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COOL DWARFS IN WIDE MULTIPLE SYSTEMS

PAPER 1: TWO MID-M DWARFS IN A LOOSELY-BOUND COMMON-PROPER-MOTION PAIR

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This is the first of a series of works devoted to investigating cool dwarfs in wide multiple systems. Here, I present Koenigstuhl 4 A and B, two bright, intermediate M dwarfs with a common high proper-motion and separated by 299 arcsec. At the most probable distance of the system, 19 pc, the projected physical separation is 5700 AU, which makes Koenigstuhl 4 AB to be one of the least-bound binary systems with late-type components found to date. I also associate the primary with a *ROSAT* X-ray source for the first time.

Introduction to the series 'Cool dwarfs in wide multiple systems'

M-type dwarfs are the most abundant stars in the Universe. They provide us with information on a wide range of topics, from formation of dust in their cool atmospheres ($T_{\text{eff}} \sim 4000\text{--}2200\text{ K}$)^{1–3}, through magnetic activity originating in their completely convective interiors^{4–6}, and the shape of the mass function down to the sub-stellar boundary^{7–9}, to possible existence in habitable zones of exoplanets that can be detected with current technology^{10–12}. Only a few hundred cooler objects, with spectral types L ($T_{\text{eff}} \sim 2200\text{--}1300\text{ K}$) and T ($T_{\text{eff}} \sim 1300\text{--}700\text{ K}$)^{13–15}, have been catalogued to date. However, the characterization of such late-type stars and brown dwarfs is in general difficult because of their intrinsic faintness, especially at visible wavelengths.

Many known cool and ultracool (spectral type later than M5 V) objects are components of resolved physical systems^{16–18}. When they are proper-motion companions to bright solar-like stars, their characterization is enormously eased: the heliocentric distance may be known (from *Hipparcos* parallax measurements), as well as their metallicities (which are very difficult to study in cool dwarfs)^{19–21} and radial velocities (from which their kinematics in

the Galaxy can be investigated)^{22–24}. While most of the searches for cool and ultracool companions to nearby stars focus on close angular separations (of a few arcseconds), which require the use of high-resolution-imaging techniques (such as adaptive optics) or facilities (such as the *Hubble Space Telescope*)^{25–27}, some benchmark late-type dwarfs are located at much wider separations (of several arcminutes)^{28–30}. Moreover, the closest star to the Sun is a late-type dwarf at over two degrees from a solar-like binary system: Proxima Centauri (M5.5 Ve)^{31–33}. At the heliocentric distance of the α Centauri system, that angular separation translates into a physical separation of 12 000 AU. That value contrasts with the tendency of equal-mass cool and ultracool binaries to be very tight^{34–36}, with physical separations rarely larger than 30 AU. However, there are a few examples of common-proper-motion pairs with very low total masses that have separations of over 1000 AU and that represent a challenge for star-formation scenarios^{37–39}.

In this paper, I present the first of a series of works devoted to investigating cool dwarfs in binary systems. They can be either newly discovered late-type wide companions to *Hipparcos* stars, poorly known equal-mass systems with very wide physical separations, or known systems with cool and ultracool components that require further characterization. I discover, recover, identify, or notice the lack of information on the systems presented in this series while I accomplish other different programmes, mainly using the *Aladin* sky atlas of the Virtual Observatory⁴⁰, either as a single author or within a collaboration. In a sense, many of my targets are examples of serendipitous discoveries or by-products of large surveys.

Koenigstuhl 4 AB: two mid-M dwarfs in a loosely-bound common-proper-motion pair

In the first of the series, I present the serendipitous identification of two late-type stars that have similar high proper-motions and are separated by about 5 arcmin. They have been tabulated only by Luyten⁴¹ (*NLTT*) and Lépine & Shara⁴² (*LSPM*), and have never been presented as a bound binary system. Those authors provided only their coordinates, approximate proper-motions, and photographic *BRI* (in the visible) and *JHK_s* (in the near infrared) magnitudes. Besides, Luyten estimated “k–m” and “m” spectral types for the bright and faint components, respectively, from visible photometry.

