makes it easy to realize that the disparity (by a factor of about 12), and permanent mutual blending, of the dips militates against accurate measurements of the secondary.

Ironically, the secondary dip can actually be recognized, at least by the eye of faith, blended into the red wing of the trace that was chosen to illustrate the ‘dip’ given by HD 85843 in Fig. 4 of the Suchkov paper³⁰. An effort to re-reduce that trace (which was selected for the illustration because the photon count was mildly greater than was usual at that time for that star) as double-lined was not really successful, and it has been concluded that no useful purpose would be served by trying to reduce the traces obtained before the recognition of the weak secondary; since that time, naturally, the observations have been integrated for longer, as has been indicated above. Even so, it has proved possible to obtain sensible double-lined reductions of them only in the vicinities of the nodes of the orbit. All the available velocities are set out in Table V, which includes 76 measurements of the primary and 28 of the secondary star. Although in this case there are no other sources of velocities to discuss with the Cambridge ones, we *know* (cf. the situation with HD 26441 above) that bluish stars need negative corrections to Cambridge velocities, so in an effort to keep near the same zero-point as usual the velocities here have all been adjusted by -0.6 km s^{-1} , like the Cambridge ones of HD 26441 which has a somewhat similar colour index.

Despite the weakness of the secondary dip, the observations that were reduced as single-lined have been weighted only $\frac{1}{4}$ in the solution of the orbit because they have about double the r.m.s. residuals from the orbit in comparison with those that were reduced as SB2. That is in part because all the early observations, and later ones where the two dips were inextricably blended together, did not receive such generous integrations as those from which it was hoped to derive the twin velocities. But another contribution to the relative raggedness of the observations reduced as if they were single-lined is the actual ‘dragging’ of the

primary dip slightly towards the velocity of the secondary when the two are blended together and it is the blend and not the primary that is being measured. The effect can be recognized directly in the orbit plot (Fig. 6), particularly near phases .6 and .8, where the velocities attributed to the primary but actually of the blend are systematically pulled away from the computed velocity curve towards the γ -velocity.

The orbit is readily determined from the velocities in Table V, with the measurements of the secondary weighted only $1/40$ in comparison with full-weight observations of the primary. Two secondary velocities that have excessive residuals have been rejected altogether. It was noticed that they stem from the two observations that had the smallest photon counts among all of those that were reduced as SB2; the count levels were nevertheless quite enough to give satisfactory velocities for the primary, which were accepted with full weight in the orbit. The resulting elements are listed below, and the orbit is plotted in Fig. 6.

P	$= 1387.5 \pm 1.0$ days	$(T)_2$	$= \text{MJD } 54337 \pm 6$
γ	$= -0.45 \pm 0.03$ km s $^{-1}$	$a_1 \sin i$	$= 169.2 \pm 0.8$ Gm
K_1	$= 9.00 \pm 0.04$ km s $^{-1}$	$a_2 \sin i$	$= 227 \pm 6$ Gm
K_2	$= 12.09 \pm 0.30$ km s $^{-1}$	$f(m_1)$	$= 0.1005 \pm 0.0014 M_\odot$
q	$= 1.34 \pm 0.03$ ($= m_1/m_2$)	$f(m_2)$	$= 0.244 \pm 0.018 M_\odot$
e	$= 0.171 \pm 0.005$	$m_1 \sin^3 i$	$= 0.74 \pm 0.04 M_\odot$
ω	$= 40.9 \pm 1.8$ degrees	$m_2 \sin^3 i$	$= 0.552 \pm 0.016 M_\odot$

$$\text{R.m.s. residual (wt. 1)} = 0.20 \text{ km s}^{-1}$$

The secondary star contributes so little to the total light that the spectral type of F8 found for the system as a whole must for practical purposes be accepted as the type of the primary star. Tabulations of the properties of main-sequence stars assign masses of about $1.2 M_\odot$ to type F8, so the quantity determined as $m_1 \sin^3 i$ in the informal table above can be equated to 1.2 and treated as an equation giving $\sin^3 i$, which comes out at 0.62 . That value has an uncertainty (compounded quadratically from the 6% of the determination here plus a comparable allowance for the uncertainties of the spectral type and the exact relationship between type and mass) of nearly 10%. We accordingly find $\sin i \sim 0.85 \pm 0.03$, leading to an orbital inclination of about 58 ± 3 degrees.

We could consider the nature of the secondary from two somewhat independent directions. They both need to make the assumption, almost certainly true, that that star is another main-sequence star (as the primary evidently is, in view of the agreement between its spectral type and its luminosity), but unfortunately they do not give accordant results. The mass of the secondary is less than that of the primary by the factor $1/1.34$, with a formal uncertainty of only 0.03 in the 1.34 , making the secondary nearly $0.90 M_\odot$, which corresponds to a type of about G6. At the same time, the areas of the dips in radial-velocity traces have a ratio of 11 or 12 to 1 , which expressed in magnitude terms is $2^{\text{m}.6}$ or $2^{\text{m}.7}$. The increasing line strength towards later spectral types, together with the improving correspondence between the stellar spectra and the (K2) mask within the *Coravel* instrument, have led to the adoption of an empirical factor of 1.15 relating the actual magnitude difference of a pair of F/G main-sequence stars to the difference in their radial-velocity dip areas: it does not take such a bright star, if its spectrum matches the mask better, to give a particular dip area. Applying that factor in the present case we obtain $\Delta m \sim 3^{\text{m}.0}$. Taking the absolute magnitude of the primary as $4^{\text{m}.0}$, we find the

TABLE V
Radial-velocity observations of HD 85843

All the observations were made with the Cambridge Coravel.
Where both components are measured, the primary velocity has received unit weight in the orbit, the secondary $1/40$ (except for the two flagged rejections).
The lone primary observations (really measurements of the blend) have been weighted $1/4$.

Date (UT)	MJD	Velocity		Phase	(O - C)	
		Prim. km s ⁻¹	Sec. km s ⁻¹		Prim. km s ⁻¹	Sec. km s ⁻¹
2000 Feb. 20.05	51594.05	+5.5	—	0.023	-0.6	—
Nov. 17.26	865.26	-5.7	—	.219	+0.4	—
Dec. 2.23	880.23	-5.8	—	.229	+0.7	—
3.20	881.20	-5.7	—	.230	+0.8	—
14.19	892.19	-6.2	—	.238	+0.6	—
2001 Jan. 6.16	51915.16	-7.0	—	0.255	+0.2	—
Feb. 7.09	947.09	-7.3	—	.278	+0.4	—
Mar. 3.02	971.02	-8.1	—	.295	-0.1	—
Apr. 28.91	52027.91	-8.0	—	.336	+0.3	—
May 11.90	040.90	-7.8	—	.345	+0.5	—
Oct. 12.20	194.20	-7.0	—	.456	+0.1	—
Nov. 1.21	214.21	-7.1	—	.470	-0.3	—
Dec. 12.22	255.22	-6.3	—	.500	-0.2	—
14.23	257.23	-6.2	—	.501	-0.1	—
2002 Jan. 18.09	52292.09	-5.2	—	0.526	+0.1	—
Feb. 15.00	320.00	-3.8	—	.546	+0.9	—
Mar. 8.03	341.03	-4.0	—	.562	+0.2	—
Apr. 3.95	367.95	-2.5	—	.581	+1.0	—
May 4.92	398.92	-2.2	—	.603	+0.5	—
June 1.90	426.90	-1.6	—	.623	+0.2	—
Oct. 19.22	566.22	+2.3	—	.724	-0.5	—
Dec. 5.20	613.20	+3.9	—	.758	-0.5	—
2003 Jan. 5.18	52644.18	+4.9	—	0.780	-0.6	—
Feb. 15.09	685.09	+6.2	—	.810	-0.6	—
Mar. 16.97	714.97	+7.7	—	.831	0.0	—
Apr. 15.93	744.93	+8.3	—	.853	-0.2	—
May 13.91	772.91	+9.0	—	.873	-0.1	—
Nov. 4.23	947.23	+7.3	—	.999	-0.3	—
Dec. 8.18	981.18	+6.3	—	1.023	+0.1	—
2004 Jan. 9.14	53013.14	+4.2	—	1.046	-0.4	—
Feb. 9.07	044.07	+2.6	—	.068	-0.4	—
Mar. 16.99	080.99	+0.9	—	.095	-0.1	—
Apr. 13.92	108.92	-0.3	—	.115	+0.2	—
May 10.89	135.89	-1.7	—	.134	+0.1	—
Oct. 27.20	305.20	-7.7	—	.257	-0.4	—
2005 Jan. 9.11	53379.11	-8.2	—	1.310	-0.1	—
Mar. 25.96	454.96	-8.0	—	.364	+0.3	—
Apr. 21.92	481.92	-8.3	+10.1	.384	-0.2	+0.2
May 21.89	511.89	-8.1	+9.3	.405	-0.2	-0.3
Nov. 25.21	699.21	-4.4	—	.540	+0.5	—
2006 Apr. 3.99	53828.99	-1.3	—	1.634	+0.1	—
May 9.93	864.93	-0.7	—	.660	-0.4	—
June 5.89	891.89	+0.5	—	.679	-0.1	—
Nov. 3.24	54042.24	+5.6	—	.788	-0.3	—
Dec. 9.27	078.27	+7.2	-5.9*	.814	+0.2	+4.6

TABLE V (concluded)

Date (UT)	MJD	Velocity		Phase	(O-C)	
		Prim. km s ⁻¹	Sec. km s ⁻¹		Prim. km s ⁻¹	Sec. km s ⁻¹
2007 Jan. 14:18	54114.18	+7.9	-13.7	1.840	-0.2	-1.8
Feb. 4:06	135.06	+8.8	-10.9	.855	+0.2	+1.7
15:09	146.09	+8.7	-12.7	.863	-0.1	+0.2
Mar. 3:97	162.97	+9.3	-12.6	.875	+0.2	+0.8
22:00	181.00	+9.5	-13.3	.888	+0.1	+0.4
Apr. 4:97	194.97	+9.6	-13.6	.898	0.0	+0.3
18:91	208.91	+9.8	-13.9	.908	+0.1	+0.2
May 12:88	232.88	+9.5	-18.4*	.925	-0.2	-4.3
31:91	251.91	+9.6	-14.7	.939	0.0	-0.8
Nov. 9:25	413.25	+3.9	-6.5	2.055	0.0	-0.2
24:22	428.22	+3.0	-6.6	.066	-0.1	-1.3
2008 Feb. 2:08	54498.08	-0.2	—	2.116	+0.4	—
Mar. 30:91	555.91	-2.8	—	.158	+0.4	—
May 19:93	605.93	-5.3	+6.3	.194	-0.2	+0.5
Dec. 7:25	807.25	-8.5	+7.7	.339	-0.2	-2.4
2009 Jan. 6:17	54837.17	-8.3	+10.7	2.361	0.0	+0.6
Apr. 19:94	940.94	-7.7	+7.0	.435	-0.2	-2.0
2010 Feb. 6:08	55233.08	-0.4	—	2.646	+0.5	—
Apr. 17:88	303.88	+1.3	—	.697	-0.2	—
2012 Jan. 4:17	55930.17	-2.3	—	3.148	+0.4	—
Feb. 2:09	959.09	-3.8	+7.1	.169	+0.1	+3.0
19:05	976.05	-4.4	+6.8	.182	+0.1	+1.8
Apr. 5:97	56022.97	-6.1	+9.0	.215	-0.1	+2.0
May 26:90	073.90	-7.3	+8.9	.252	-0.2	+0.4
Dec. 2:26	263.26	-8.4	—	.389	-0.3	—
2013 Mar. 13:99	56364.99	-6.9	+9.6	3.462	+0.1	+1.2
Apr. 5:94	387.94	-6.8	+8.1	.478	-0.2	+0.2
2014 Apr. 16:93	56763.93	+3.9	-7.4	.749	-0.1	—
2015 Jan. 20:16	57042.16	+9.8	-12.7	3.950	+0.4	+1.0
Mar. 27:06	108.06	+7.6	-11.8	.997	0.0	-0.5
Apr. 29:92	141.92	+6.2	-8.3	4.022	0.0	+1.1

*Rejected

secondary to be 7^m.0, which corresponds to a main-sequence type of about K4. The mass of such a star is always tabulated at about 0.7 M_{\odot} , so the expected ratio of the radial-velocity amplitudes should be about 1.7, more than ten standard deviations away from the 1.34 ± 0.03 found from the orbital elements. The writer is naturally embarrassed at leaving such a glaring incongruity in this discussion, but he sees no plausible means of reconciling the results of the two methods of deriving the character of the secondary star. Despite the embarrassment, he feels that it would be irresponsible, having once noticed the discrepancy, to refrain from saying anything about one (or both) of the approaches to the question of the secondary, and thereby to sweep the issue under the carpet. The best that can be said is that that star is certainly a lot fainter than the primary, but it gives a surprisingly weak dip in radial-velocity traces in relation to the mass ratio indicated by the orbital elements.

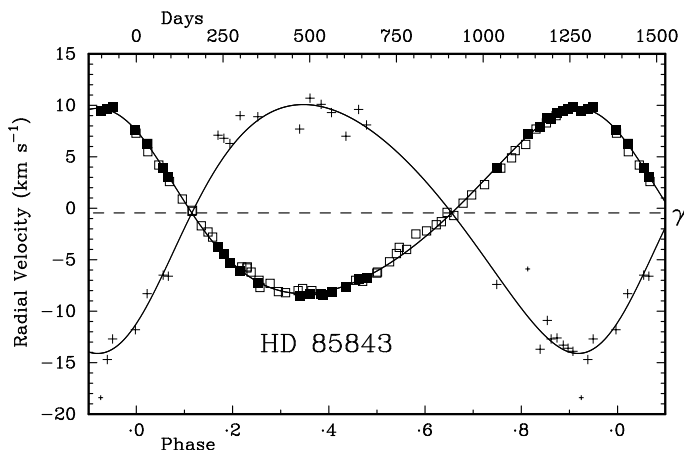


FIG. 6

The observed radial velocities of HD 85843 plotted as a function of phase, with the velocity curves corresponding to the adopted orbital elements drawn through them. All the observations were made with the Cambridge *Coravel*. Observations of the primary are plotted as squares. Those that were reduced as double-lined are shown with filled symbols; those that could not be so reduced and really measure the velocity of the blend of the two components appear as open symbols. Since the secondary dip is a very weak feature (cf. Fig. 5), the blend is usually not a bad substitute for the primary, yet in the neighbourhood of the conjunctions (particularly the one near phase .6) the velocity can be seen to be ‘dragged’ towards the γ -velocity by the adulteration of the primary dip with that of the secondary. To bring the variances of the velocities into approximate equality in the solution of the orbit, in which blend velocities have been used as if they were velocities of the primary, it has been necessary to down-weight the blends by a factor of 4 in comparison with genuine primary velocities stemming from double-lined reductions. Velocities of the secondary, which are understandably very ragged, are plotted as pluses and have merited a weight of only $\frac{1}{40}$ in the solution of the orbit, even after the rejection of the two worst ones, which are plotted with diminutive symbols.

Acknowledgement

I am pleased to record my gratitude to Dr. J. Andersen for advice concerning HD 51001.

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REVIEWS

Mt John: The First 50 Years, by J. Hearnshaw & A. Gilmore (Canterbury University Press, Christchurch), 2015. Pp. 215, 24 × 31 cm. Price NZ\$59.99 (about £26) (hardbound; ISBN 978 1 927145 62 3).

The richness of the southern sky in comparison to the northern one is apparent to anyone travelling to the Southern Hemisphere, and just half a century ago the number of large telescopes available to tap that cornucopia was rather limited, which led Frank Bradshaw Wood, of the University of Pennsylvania, to team up with the dynamic New Zealand amateur, Frank Bateson, to build an observatory on Mt. John in New Zealand's South Island. Furthermore, to short-period-variable-star observers (as Brad Wood was), the rotation of the Earth and consequent daily interruption of their work has long been something of a problem, and to overcome it (in the days before satellites) they had been moved to look to a network of telescopes spread around the globe for an uninterrupted flow of data. And the longitude of Mt. John neatly fills a gap there too. The rest, as they say, is history, and that full history is laid bare in this excellent volume compiled by John Hearnshaw & Alan Gilmore to celebrate 50 years of that observatory.

The story is one of gradual growth, often against a background of financial pressure and personality clashes, fully aired in this no-punches-pulled account, until most recently when the Japanese joined the fray and chipped in with the 1.8-m *MOA (Microlensing Observations in Astrophysics)* telescope*. All that is described and copiously illustrated with pictures not only of the instruments and personalities but also of the stunningly beautiful region around the observatory, including the glorious turquoise glacial Lake Tekapo. The site is now incorporated into an internationally recognized dark-sky reserve, and a booming outreach programme is already making the most of it to popularize astronomy.

In the early 1980s — thus in the first half of the half-century being celebrated here — I had the pleasure of a short observing run at Mt. John and can thus delight in this update. The book also records the progress of the astronomical endeavours of the University of Canterbury in Christchurch with a list of the post-graduates, both home-grown and visitors, who have flourished there. Clearly, for a country with a relatively small population, New Zealand has punched well above its weight in astronomy — as it long has in rugby and more recently in cricket; it even provided an Editor for this *Magazine* in the 1990s! I trust that in due course we'll get a book to celebrate the first century of the Mt. John Observatory. — DAVID STICKLAND.

* One hopes that this MOA doesn't soon go the way of the other Moa, an enormous flightless bird once found in New Zealand!

Mary Somerville and the World of Science, by Allan Chapman (Springer, Heidelberg), 2015*. Pp. 92, 23.5 × 15.5 cm. Price £44.99/\$54.99 (paperback; ISBN 978 3 319 09398 7).

Minor astronomers might aspire to have an asteroid named after them, and the more observant might discover their own comet, but to have an Oxford University college is really entering the major league. Mary Fairfax Somerville (1780–1872) was not a ‘blue stocking’ but a great believer that intelligent, thinking, scientific, and industrious women should be demonstratively feminine. She was a staunch believer that women should have equal educational and career opportunities to men. But far from just banging the Victorian drum of gender politics she skilfully led by example, showing the world exactly what women were capable of.