In Table I, I compile names, coordinates, and magnitudes from a number of sources (2MASS: *Two-Micron All-Sky Survey*⁴³; CMC14: *Carlsberg Meridian Catalogue 14*⁴⁴; USNO-B1.0: US Naval Observatory-B⁴⁵; and references above) for the two components in the system, NLTT 6496 and NLTT 6491. Hereafter, I refer to them as Koenigstuhl 4 A and Koenigstuhl 4 B, respectively. This is a continuation of the *Koenigstuhl* nomenclature introduced by Caballero³⁷, in which new systems were designated with the name of a mountain near Heidelberg, Germany.

I identified the Koenigstuhl 4 system as a promising low-mass proper-motion system while searching for new and poorly-investigated late-M dwarfs bright enough to be suitable targets for radial-velocity searches for exoplanets. As illustrated in Fig. 1 and shown in Table II, the catalogued proper-motions of Koenigstuhl 4 A and B are very similar, although not identical. Since the *NLTT* and *LSPM* proper-motion error bars are not tabulated, the USNO-B1.0 ones may be underestimated and the PPMXL (Positions and Proper Motions-Extended Large⁴⁶) proper motions are marginally consistent only within 1 σ uncertainties, a dedicated astrometric study was needed confidently to discard or confirm the common proper-motion of A and B.

TABLE I
Basic data for Koenigstuhl 4 A and B

Datum	A	B	Origin
NLTT	6496	6491	NLTT
LSPM	J0156+3033	J0156+3028	LSPM
α J2000	01 ^h 56 ^m 45 ^s .71	01 ^h 56 ^m 41 ^s .48	2MASS
δ J2000	+30° 33' 28".8	+30° 28' 48".9	2MASS
B_T (mag)	16.0	17.7	NLTT
r (mag)	14.449	16.318	CMC14
R_F (mag)	14.1	15.9	NLTT
I_N (mag)	11.75	13.32	USNO-B1.0
J (mag)	10.323	11.917	2MASS
H (mag)	9.718	11.305	2MASS
K_s (mag)	9.449	11.029	2MASS
Sp. Type	M4.5±0.5 V:	M6.5±0.5 V:	This work
M (M_\odot)	0.22 ^{+0.03} _{-0.04}	0.12±0.01	This work

TABLE II
Proper motions of Koenigstuhl 4 A and B

Origin	A $\mu_\alpha \cos \delta$ (mas yr ⁻¹)	μ_δ (mas yr ⁻¹)	B $\mu_\alpha \cos \delta$ (mas yr ⁻¹)	μ_δ (mas yr ⁻¹)
NLTT	+230	-16	+259	-41
USNO-B1.0	+220±2	-10±1	+210±1	-10±1
LSPM	+229	-13	+220	-8
PPMXL	+219±5	-12±5	+208±4	-12±4
This work	+213.0±1.1	-15.1±0.4	+205.8±1.2	-15.5±0.5

TABLE III
Astrometric observations of the Koenigstuhl 4 AB system

Epoch	ρ (arcsec)	θ (deg)	Origin
1951 Nov. 05	298.9±0.3	190.6±0.2	POSSI
1991 Sep. 17	298.9±0.3	190.5±0.2	POSSII Red
1993 Sep. 15	299.0±0.3	190.5±0.2	POSSII Blue
1995 Oct. 15	299.1±0.3	190.5±0.2	POSSII Infrared
1999 Nov. 05	298.96±0.10	190.5±0.2	2MASS
2001 Oct. 05	298.85±0.07	190.6±0.2	UCAC3
2003 Dec. 06	299.1±0.2	190.4±0.2	CMC14

I followed the procedure described by Caballero⁴⁷ to measure the proper-motions of Koenigstuhl 4 A and B. I used eight (A) and seven (B) astrometric epochs (Table III) taken from 2MASS, CMC14, the SuperCOSMOS⁴⁸ digitizations of the photographic plates from the *United Kingdom Schmidt Telescope (UKST)* and first epoch of the Palomar Observatory Sky Survey (POSSI), the *Third US Naval Observatory Astrograph Catalog (UCAC3)*⁴⁹, and the *Hubble Space Telescope Guide Star Catalogue*, version 1.2 (GSC1.2)⁵⁰. The first and last epochs were separated by 51 years. Koenigstuhl 4 B was not catalogued by GSC1.2 (epoch 1983 September 08).