At heart Somerville was forced, by the fashion of the times, to be a scientific communicator and not a discoverer. She did this by writing four extremely influential books: *On the Mechanism of the Heavens* (1831), *On the Connexion of the Physical Sciences* (1834), *Physical Geography* (1848), and *Microscopic and Molecular Science* (1869). These were universally widely read and went into many editions.

Allan Chapman, the Oxford historian of science, has produced a delightful, very readable, and eruditely referenced biography of this influential pioneering woman scientist. He skilfully stresses the way in which she merged into the intellectual and social scene of the times and used her considerable mathematical and astronomical talents in unsupportive circumstances. Mary Somerville was at the heart of the country’s scientific endeavour and was friends with and corresponded often with the likes of John Herschel, William Buckland, Thomas Young, and William Wollaston. Throughout the book we are, however, subliminally presented with two nagging questions. “How would things have turned out if Mary had been born a man?”; and “What would have happened if she had been born at a different time?”. Happily things have greatly improved in the over-140 years since Mary’s death, but we have still some distance to travel. — DAVID W. HUGHES.

Narratio Prima or First Account of the Books on the Revolutions by Nicolas Copernicus, by Georg Joachim Rheticus (Nicolas Copernicus Foundation, Rybno, Poland), 1540, reprinted in facsimile, with an introduction by Jarosław Włodarczyk, 2015. Pp. 150, 20.5 × 14 cm. Price Zł35 (about £6) (hardbound; ISBN 978 83 941726 1 7).

Rheticus (birth surname Iserin, later de Porris and, finally, von Lauchen, using a toponym) was the young scholar who, coming to be mentored by Copernicus late in the latter’s life, finally persuaded him to put his work on a heliocentric cosmos into print in 1543. About three years before, Rheticus himself wrote a “first account” of the ideas. Printed in Danzig, copies of the *Narratio Prima* soon reached readers in centres of mathematical and astronomical investigation throughout central Europe and beyond.

The Introduction, which forms the first half of the present volume, takes us from the birth of Rheticus in 1514 to work by Kepler in 1621 that included a fifth printing of *Narratio Prima* (in contrast, *De Revolutionibus* itself received only three editions in the 16th Century). Part II is the facsimile of the first, Danzig, edition, complete with two pages of “Errata sic Corrige” and three pages of translations of Greek phrases used by Rheticus into Latin. There is

*This 2015 Springer book is a reprint of the book originally published by Canopus Publishing Limited, Bristol, in 2004.

some comfort to find, after struggling with a word that begins “*χρον* ...”, to see “*a tempore*”. We also get a facsimile of the first page of *De Revolutionibus Orbium Coelestium Libri* itself, with the “*Orbium Coelestium*” crossed out in red (Fig. 6), while Fig. 7 is the notorious preface “*Ad lectorem de hypothesibus huius operis*” added anonymously by Osiander, completely crossed out in red. Both are from the copy that Rheticus presented to Georg Donner (no, he won’t be on the exam). Włodarczyk’s introduction tells us that the crossing-out was done by Rheticus himself, not by the recipient.

Crossing out the preface made sense, since Copernicus would definitely not have wanted the claim therein that heliocentrism was merely a calculating device, not a picture of the real world. Cancelling “*Orbium Coelestium*” strikes me as more mysterious, but clearly justifies the scholarly custom of referring simply to *De Revolutionibus*.

A simultaneously-published Polish edition has the same introduction and a Polish translation of *Narratio Prima*, rather than the facsimile. Some of the material covered in the introduction can also be found in Dennis Danielson’s *The First Copernican* (reviewed earlier in these pages (127, 340)).

Experts debate vigorously the extent to which Copernicus (and presumably Rheticus) wanted their improved theory for astrological purposes. But the latter, at least, so used it, predicting, for instance, the fall of the Ottoman Empire for 1652, following a triple planetary conjunction. He was off by about 250 years.

Conflict of interest: my copy of this utterly fascinating book was a ‘presentation copy’ signed by Robert Szaj, the General Director of The Nicolas Copernicus Foundation, which sponsored the publication, along with half a dozen commercial organizations, whose logos appear on the last couple of pages. Additional support came from the Town of Lubawa, where Copernicus and Rheticus were living in 1539, when the decision was made to publish *De Revolutionibus* and where Rheticus wrote *Narratio Prima*. — VIRGINIA TRIMBLE.

Stars, Myths and Rituals in Etruscan Rome, by Leonardo Magini (Springer, Heidelberg), 2015. Pp. 181, 24 × 16 cm. Price £90/\$129 (hardbound; ISBN 978 3 319 07265 1).

Here we are spirited back to the days of Numa Pompilius (715 – 673 BC), the second king of Rome. He greatly improved the Romulean calendar and introduced the first western calendar that had a clear astronomical link to the solar year. That replaced the primitive lunar calendar which simply relied on counting the appearances of new moons, a calendar that very quickly got out of step with agricultural and pastoral observations. It also removed reliance on the imprecise anniversaries of such natural events as the onset of lambing time and the return of certain migratory birds. The new Numan calendar incontrovertibly showed that Ovid was wrong — the Roman academia was not an astronomy-free zone — in fact the Romans were very interested in astronomy and had a detailed knowledge of celestial periodicities, if only to ensure that their feast days occurred when they should.

Magini’s central interest concerns the origins of the Roman calendar, and specifically how much it was based on Etruscan (Tuscan) ideas and how much of those, in their turn, were based on practices originating in the Middle East. The main calendrical problem of the time was deciding which days were normal days (when you had to work) and which days were feast days (when you could let your hair down). The latter came with the problem of choosing a specific god or goddess for the specified day.

Stars, Myths and Rituals in Etruscan Rome is an intriguing, detailed, erudite, and informative book that concentrates on problems such as the naming

of the months, the relationship between the three graces (Fortuna and her handmaidens) and the new moon and its waning and waxing crescents, the celestial movement of Venus and its relationship to the feast days of Veneralia and Matralia, and the solstitial feasts of Saturnalia and Fors Fortuna. Magini also investigates such things as the influence of solar and lunar eclipses and the understanding of the Saros cycle, the relationship between Summanus and Compitalia and the revolution of the lunar nodes and line of apsides, and the feast days of Terminalia and Anna Peranna and the entrance of the Sun into the constellations of Pisces and Aries. Associated rites and myths are stressed throughout. (I was rather surprised, for example, to learn that in those times the most auspicious day for a woman to get married was April 1, and the favoured day for conception was June 11.)

The book ends with a detailed chart that lists all the days of the year and highlights the festival days and the related divinities. When it comes to present-day holidays we certainly live in much less complicated times. — DAVID W. HUGHES.

New Insights from Recent Studies in Historical Astronomy: Following in the Footsteps of F. Richard Stephenson, edited by Wayne Orchiston, David A. Green & Richard Strom (Springer Heidelberg), 2014. Pp. 359, 24 × 16 cm. Price £171/\$259 (hardbound; ISBN 978 3 319 07613 3).

Historical astronomy involves the application of old records to modern astrophysical problems. The proponents of this subject have to have a belief that our ancient predecessors knew what they were doing, and tried their best to record their observations accurately. So, for example, if King Mirian of Georgia says that he saw a total solar eclipse on 6th May AD 319, when standing on top of a mountain near Mtskheta, that eclipse happened when he said it did, and was visible from where he was.

One of the first historical astronomers was England's second Astronomer Royal, Edmund Halley (1656–1742). He discovered that the Earth–Moon distance was changing, that stars had a proper motion, and that a certain comet was periodic. The modern leader in the field is Emeritus Professor Richard Stephenson, who has spent most of his academic life at Durham University. His historical-astronomy research has resulted in him receiving not only the Jackson-Gwilt Medal of the Royal Astronomical Society, the Tompion Medal of the Worshipful Company of Clockmakers, and the LeRoy E. Doggett prize of the American Astronomical Association, but he now has the *festschrift* under review dedicated to his work. The occasion was the celebration of his 70th birthday in 2011 April. Stephenson's main contributions concern the variation of the Earth's spin rate, observations of supernova explosions, the past orbit and visibility of Halley's Comet, the variability of solar sunspot and auroral activity, early oriental star maps, and ancient chronologies. All these studies are based on his interpretation and deciphering of ancient records from China, Japan, Korea, Babylon, the Arabic world, and Europe.

The book is divided into three sections. The first part concentrates on the major topics of Stephenson's research mentioned above. Part two considers Islamic and Oriental astronomy and specifically Stephenson's dedication to the accurate and detailed investigation and translation of historical Chinese astronomical records and diaries and also tomes such as al-Šūfi's *Book of Fixed Stars* (AD 964). Richard Stephenson has been a life-long supporter of amateur astronomy societies (and especially the Newcastle Astronomical Society where he has been the president for the last thirty years), hence section three of the *festschrift* concentrates on the contributions to astronomy of such renowned

19th-Century ‘amateur’ (*i.e.*, enthusiastic, highly competent, but self-funded) astronomers such as Johann Schroeter, Henry Beighton, Carl Rümker, João de Moraes Pereira, R. T. A. Innes, and C. J. Merfield.

This book is a delight. It is beautifully illustrated, superbly produced, and well referenced, and the seventeen individual research papers underline the vibrancy, variety, and importance of the contributions of today’s students of historical astronomy and the history of astronomy. — DAVID W. HUGHES.

Skyscapes: The Role and Importance of the Sky in Archaeology, edited by Fabio Silva & Nicholas Campion (Oxbow Books, Oxford), 2015. Pp. 148, 24 × 17 cm. Price £38 (paperback; ISBN 978 1 782978 40 4).

One of the main problems with astroarchaeology is that there is no one to ask, and no written records to consult. You stand there looking at Stonehenge or Avebury in Wiltshire, or the temple of Ramses II on the west bank of Lake Nasser, or Mnajdra in Malta, or recumbent stone circles in north-east Scotland, or dolmen at Manorbier in south Pembrokeshire, and you wonder if their constructors had any serious thoughts for the sky above. Unfortunately the sky presents a host of potentially relevant phenomena. We have, for example, solstitial risings and settings of the Sun, lunar excursions on the east and west horizons, and lots of stellar candidates, such as Aldebaran, Sirius, Orion’s Belt, and the Pleiades. If only you could chat to a stone-age bystander and ask him what he thought was significant.

Today’s astro-archaeology academic can thus suggest a host of possibilities, and this multiplicity and uncertainty tends to lessen the perceived rigour of the subject to acquaintances and rivals in allied fields. To counteract this the astro-archaeologists, and especially most of the eleven who have contributed papers to the volume under review, seem to try to enhance their credibility and status by resorting to unnecessary jargon and verbosity. The papers in this book originated from a full-day archaeo-astronomy session at the 2012 Liverpool Theoretical Archaeology Group conference. Unfortunately the papers resound with phrases like “foregrounding essentially post-processualist thinking”. We are asked to “dismantle the nature::cultural dualism and its Cartesian implications”. And we must continually worry about ways in which “skyscapes are enculturated”.

This obfuscation is a pity, because there is a great deal to be admired in this book. It is beautifully illustrated, well referenced, and extremely thought-provoking. When we go back well over 2000 years humanity had far fewer distractions than it does today. And clearly the sky — acting as it did then as a clock, compass, calendar, and myth generator — had greater significance. Trying to understand the prehistoric importance of the sky is clearly a noble task and a subject greatly deserving academic effort. But as an ‘advertisement’ to encourage new disciples into the field I am not too sure that this book quite hits the mark. I know the difference between ‘land’ and ‘landscape’, but the book under review did not convince me that there was a real difference between ‘sky’ and ‘skyscape’. Inventing new words does not really help. — DAVID W. HUGHES.

Philosophy of Chance, by Michael Heller (Copernicus Center Press, Krakow), 2012, English translation of 2011 Polish original. Pp. 231, 24 × 15.5 cm. Price €39.90 (about £28) (hardbound; ISBN 978 83 7886 000 6).

Michael Heller (Michał Kazimierz, when he is home in Tarnów, Poland) holds degrees in theology, philosophy, and cosmology and has written about them all,

most often in recent years about the interfaces among them. Thus the present volume does not, for the most part, tell us about the Universe or what to think about the Universe, but, more nearly, how to try to think about the Universe through the role that chance plays in cosmic evolution, including biological evolution. Mathematics belongs in there somewhere as well; Kolmogorov, Hilbert, and all are here, though not Wigner, despite a closing statement of “metaphysical amazement: we have managed to decipher this much” which sounds (at least to me) a good deal like Eugene’s phrase about the unreasonable power of mathematics to describe the real world.

What is said about what we normally think of as astronomy? Not a great deal: ‘How does the Universe work?’ deals with the principle of least action, and ‘probability and astronomy’ with the Poincaré recurrence time (for planetary orbits). But the key point is that “our mathematicised sciences are nothing else but an attempt to decipher” what Einstein called “the Mind of God”, which Heller later calls “the Great Matrix of the Universe”. The chance here is not Pasteur’s, which favours the prepared mind, but the chance that made that mind possible in our finely- (divinely-)tuned Universe. You will need to be able to ignore slips like the claim that two nuclei of beryllium and one nucleus of helium must combine in order to produce carbon, plus (predictably) the misleading credit to Hoyle. I coped with that better than with the certainty that there must be some Higher Authority which is the source of all rationality, including the probabilistic aspects.

Publication of the book was supported by the John Templeton Foundation Grant ‘The Limits of Scientific Explanation’, and the author received the 2008 Templeton Prize for progress in spirituality. My copy was a gift from the author, inscribed “to Virginia in an unprobable place — Rzepiennik” where we both were for an event marking the dedication of the Queen Jadwiga Observatory there, and dated “8th June 2012”, most mysterious of all, because the event was on the 8th of June 2015. Well, “with G.d all things are possible”, though I think the author is more nearly saying “all things possible (however improbable) are with G.d”. — VIRGINIA TRIMBLE.

Stella Novae: Past and Future Decades (ASP Conference Series, Vol. 490), edited by P. A. Woudt & V. A. R. M. Ribeiro (Astronomical Society of the Pacific, San Francisco), 2014. Pp. 413, 23.5 × 15.5 cm. Price \$77 (about £50) (hardbound; ISBN 978 1 58381 864 0).

Classical and recurrent novae are objects with a long and rich history of observation stretching back into antiquity. They are of great interest to both professional and amateur astronomers: as well as being bright transient events, often accessible with small telescopes or even the naked eye, they are of fundamental importance as (for example) potential progenitors of supernovae type Ia. However, meetings dedicated to the subject are somewhat infrequent, since nova results are often subsumed into larger meetings on white-dwarf studies in general. This book, therefore, being the proceedings of a novae conference held in Cape Town in 2013 February, covers a great deal of ground. The format of the book will be familiar to anyone who owns any other volumes in the Astronomical Society of the Pacific’s Conference Series, and it is well edited, very readable, and with clear figures. The papers presented are divided into twelve distinct chapters, covering specific theoretical or observational topics, for example, chapters dedicated to the extragalactic population, the detailed multi-dimensional modelling of the outbursts, and observational studies over a variety of different electromagnetic regimes, from radio to gamma-rays. Many of the

chapters begin with a review of the topic, hence this volume is an excellent resource for a newcomer to the field. In particular, the opening review by Shara, the overview on the extragalactic population by Shafter, and the discussion of the spectroscopic development of a nova by Shore together provide an excellent summary of our understanding of these objects as it stands in the early part of this decade. Regarding the new results presented in this volume, while I don't have the space to detail each one of many interesting papers, one personal highlight was the numerous discussions of the recurrent nova T Pyxidis, which is a particularly interesting object in the context of the broader question of the period distribution of cataclysmic variable stars (the progenitors of classical and recurrent novae). I also found the chapter on the modelling of the outbursts to be most interesting: it is clear that the past decade has seen a great deal of advancement in our theoretical understanding, and the results in this chapter were clearly presented and comprehensible even to this humble observer.

What is most striking from this volume, however, is the effect of recent increases in our observational capability, with robotic telescopes providing unprecedented sampling of the temporal evolution of the nova light, and observations from space- and ground-based facilities providing additional key elucidation. This volume also makes clear that the future outlook for the field is bright from an observational perspective. The next decade will see an amazing increase in our wide-field-survey capacity, which means a huge increase in the rate of detection of explosive transients of all types, including novae. The extragalactic papers in this volume, in particular the work on the M 31 population, show the value of more complete samples. The next meeting in this series promises to be most interesting indeed! — CHRIS COPPERWHEAT.

Ecology of Blue Straggler Stars, edited by H. M. J. Boffin, G. Carraro & G. Beccari (Springer, Heidelberg), 2015. Pp. 349, 24 × 16 cm. Price £117/\$179 (hardbound; ISBN 978 3 662 44433 7).

It is as refreshing as it is still rare to find not the hashed-together Proceedings of a conference, but organized information distilled from it, presented here by 21 selected authors. The Springer ASSL Series is encouraging such undertakings — a development heartily to be welcomed; despite the heavier workload for both authors and editors, the product is far more useful to the research world than a set of disconnected contributions. (The latter may constitute a 'publication' for an individual author to chalk up, but most such contributions find their way into the peer-reviewed journals, if not there already.)

Blue straggler stars (BSS), which appear blue-ward of the cluster turn-off point, have been a headache since their existence was first admitted as more than just a case of mistaken identity in a colour-magnitude diagram. Through improved imaging capabilities they are now found in all regions of both open and globular clusters and even dwarf galaxies, thus offering a broad range of environments and conditions for their formation. This first attempt to straighten out in 14 chapters the queries inherited from puzzled observers covers the whole gamut, from observational evidence of all kinds and wavelengths to the most plausible (and even implausible) theories and models for their formation. As such, it is a valuable resource for every hot-star researcher. Special mention must be made of G. W. Preston's Chapter 4, which is masterful, enlightening, honest, blunt, and tinted deliciously with whimsical humour.