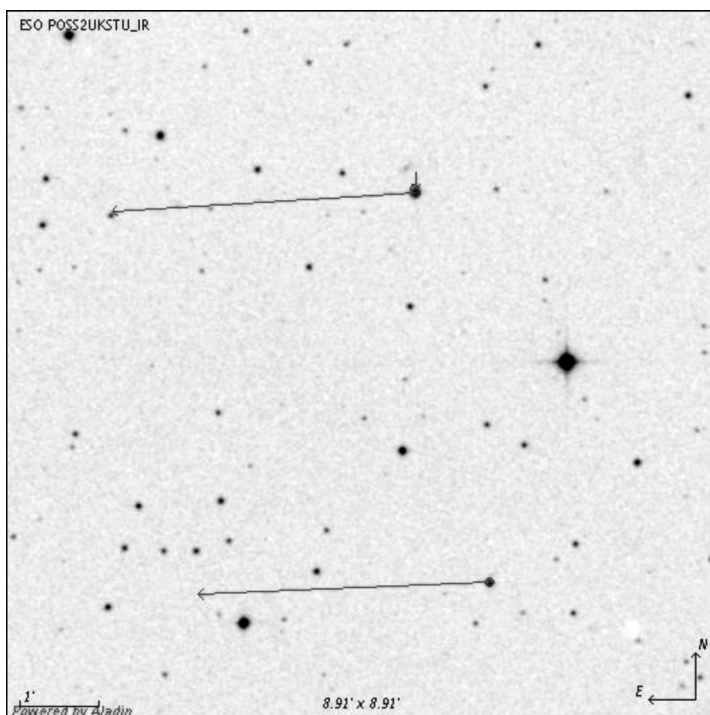


FIG. 1

UKST I_N -band image provided by *The Digitized Sky Survey* from the European Southern Observatory at Garching constructed with *Aladin* showing Koenigstuhl 4 A (top) and B (bottom). The long arrows indicate the *LSPM* proper-motions as tabulated by *Simbad*. The primary is marked with a short arrow.

The proper-motions measured by me (last row of Table II) resemble each other more than in the case of *NLTT*, *USNO-B1.0*, *LSPM* or *PPMXL*. The resemblance led to a definitive test, which was to measure the constancy with time of the angular separation (ρ) and position angle (θ) between Koenigstuhl 4 A and B. That test is more reliable than measuring the proper-motions of the components separately because it is not affected by the parallax (indeed, the parallactic wobbling is the main contributor to the 'large' error bars of $1.1\text{--}1.2\text{ mas yr}^{-1}$ in right-ascension proper motion).

Table III summarizes the ρ and θ measurements for the seven available astrometric epochs. The UCAC3 and CMC14 epochs of A and B were not simultaneous, but separated by 7 and 62 days, respectively, so I took the average date for the computation. In spite of that source of error, the standard deviations of the mean angular separation and position angle in the 51-year interval remained rather small, of only 90 mas and $0^\circ.05$, respectively, which confidently confirmed the common proper-motion of Koenigstuhl 4 A and B.

Once the common proper-motion was confirmed, the system needed to be characterized in detail. The basic data of Koenigstuhl 4AB as a system are summarized in Table IV. By comparing the $r'-\mathcal{J}$ and $\mathcal{J}-K_s$ colours measured for Koenigstuhl 4 A and B with those of known M dwarfs with Sloan Digital Sky

TABLE IV
Basic data of the Koenigstuhl 4 AB system

$\langle \rho \rangle$ (arcsec)	298.97 ± 0.09
$\langle \theta \rangle$ (deg)	190.49 ± 0.05
d (pc)	19^{+6}_{-4}
s (AU)	5700^{+1800}_{-1200}
M_{total} (M_{\odot})	$0.34^{+0.04}_{-0.05}$
$-U_{\text{g}}^*$ (J)	$7.7\text{--}8.2 \times 10^{33}$