On the negative side, the editors did not do their job as conclusively as Springer would surely have liked. The general concept behind the Series is

that the information be presented as if from the keyboard of a single author. While some — possibly much — effort may indeed have gone into achieving that objective, one still finds later chapters sketching the discovery of blue straggler stars when Chapter 2 has already specifically described all that history. Acronyms (of which there are rather too many) are re-defined too many times (even ‘BSS’ is re-defined in chapters as late as 5, 6, 11, and 14), and some notation is inconsistent; though trivial in relation to the importance of the book, such inconsistencies convey the slightly unfortunate impression that here is a group of authors each having their own say, rather than a uniform distillation of the information which they have supplied. Split infinitives abound, along with a few other misuses of the English language, but again they are not fatal flaws — one just wishes that there had been an additional proof-reading stage. More seriously, there is repetition and circularity in the way the chapters present topics, and most have a stab at describing the three current possible origins of BSS. It might have been more logical to take the reader sequentially through observing to interpreting to modelling, rather than creating what is little more than a Proceedings after all — again, an editorial shortcoming. The chapters do not have abstracts, which, in view of the circularity just alluded to, were rather missed. Making Chapter 1 a general explanation of stellar evolution without any mention of BSS seems ill-advised; it should have been placed in an Appendix.

Those relatively small niggles aside, the book is definitely to be welcomed as a much-needed contribution to the study of stellar physics. It should be in every departmental library. — ELIZABETH GRIFFIN.

Asteroseismology of Stellar Populations in the Milky Way, edited by A. Miglio, P. Eggenberger, L. Girardi & J. Montalbán (Springer, Heidelberg), 2015. Pp. 185, 24 × 16 cm. Price £126/\$199 (hardbound; ISBN 978 3 319 10992 3).

Asteroseismological calibration of theoretical stellar models is rapidly becoming a major tool for studying stellar populations. As was anticipated long ago, coupling empirically derived seismic scaling relations with more traditional astronomical data can improve greatly our ability to estimate some of the parameters characterizing the global properties of stars, such as mass, surface gravity, metallicity, and age. This book is the proceedings of a workshop that was held at the Sexten Centre for Astrophysics, Bolzano, in 2013 July, to assess the current impact of the new tool, the improvements that it has made, and the further improvements that it is likely to make. Many of the contributions are brief reviews of specific activities. On the whole they avoid detail of either the procedures that are being adopted for drawing inferences from the combined data, or the physics that it is hoped to be learned from them — it appears to have been presumed that that was known already by the participants. But instead they provide a valuable compendium of the most important activities in, or about to be in, progress, together with the results that are being obtained from them, however preliminary. Some emphasis is given to how asteroseismological inference is already causing some adjustment to the parameter estimates, and to (in some cases evidently over-optimistic) opinions of what the uncertainties in those estimates are. Additionally, there are useful purely astronomical contributions that ignore the asteroseismological information entirely. There are descriptions of new and intended future collaborative projects, and what they are designed to achieve. The volume of literature in population analysis is vast; therefore the reviews are extremely useful for guiding scientists interested in

only the broader aspects of the subject through areas about which they might be particularly curious. Some of them also indicate where more careful analysis is urgently needed. — DOUGLAS GOUGH.

Galactic and Intergalactic Magnetic Fields, by U. Klein & A. Fletcher (Springer, Heidelberg), 2015. Pp. 236, 24 × 16 cm. Price £90/\$129 (hardbound; ISBN 978 3 319 08941 6).

Doesn't the very title of this volume set your mind yearning for the steak-like taste of the processes that gave rise to these large-scale fields and the gravity-touch of their strength and extent when structure formation and the birth of Population III stars were getting started? Well, it did mine. But I had to settle for a take-out pizza of 'applications'. Perfectly serviceable and fit for purpose, but not the vision I had.

Chapter 9 gives us 3¼ pages on the origin of seed fields in Biermann batteries; mention of electro-weak, quark-hadron, and GUT eras but with no references, equations, or explanations; Harrison on eddies with the electron soup rotating faster than the ion soup; and the Weibel instability, again with reference only to a 13-year-old review.

Instead, the book, which came out of separate lecture courses taught by the authors at Bonn and Newcastle-upon-Tyne, begins with radiation processes and polarization diagnostics, and then moves on to molecular clouds, supernova remnants, galactic fields and dynamos, AGNs, clusters of galaxies, and the intergalactic medium. I wish very much that the authors had provided lots of homework problems, so that the reader could be sure of what she is supposed to have learned to calculate from each chapter.

History is confined to two pages, with credit for the discovery of interstellar polarization given exclusively to William Hiltner and no mention of the simultaneous paper by John Hall. They had started out to work together, but published separately in 1949 (I don't know why), and died 15 days apart in the fall of 1991. Credit for explaining the polarization is given to Davis and Greenstein (1951), though I think Thomas Gold's slightly later mechanism, which did not require dust grains to have particular magnetic properties, is closer to our modern understanding. I once asked Jesse (Greenstein) what had made him think of interstellar magnetic fields. His response was that it had probably been a talk by Enrico Fermi about magnetic acceleration of cosmic rays.

Klein & Fletcher have given us some glorious illustrations. Figure 4.3 is the Galactic Faraday (-rotation) sky. It looks surprisingly much like a pre-*WMAP* image of the CMB sky, colour-coded from +4, a dark orangey-brown, to -4, deep indigo blue. Unfortunately, we are not told +4 or -4 whats. Fig. 4.2 plots rotation measures of pulsars of known distance on the Galactic plane. The data are said to provide evidence that field azimuthal direction reverses sign between arm- and inter-arm annuli. I don't mean to question the conclusion, but the arms that are drawn in "to guide the eye" don't look much like the bifurcating, feathery ones seen in 21-cm radiation.

Figures 4.1 and 4.4 both compare total 1420-MHz flux with polarized flux, 4.1 on a scale of 1–10° and 4.4 over the whole sky (as visible from the *VLA*). Different in both cases, though somewhat puzzlingly. For the whole-sky map, polarization shows what seem to be parts of loops extending toward the poles that are not seen in the total flux (which, say the authors, reflects an "almost isotropic distribution of radio sources") (but the 1420-MHz continuum is not a sum of sources). In the close-up view, both total and polarized intensity show

lots of structure (amplitudes not explained), but it is the total flux map that has elongated features perpendicular to the Galactic plane (at Galactic longitude $190\text{--}210^\circ$, latitude $+4$ to $+14^\circ$).

Although the authors say they are addressing upper-level undergraduates and beginning graduate students, there is no doubt that this is difficult material, especially the chapter on galactic dynamos. The units are (largely) cgs, so I can say with reasonable confidence that the various analyses lead to magnetic fields for galaxies that are the ones we grew up with (a few μG) and brightnesses of things in reasonable numbers of ergs s^{-1} . But rotation measures of course come in rad m^{-2} , because the world's first radio astronomers were mostly once radar engineers, and their first observations were mostly long-wavelength ones. — VIRGINIA TRIMBLE.

Spectroscopic Instrumentation: Fundamentals and Guidelines for Astronomers, by T. Eversberg & K. Vollmann (Springer, Heidelberg), 2015. Pp. 653, 24×16 cm. Price £72/\$99 (hardbound; ISBN 978 3 662 44534 1).

If a picture is worth a thousand words, then what is the value of a spectrum? From the earliest attempts at astronomical spectroscopy in the first decades of the 19th Century to the present day, spectroscopic techniques have become a fundamental part of the astronomer's tool-kit. We use spectra to determine the physical parameters of astronomical objects, study their space motions, and measure distances. There have been many triumphs, including the measurement of the distance-scale of the Universe through spectroscopic redshift surveys and detection of the presence of extra-solar planets orbiting other stars with high-precision radial-velocity measurements. As new telescopes are developed, increasing demands are placed on the accompanying spectroscopic instrumentation, whether it is stability, spectral resolution, wavelength coverage, or throughput. Therefore, a comprehensive, up-to-date manual for potential designers and users of spectroscopic instrumentation is long overdue.

The immediate question to answer is can this book answer that need? It certainly does. It is a comprehensive volume that includes all the fundamental optics concerned with the components of spectrographs and their construction. It also relates this material to examples of real instruments that the authors have built. It is strongly rooted in their personal experience, as can be gathered from the slightly quirky, but informative, anecdotes that introduce each chapter. This is a nice touch, although the relevance of some stories is not always obvious. But there are some minor disadvantages in that approach. Most of the book concentrates on the visible spectral range with limited reference to the techniques applied outside it, in the IR, UV, or X-ray régimes. While the fundamentals are similar, there are some special problems associated with those other wavebands that I would like to have seen addressed for the volume to be fully comprehensive. The examples are also firmly anchored to ground-based facilities, where mass and space constraints are not nearly as demanding as for space-based applications. Some consideration of those issues would have been useful.

Nevertheless, those minor niggles aside, this is a text that will readily find a place on my bookshelf. It is a volume to which I expect to refer frequently, particularly in my efforts to develop spectrographs for future space missions. A few years ago, I also planned to write a book on astronomical spectroscopy, but eventually had to shelve the project due to lack of spare time. I am pleased to report that these authors have done an excellent job of filling the gap I identified at the time and that I need not revive my own project. — MARTIN BARSTOW.

High-Resolution Imaging: Detectors and Applications, by Swapan K. Saha (Pan Stanford Publishing, Singapore), 2015. Pp. 565, 23 × 15 cm. \$149.95 (about £98) (hardbound; ISBN 978 981 4613 27 9).

Author S. K. Saha has previously written on *Diffraction-Limited Imaging with Large and Moderate Telescopes* (2007) and *Aperture Synthesis: Methods and Applications to Optical Astronomy* (2010). The present volume focusses on modern detecting systems, technologies, and designs, evaluation and calibration, and control electronics, and touches on scientific applications and results, mostly astronomical and mostly in the infrared. Well-known experts in these territories include Antoine Labeyrie and the late Robert Hanbury Brown (both mentioned in the preface and multiply cited in the very extensive and careful bibliography, though as always I would have liked 'back references' from the papers and books cited to the main text).

The extreme difference from a standard astronomy book becomes clear when you notice that CCDs, photon-counting devices, MerCadTel-based detectors, non-linear heterodynes, and superconducting tunnel junctions are in the main text, but H II regions and active galactic nuclei are explained in footnotes. Something like five pages each of definitions of acronyms and symbols cover most of what is needed, but one has to flip madly between the values of physical and astronomical constants on page xxvii and the tables of radiometric and photometric units on p. 533 to figure out that lx (Lux) = lm m⁻² is another unit, not the measured value of any property of any source. lm is lumen but R_L (load resistance) has no definition or numerical value at all. I spent quite a while admiring Table A.3, the Zernike polynomials for various optical aberrations. Spherical is $j = 11, n = 4, m = 0$ (the zero m I understand!). Comas are $m = 1$ and astigmatism $m = 2$, which also make sense even to bears of very little brain.

None of the images are in colour, but Fig. 4.3 of a normal retina and one with cystoid macular oedema with retinal thickening (*etc.*) is quite gory enough in black and white, as one sits there wondering which is more like one's own. The point of the figure is actually that the images come from optical coherence tomography and a complementary metallic-oxide semiconductor (CMOS) camera.

I predict that the volume will be a valuable resource for astronomers and students involved in the design of modern instrumentation or attempting to make use of data with instrumentation that they did not design. Indeed, I so predicted on the back cover (conflict-of-interest statement). Prof. Labeyrie of the College de France adds amateur astronomers interested in high-resolution imaging to the user pool (also on the back cover). — VIRGINIA TRIMBLE.

Hubble's Legacy, edited by R. D. Launius & D. H. DeVorkin (Smithsonian Books, Washington), 2015. Pp. 224, 23 × 15 cm. Price \$24.95 (about £16) (paperback; ISBN 978 1 935623 74 8).

Hubble's Legacy appeared just in time to coincide with the 25th anniversary of the telescope's launch in 1990 April. Over those 25 years, the *Hubble Space Telescope* has not only produced iconic images and ground-breaking science — it has been at the centre of a compelling tale of endeavour, perseverance, and overwhelming success snatched from the jaws of catastrophic failure. *Hubble's Legacy* tells that story from the viewpoint of the scientists, engineers, astronauts, artists, and curators who have been closely involved at different stages throughout the telescope's lifetime.

The book is based on lectures given at a public symposium in 2009 to celebrate the opening of a new exhibit at the National Air and Space Museum (NASM) in Washington, DC. The story takes us from the early concept of a large optical space telescope, through the launch and subsequent discovery of *Hubble's* spherical aberration, to the servicing missions that not only fixed it but also allowed *HST's* lifetime to be extended and its capabilities enlarged. The story is told from a range of viewpoints, giving proper consideration to the technological, scientific, and political aspects.

This is not a book about the science of *Hubble*, although it gives due acknowledgement to key breakthroughs such as the Hubble Deep Field images. Instead, it provides a fascinating insight into how *HST* fitted into NASA's grand plans, especially in its relationship to the Shuttle programme. Because it is told as a first-person narrative, it also brilliantly evokes the shock of discovering the telescope's eyesight problems, and the enormous suspense when waiting to see if the first servicing mission had been a success. A highlight is the account given by Dr. John Grunsfeld, a veteran of three *Hubble* servicing missions, whose obvious devotion to the project shines clearly through in his writing.

Perhaps inevitably, I found some essays harder to engage with than others, mainly due to my personal interests rather than the quality of writing. For example, I found the history of *Hubble* itself more interesting than the history of the *HST* exhibit at NASM. However, the breadth of the book is undoubtedly a strength, and I hope it will find a readership among historians as well as scientists. I was left with an overwhelming impression of how truly audacious the *Hubble* project has been — and a real sense of privilege that I have been able to use it in my own work. — JO BARSTOW.

À la rencontre des comètes: de Halley à Rosetta, by J. Lequeux & T. Encrenaz (Belin, Paris), 2015. Pp. 144, 24.5 × 18.5 cm. Price €22.90 (about £16) (paperback; ISBN 978 2 7011 9206 2).

As with *L'exploration des planètes*, in the same series and by the same authors, which I reviewed last year in these pages (134, 294), this work is aimed at interested amateur astronomers and members of the general public. As such it gives a good summary of our knowledge of comets from earliest times to the initial results on 67P/Churyumov–Gerasimenko, even including discussion of the significance of the D/H ratio in 67P. As well as the few images from *Philae*, it incorporates an eight-page section of the striking images returned by the *Rosetta* orbiter, including the coloured map of the comet's surface areas. The last chapter on exoplanets and their exocomets is particularly valuable. There are appendices on cometary nomenclature and another detailing all the instruments on both *Rosetta* and *Philae*. Overall, the coverage is excellent, although I think I would have liked rather more discussion of the differences between the compositions of minor planets and comets and their significance.

Once again, the French viewpoint is interesting in being slightly different from that given by most English-language books, especially as both authors (and Encrenaz in particular) have been involved with various space missions to comets, ranging from *Giotto* to *Rosetta*. Until reading this book, I have to admit my ignorance of the *Odin* satellite (a Finland/Canada/France collaboration) and its contribution to cometary studies, as well as the work on exocomets, and not only how thousands of these have been discovered, but also how they fall into two distinct populations.

Unfortunately, there are a few moderately significant instances of errors

and problems. There is some confusion over the comets that Edmond Halley identified as having similar orbits. In the box on p. 16, the correct returns (1531, 1607, and 1682) are listed. Yet on p. 47, comets of 1305, 1380, and 1456 are added. The date of 1305 is incorrect for Comet 1P/Halley, which had a return in 1301. Halley himself suspected that the apparitions of 1305, 1380, and 1456 could be the same object, but we now know that the comets of 1305 and 1380 were non-periodic. However, 1P/Halley's correct date of 1301 is mentioned on p. 47 in connection with Giotto's painting *The Adoration of the Magi*, and in the caption on p. 48.

On p. 95 there is a figure showing, to scale, all the minor planets and comets for which close-up images have been obtained to date. Although the idea is excellent, there are two problems: minor planet Itokawa (0.5-km maximum diameter) appears as a tiny white dot — looking like a printing flaw — against Mathilde (66 km), printed 55 mm across. Similarly, although the idea of showing the images in their true albedo is fine, the comets, in particular, are so dark that they are almost impossible to see against the black background. The faint smudge that is the tiny image of 103P/Hartley 2 may be glimpsed only under certain lights after struggling to find the correct angle at which to tilt the page.

There are a few minor errors: a misprint of 1890 instead of 1980 for the date of a *Voyager* mission (p.80); an incomplete formula for ethylene glycol in the table on p.88, and a few other inconsistencies — none of which are particularly significant.

The bibliography gives works in French, English, and sources on the Internet. The cometary works are, perhaps, rather technical for most of the intended readership, because some books, such as Don Yeoman's *Comets: A Chronological History* (Wiley, New York, 1991) and *Fire in the Sky*, by Olson and Pasachoff (Cambridge University Press, 1998) have been omitted. All in all, however, a useful summary of current knowledge of comets with good indications of how (we hope) our understanding of these objects in both the Solar System and beyond is likely to progress in years to come. — STORM DUNLOP.

Celestial Shadows: Eclipses, Transits, and Occultations, by J. Westfall & W. Sheehan (Springer, Heidelberg), 2015. Pp. 713, 24 × 16 cm. Price £117/\$179 (hardbound; ISBN 978 1 4939 1534 7).

"We were now within a sunless cone of negativity that stretched from us to the silhouetted edge of the Moon", write authors Westfall and Sheehan of their experience in the dusky corridor of the 1991 July total eclipse in Baja California. "We fell under a combination of surprise, awe, and a little fear." It's that visceral reaction to an otherwise precisely predictable event, as much as the visual splendour, that draws *aficionados* to total solar eclipses around the world.

In the 700-plus pages of this monumental volume, Westfall & Sheehan investigate not just eclipses but also their shadowy relatives, transits and occultations (pedantic note: an eclipse of the Sun is strictly an occultation, of course, but it's too late to change the name now). Eclipsing stars, the hide-and-seek of planetary satellites, and transiting exoplanets all fall into their net, and they aim to entice amateurs into making useful observations of them.

There are some interesting factual surprises. Did you realize, for example, that the Moon gets slightly *brighter* as it enters the Earth's penumbra at a lunar eclipse, rather than darker, because the 'opposition surge' outweighs the slight dimming caused by the insipid shadow? The first coronal mass ejection was observed at a solar eclipse in 1860, although not recognized for what it was for

well over a century — at that time, it was not even certain whether the corona belonged to the Sun or the Moon.