Survey (SDSS; $u'g'r'i'z'$) and 2MASS (JHK_s) photometry from West *et al.*⁵¹ (CMC14 r' magnitude is in the SDSS system) and the magnitude difference between the two components, I concluded that the only possible spectral type combinations are $M4.0\text{ V}+M6.0\text{ V}$, $M4.5\text{ V}+M6.5\text{ V}$ or $M5.0\text{ V}+M7.0\text{ V}$ (see the bottom of Table I). Assuming the intermediate combination as the most probable one and spectral-type uncertainties of 0.5 sub-types, and using the spectral-type- J -band absolute-magnitude relationship given by Caballero *et al.*⁵², I derived a conservative distance of 19^{+6}_{-4} pc. From the M_J magnitudes and the NEXTGEN models⁵³ for ages older than 1 Gyr, I estimated the masses of Koenigstuhl 4 A and B at around 0.22 and 0.12 M_{\odot} , which gave the system a total mass of only around 0.34 M_{\odot} (Table IV).

The spectral-type estimation is supported by the existence of a nearby *ROSAT* All-Sky Survey X-ray source, 1RXS J015645.8+303332 (count rate $\text{CRT} = 4.4 \pm 1.5 \text{ ks}^{-1}$)⁵⁴, at only 3.3 arcsec from Koenigstuhl 4 A. Because of its star-like hardness ratios ($HR_1 = -0.5 \pm 0.3$, $HR_2 = -0.7 \pm 1.4$) and the large incidence of X-ray activity among mid-M dwarfs^{55–57}, I propose that the origin of the 1RXS source is Koenigstuhl 4 A itself.

The projected physical separation s of around 5700 AU makes Koenigstuhl 4 AB one of the widest systems with $M_{\text{total}} < 0.4 M_{\odot}$. A very wide binary is synonymous with a weakly bound system in the process of being disrupted by the gravitational potential of the Galaxy^{58–60}. As a proxy to the gravitational potential energy U_{g} , I computed $U_{\text{g}}^* = -GM_1M_2/s$ for Koenigstuhl 4 AB, which is one of the lowest binding energies known. I provide an interval of $-U_{\text{g}}^*$ in Table IV because it peaks at the most probable values ($M4.5\text{ V}+M6.5\text{ V}$ at $d = 19$ pc); cooler spectral types imply a smaller physical separation (*i.e.*, a smaller distance) but less total mass, while warmer spectral types imply a larger total mass but a wider physical separation (*i.e.*, a larger distance).

Koenigstuhl 4 AB falls in a binding-energy/total-mass diagram (*e.g.*, Fig. 2 in Caballero⁶⁰) very close to the only representatives of the rare class of very wide ($s > 1000$ AU), very-low-mass ($M_1+M_2 < 0.2 M_{\odot}$), equal-mass ($M_2/M_1 \sim 1$) binaries: Koenigstuhl 1 AB ($s \sim 1800$ AU)³⁷, 2M0126–50 AB ($s \sim 5200$ AU)³⁸, and 2M1258+40 AB ($s \sim 6700$ AU)³⁹, which are the least-bound multiple systems with low-mass components known to date with the exception of the very young brown-dwarf-plus-exoplanet system 2M1207–39 AB⁶¹. The larger total mass of Koenigstuhl 4 AB is compensated by its wider physical separation, which makes the system a new challenge for low-mass star-formation scenarios and studies of dynamical encounters in the Galactic disc. Although there is an obvious lack of data on the system (*i.e.*, effective temperatures, surface gravities, activity indicators, radial velocities, accurate photometry in the optical) one would venture to guess that Koenigstuhl 4 AB is in the process of disruption.

Low-resolution optical spectroscopy for accurate spectral-type determination and study of the H α emission (a signpost of activity) of Koenigstuhl 4 A and B is planned for the near future.