That CME, incidentally, occurred less than a year after the famous Carrington event of 1859, near the peak of solar cycle 10. During the second half of the 19th Century, with the development of international travel, eclipse chasing became a popular sport with astronomers. In 1896 the BAA chartered a ship to observe an eclipse in northern Norway, the forerunner of the modern ship-borne eclipse expeditions.

Celestial Shadows incorporates the authors' earlier book on transits of Venus, (see p. 406 of the 2004 October issue of *The Observatory* for a review), updated with reports on the 2004 and 2012 events. (One slip in that section that could create confusion: Jeremiah Dixon is rechristened James in at least two places.) That book was itself substantial. As a result, there is more about transits in this current volume than eclipses, leaving a certain imbalance. For example, I would have liked to see more about the value of solar eclipses in tracking the Earth's changing rotation, which rates less than a page.

The authors enjoy word play as well as shadow play: "Without intending to be obscurantist, we readily admit that we must clothe in the obscurity of language our descriptions of obscurations", they write. Some might find their verbosity in these closely typeset pages to be wearying. Those who plough through the verbiage, though, will come away with a new appreciation of the science behind these shadowy events and the value of observing them. — IAN RIDPATH.

Camille Flammarion's The Planet Mars, as translated by Patrick Moore, edited by William Sheehan (Springer, Heidelberg), 2015, Pp. 528, 24 × 16 cm. Price £117/\$179 (hardbound; ISBN 978 3 319 09640 7).

Sometime in Paris at the start of World War II, a young Romanian student named Jean Dragescu picked up for 125 francs a copy of Camille Flammarion's epic work, *La Planète Mars et ses conditions d'habitabilité*. In 1992 Jean Dragescu (by now a naturalized Frenchman) kindly presented it to me. I have always loved this book, for it reproduces, in a single volume, all the noteworthy observations of Mars from the earliest times up to the year 1890. But what about an English edition?

Within a few years of translating the Mercury and Mars books of E. M. Antoniadi from French into English, Patrick Moore turned his attention to this book and a companion volume. A born publicist, Flammarion (1842–1925) had chosen the years of two favourable perihelic oppositions for those volumes to appear in print: 1892 and 1909. Flammarion basically discussed all the available documents, and that has made the work the first point of call for later historians interested in who discovered what and when. The second volume advanced the story by a decade only, but covered the period when the majority of Schiaparelli's *Memoria* had appeared in print.

Patrick once told me that much of his translation had been done while waiting at airports for flights, or when travelling. But the notes were faithfully re-typed on his trusty Woodstock 1908 typewriter with three sheets of carbon paper. These four precious copies were then distributed to libraries such as the RAS, while Patrick searched for a publisher; twenty years ago he kindly allowed me to make a fifth copy for myself. Formal publication was not as easy as with the Antoniadi books. Flammarion's works had been more profusely illustrated, and in the pre-desktop-publishing days of the early 1980s the reproduction of all the figures would have been prohibitively expensive. After an abortive attempt

by the late P. M. E. Erwood, Patrick gave one copy of the typescript and the original French books to Bill Sheehan to seek a publisher overseas. It is good to record that Sheehan ultimately found Springer willing to undertake publication. The book now reviewed is volume 1 of Flammarion's classic. (There are still only five copies of the English translation of volume 2!) The quality of reproduction is excellent, and both editor and publisher must be commended for their achievement. Sheehan had a considerable amount of editorial work to perform, and his introduction contains a well-balanced sketch of Flammarion's life, recalling that it was Flammarion's book that had catalyzed Lowell's life-long infatuation with the Red Planet.

Flammarion was a great believer in Fontenelle's view of '*La vie Universelle*', and he was entirely at ease with the idea of Martian life. Thus he was never happier than when reviewing Schiaparelli and the work of those who could confirm the mysterious *canali*. But the work is entirely objective, for the original observations are reproduced so that one can draw one's own conclusions. Many obscure sources appear in Flammarion which might otherwise have been lost to history. The work includes drawings by famous names such as Cassini, Bianchini, Arago, Beer & Mädler, Lockyer, Dawes, Secchi, and the earlier results from Schiaparelli. Flammarion divided the text into various epochs. The first period covers 1636 to 1830, the next, 1830 to 1877, and the third, 1877 to 1892. Thus the second period coincides with the first maps of the planet, which would later serve to demonstrate that, unlike the Moon, obvious changes were often occurring on the surface. Flammarion used only published sources: the book already being large (the original runs to 608 pages), he did not look for unpublished manuscripts. Of course, all the data are visual: planetary photography was in its youthful phase in 1890. Flammarion wrote a good summary about our knowledge of the planet at the time as a conclusion to the work.

Flammarion is sometimes said to have prepared a third volume in manuscript, but in fact he never got further than arranging relevant Mars publications in folders by apparition. I enquired years ago, and discovered that there is no such manuscript in the archives at Juvisy. In any case, he would have been troubled in reviewing the apparition of 1909, for in that year his former pupil, Antoniadi, had effectively eliminated the Martian canal network through his superb observations from Meudon. And by the end of World War I, when he might realistically have considered bringing out another volume, Flammarion's writing had turned to spiritualism and allied subjects.

One can occasionally find the original French volumes for sale at high prices, but I suspect that many would-be readers of Flammarion will find this English translation much more convenient. I have no hesitation in recommending it wholeheartedly. — RICHARD MCKIM.

Einstein's Dice and Schrödinger's Cat: How Two Great Minds Battled Quantum Randomness to Create a Unified Theory of Physics, by Paul Halpern (Basic Books, New York), 2015. Pp. 271, 23.5 × 15 cm. Price \$27.99 (about £18) (hardbound: ISBN 978 0 465 07571 3).

As you undoubtedly already know, the subjects of this book "battled to create a unified theory of physics, and failed". Readers best able to appreciate the volume will probably also have heard of Einstein's dice (which his 'Old One' first did not play with in a 1926 December 4 letter to Max Born) and Schrödinger's cat (which he described in a 1935 August 19 letter to Einstein, acknowledging

input from a less-well-known Einstein powder-keg *gedanken* experiment).

Is this an astronomy book? Obviously not. But it has some important, little-known details about the 1914 August eclipse of the Sun and Einstein's involvement in the expedition that was supposed to look for gravitational bending of light from background stars, but ended up with the observers as prisoners instead. There is also a brief discussion of cosmology, including Einstein's static model, the de Sitter solution, credit to Hubble (but no mention of Slipher's radial velocities) for establishing an expanding Universe, and most surprising, mention of papers by Schrödinger presenting solutions to the field equations of General Relativity that included our friend Λ , regarded by the author as an anticipation of dark energy! Einstein did not much like what he called Schrödinger's "non-observable negative density in interstellar space". And our Λ is, of course, a negative-pressure substance. "Interstellar" presumably means that Einstein was, at the time, sure that the Milky Way was the Universe.

Halpern's book has an enormous richness of detail about both men's lives and work, which continued (mostly unappreciated by the rest of science, which had moved on) essentially up until their deaths. All biographies have sad endings, this one perhaps more than most (no, it isn't *just* a biography of Schrödinger and Einstein!), but please keep reading your copy right up to page 235, where there is a good joke and a "mystery guest". The joke comes from the brief flourish of superluminal neutrinos: "We don't allow faster than light neutrinos in here, said the bartender. A neutrino walks into a bar.", and the mystery guest is Emily Litella, who is apparently known (though not to me) for saying "never mind".

Conflict-of-interest statement: my copy of *'Dice and Cat'* was a signed present from the author, who is considerably better looking than the picture on the back jacket flap. — VIRGINIA TRIMBLE.

The Atmosphere and Ionosphere, edited by V. L. Bychkov, G. V. Golubkov & A. I. Nikitin (Springer, Heidelberg), 2014. Pp. 362, 24 × 16 cm. Price £175/\$269 (hardbound; ISBN 978 90 481 3211 9).

The full title of this book, if the sub-title is included, is *The Atmosphere and Ionosphere. Elementary Processes, Monitoring, and Ball Lightning*. The author of this review volunteered to write it, despite its being outside the range of his supposed competence, on account of his curiosity over ball lightning, which is a phenomenon that has been reported often enough to make the reality of its existence pretty convincing yet one that seems to defy rational explanation and to have eluded convincing photography. The potential interest of the section on ball lightning promised to outweigh the bother of reviewing also the other sections, which being specifically characterized as "elementary" seemed likely to be written at a level compatible with the reviewer's intellect even if outside any specific expertise that he might be thought to possess.