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A PHOTOMETRIC STUDY OF THE DWARF ALGOL BINARY
V1001 CASSIOPEIAE

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A photometric analysis of the very-short-period ($\sim 0^d.43$) dwarf binary with a classical Algol-type curve, V1001 Cassiopeiae, is presented. Its short period, similar to the majority of W UMa variables, yet distinct EA light-curve, make it a rare and interesting system for photometric investigation. The photometric *VRI* standard magnitudes suggest a K4 spectral type. A period study and synthetic *BVR* and *I* light-curve solutions are presented. The *I* curve was modelled separately, but there are only slight differences in the solutions. Our present solution, following a mass-ratio search, gives a mass ratio of near 0.38, a temperature difference of ~ 800 K, with fill-out factors of $\sim 70\%$ and $\sim 85\%$ of the primary and secondary star contact Roche lobes, respectively. A cool spot with $\sim 40^\circ$ radius was used to fit the interesting asymmetries of the light-curve. The secondary component is in the M1–M5 range of main-sequence spectral types.

Introduction

Numerous claims for the shortest-period Algol-type systems are found in the literature. These include W Crv¹, which actually has an EB light curve, and VZ Scu², which is a blue nova-like variable. Neither of these have classical Algol light-curves. A classical Algol means a light-curve with a large eclipse difference, a shallow-amplitude secondary, and relatively long stretches of low variation outside eclipse. In this regard, we report the first high-precision, multi-band light-curves of V1001 Cas, possibly the shortest-period, non-degenerate, classical Algol. It was observed as a part of our student/professional collaborative studies of interacting binaries from data taken in conjunction with the National Undergraduate Research Observatory (NURO) and the Southeastern Association for Research in Astronomy (SARA).

Observational history

The position of V1001 Cas [GSC 3651-0655, 2MASS J23501708+5111288, MisV1222, USNO-A2.0 1350-18742581] is $\alpha(2000) = 23^{\text{h}} 50^{\text{m}} 15^{\text{s}}.876$, $\delta(2000) = +51^{\circ} 11' 52''.85$. It was discovered in 2005 by Seiichi Yoshida of the MISA0 Project³ and identified as an EB type or possibly an EA/RS-type variable with a period of $0^{\text{d}}.4288$. They report a magnitude range of $13.61 - 14.71$. V1001 Cas appeared in the 78th name-list of variable stars⁴.

The present observations

Our *B*, *V*, *R*, and *I* data on V1001 Cas were recorded with the Lowell 31-inch reflector in Flagstaff with a CRYOTIGER-cooled ($<-100^{\circ}\text{C}$) NASACAM and a $2\text{K}\times 2\text{K}$ chip and standard *BVR_cI_c* filters. They were taken on 2009 September 26 and 27 on NURO scheduled observing time, and on 2009 September 28 using the SARA 36-inch telescope at KPNO in remote mode. The SARA camera was a Finger Lakes $2\text{K}\times 2\text{K}$ CCD cooled to -20°C . The light-curves subjected to synthetic-light-curve modelling included 110 *B*, 114 *V*, 115 *R*, and 114 *I* individual CCD observations. Typical NURO exposure times were 160s in *B*, 60s in *V*, and 40s in *R* and *I*. Images were calibrated in a standard way using biases, dark frames, and *UBVRI* sky flats. The variable, *V*, the comparison star, *C* (GSC 3657 0606) [$\alpha(2000) = 23^{\text{h}} 49^{\text{m}} 55^{\text{s}}.910$, $\delta(2000) = +51^{\circ} 11' 06''.85$], and the check star, *K* (GSC 3657 0633) [$\alpha(2000) = 23^{\text{h}} 50^{\text{m}} 15^{\text{s}}.88$, $\delta(2000) = +51^{\circ} 11' 52''.85$], are shown on the finding chart in Fig. 1. A plot of the *R* and *I* data taken on 2009 September 26 is shown in Fig. 2. The *BVR_cI_c* observations in ΔM , in the sense of variable minus comparison, are given at:

<http://usclanaster.sc.edu/faculty/faulkner/V1001CassiopeiaObservations.pdf>.