The sub-title turns out to be entirely misleading as far as *Elementary* is concerned. The book has five chapters, of which only one is about ball lightning. The others are largely over the reviewer's head, and do not by any means match the impression given by the *Elementary* in the sub-title. The first chapter (68 pages) is entitled 'Optical quantum properties of GPS signal propagation medium-D layer', and a flavour of its content is offered by the excerpt reproduced here (Fig. 1), which is a great deal more informative as to the nature and level of the writing than any comment that the reviewer would have wit enough to make. The next two chapters are 'Prebreakdown air ionization in

$$\begin{aligned}
 r_{LL_z \rightarrow L'L'_z}(R) &= \left\langle \Psi_{nm}^{(L,L_z)}(\mathbf{r}, R) | \mathbf{r} | \Psi_{nm'}^{(L',L'_z)}(\mathbf{r}, R) \right\rangle \\
 &= \sqrt{\frac{4\pi}{3}} \sum_{p=0, \pm 1} \mathbf{e}_p \sum_{l \geq l_*}^{n-1} A_{\nu_L, lm}^{(L,L_z)}(R) \left[A_{\nu_{L'}, l+1m'}^{(L',L'_z)}(R) r_{\nu_L l, \nu_{L'} l+1} S_{mm'}^{ll+1}(p) \right. \\
 &\quad \left. + A_{\nu_{L'}, l-1m'}^{(L',L'_z)}(R) r_{\nu_L l, \nu_{L'} l-1} S_{mm'}^{ll-1}(p) \right], \quad (1.52)
 \end{aligned}$$

$$\begin{aligned}
 r_{LL_z \rightarrow nlm}(R) &= \left\langle \Psi_{nm}^{(L,L_z)}(\mathbf{r}, R) | \mathbf{r} | \Psi_{nlm}^{(c)}(\mathbf{r}) \right\rangle \\
 &= \sqrt{\frac{4\pi}{3}} \sum_{p=0, \pm 1} \mathbf{e}_p \left[A_{\nu_L l-1m}^{(L,L_z)}(R) r_{\nu_L l-1, nl} S_{mm}^{l-1l}(p) + \right. \\
 &\quad \left. + A_{\nu_L l+1m}^{(L,L_z)}(R) r_{\nu_L l+1, nl} S_{mm}^{l+1l}(p) \right]. \quad (1.53)
 \end{aligned}$$

FIG. 1

An example of this elementary text.

the atmosphere' and 'Current trends in the development of remote methods of detecting radioactive and highly toxic substances'. They too give the impression of being written at a high technical level; the latter chapter runs to 88 pages, of which the last 13 are occupied by a bibliography printed in small type.

It does not help the reviewer that the book is written in 'Russian American'. Its three editors are all Russian and all of them are authors or co-authors of one or more chapters (one of them, Bychkov, of three). Moreover, in a statement opposite the title page, the volume is described as being one of a series *Physics of the Earth and Space Environments*, which has three 'Series Editors', whose names and/or addresses suggest that two are Italian and one is German; and the publisher is Springer, which gives a number of cities in different countries as its address (though without such details as postal addresses such as would enable one actually to find it in any of them). The reviewer's effort to establish from the Web just where the company is actually based was quickly mired in ambiguity and even seemingly deliberate obfuscation*, but at the risk of divulging an erroneous opinion the writer of this review believes that the company is actually German. Indeed Springer is the German word for the knight in a chess set — an apt analogy in view of the dodgy way in which efforts to track it to source are systematically foiled! Far be it from the reviewer to appear as a xenophobe, but he cannot resist saying (and it would not be fair to readers of this review if he did) that the apparent objective of putting the language into good American, if indeed that were really considered desirable and were not itself an oxymoron, might have been better served by an editorial syndicate whose membership included educated Americans.

*Try Googling 'Know your Springer', and you get a somewhat honest page with a headline, "Do you mean Springer, Springer or Springer?", and then a second-level headline, "What do we share with Merck and GE? The fact that our company name causes confusion."!

The long chapter on ball lightning gives 48 numbered anecdotes referring to the effects of such ‘lightning’ on people and 52 of events in aeroplanes (“airplanes”). (The *Preface* misinforms us that the numbers of those anecdotes are 46 and 50, respectively.) Many of the anecdotes are decades old (I noticed one from 1934 but it may not be the oldest) and some are difficult to understand. They refer to such things as “fiery balls”, which often seem to be of football size, but in different anecdotes dimensions from 3 cm to metres are mentioned. Each item is followed by a paragraph labelled **Comment**, presumably editorial. Often the comments are merely summaries of the anecdotes themselves, but in other cases they refer to things that are not mentioned in the anecdotes and appear to be products of editorial imagination. They are apt to refer to explosions, and to electromagnetic pulses, and high-frequency radio emission (which in one instance is said to have evaporated a ring from a girl’s finger, without, however, damaging the finger), and other properties, for which not so much as a scrap of evidence is apparent. Indeed, in some cases there is absolutely no relationship between the anecdote and the comment at all. Readers may find the following example of a complete anecdote and comment so remarkable as to warrant forgiveness of the apparent waste of space by the inclusion of a *verbatim* quotation from the book (in which it occurs on p. 211) in this review. The first two lines of the anecdote also illustrate the sort of difficulty sometimes encountered with Russian English.

“In July 1952, during severe rain and a thunderstorm with a strong thunderclap, a sound like the discharged cartridge of an electric bulb rang out; a ball 5–6 cm in diameter jumped out of the bulb. It fell on the head of a man who had entered the room and blew up in his hair. The man lost consciousness. As [*sic*] this moment, linear lightning struck wires outside. The man remained alive; however, he had pain in his head that lasted 2 weeks.

Comment. The case of BL’s origination on a transmission line and exploding at a strike of linear lightning is described. The victim was rescued, apparently, by his clothes, which had become wet during the rain over [*sic*; shower?]; most of the BL charge passed over his wet clothing.”

In one case a ball (size not mentioned) was reported to have struck and killed a boy and blown his shirt off him. The comment is, “The electrostatic field of this charge appeared to be strong enough to create a force that tore the shirt off the boy. The boy died from overheating of his internal organs due to the powerful electromagnetic radiation pulse that was released by the BL at its explosion.” It seems necessary to suspend one’s normal scientific judgement to believe that there could be enough power in a small ball of something-or-other that could float through the air, to tear clothing off a person and fry his internal organs. In no case is there any actual evidence of electrostatic fields or of any radiation pulse; powers of many kilowatts (sometimes 20, in one case 100) are ascribed, albeit over short durations, to the activities of the floating balls.

In another case, “a bright yellow ball the size of a tennis ball” appeared in a tent shared by five mountaineers. Seemingly systematically, it visited them in their respective sleeping bags, leaving holes in the bags and in clothing. “When the fiery ball penetrated my bag, I felt a fierce pain as if I had been burnt by a welding iron, and I lost consciousness. Sometime later, having regained consciousness, I saw the same yellow ball, which almost methodically, as if in a known sequence, penetrated each sleeping bag, and with each such visit caused

a desperate human shout. This action was repeated several times. . . When I came to my senses again, apparently for the fifth or sixth time, the “devil ball” was not in the tent. . .” One of the party was killed; the others were rescued by helicopter and taken to hospital, where the man who reported on the incident was found to have seven wounds which “were not burns; rather, it appeared as if muscle tissue had been pulled off from my bones.” It beggars belief that a small ball of something that could float through the air would somehow possess the power and the seemingly deliberate autonomy, while itself remaining intact, to burn its way successively into several sleeping bags, several times each, and seriously harm (and in one case kill) the occupants. The account also includes the statement, “The duration of the BL’s presence in the tent was nearly 10 s.” The yellow ball would surely have had its work cut out to visit several sleeping bags several times each in less than ten seconds, and so would the narrator, in that short time, to have lost consciousness, regained it “sometime later”, and have that whole cycle repeated several times.

I have dwelt at some length on the book’s chapter on ball lightning, which I feel competent to read and criticize. There is one other, relatively short but otherwise somewhat analogous chapter, written by the editor Bychkov, on *Atmospheric Gelatinous Meteors*, which is equally easy to read and equally difficult to accept as a piece of scientific investigation, although both the ball-lightning and the gelatinous-meteor chapters conclude with scientific-looking but inconclusive discussions with plenty of algebraic formulae. I cannot offer an expert opinion on the first three chapters, but if the two that I can read with seeming comprehension and interest are anything to go by, the others, too, may be dressed up with elaborate algebra and discussion without correspondingly advancing their subjects. And the price of the book, at £175 (almost 50p a page), is altogether outrageous. — R. F. GRIFFIN.

OTHER BOOKS RECEIVED

Coronal Magnetometry, edited by S. Tomczyk, J. Zhang & T. Bastian (Springer, Heidelberg), 2014. Pp. 218, 24 × 16 cm. Price £90/\$129 (hardbound; ISBN 978 1 4939 2037 2).

This volume is reprinted from *Solar Physics* and collects the 12 papers presented in Boulder in 2012 May at the ‘Workshop on Coronal Magnetism — Connecting Models to Data and the Corona to the Earth’, together with a short Preface.

Here and There

THREE STRIKES AND YOU’RE OUT

299,792 km travelled [in a second] by a photon in a vacuum; 200,000 km travelled by the Solar System in orbit around the Galactic Centre; 29,800 km travelled by Earth in orbit around the Sun; 7700 km travelled by the *International Space Station* in orbit around the Earth; 16.26 km travelled by *New Horizons*, the fastest spacecraft ever, now arriving at Pluto — *New Scientist*, 2015 June 27, p. 31.

NOW JUST WAIT A SECOND!

Tidal friction causes the Earth’s rotation to slow down, which means that atomic clocks fall behind. — *Daily Telegraph*, 2015 June 30, 3rd leading article.