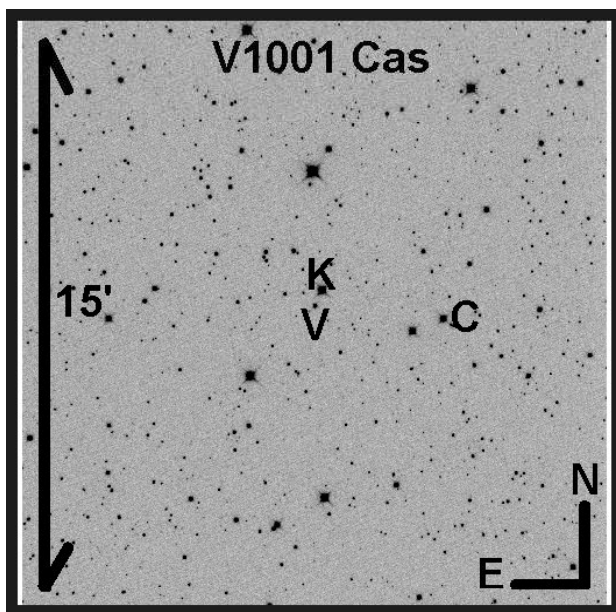


FIG. 1

Finder chart of V1001 Cas (V), comparison (C) and check (K) stars.

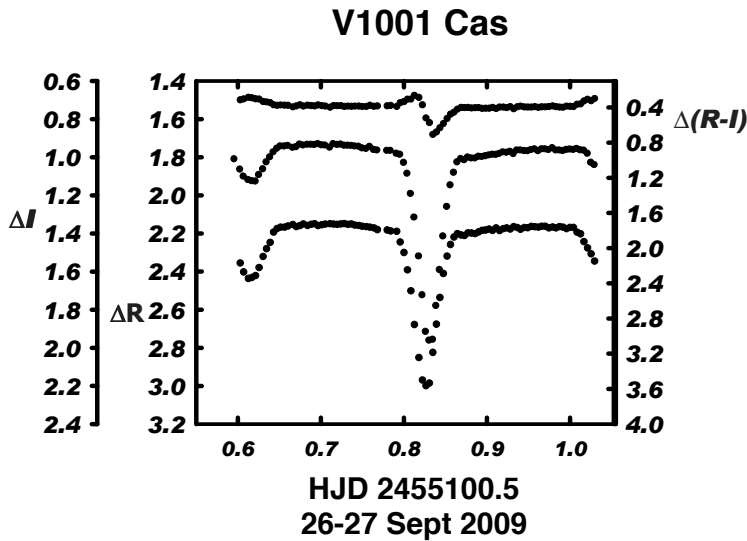


FIG. 2
R and I data of V1001 Cas taken on 2009 September 26.

Period determination

Discovery observations of this variable are available to the public⁵. From these data, we performed parabolic fits to the eclipses and determined five new times of minimum light which we used in our period determination. Additional times of minimum light were calculated from our present observations using averages of parabolic fits. These are given in Table I along with their standard errors. Using all available data, we calculated the following improved linear ephemeris:

$$\text{HJD Min I} = 2455101.6890 \pm 0.0008 + (0.4287188 \pm 0.0000002) E. \tag{1}$$

The O – C residuals calculated using equation (1) are given, along with all available timings and their O – C residuals, in Table I.

TABLE I
O – C residuals calculated from equation (1).

	<i>Epochs</i>	<i>Cycles</i>	<i>O – C</i>	<i>Reference</i>
1	2452931.9439	–5061.0	0.0009	3,5
2	2452934.9397	–5054.0	0.0044	3,5
3	2452936.0186	–5051.5	0.0028	3,5
4	2452937.0847	–5049.0	0.0030	3,5
5	2452939.0206	–5044.5	0.0038	3,5
6	2455100.6160 ± 0.0005	–2.5	0.0012	this paper
7	2455100.8306 ± 0.0036	–2.0	0.0010	this paper
8	2455101.6888 ± 0.0002	0.0	0.0002	this paper
9	2455103.6196 ± 0.0005	4.5	0.0014	this paper
10	2455103.8337 ± 0.0003	5.0	0.0011	this paper

TABLE II
Standard magnitudes for *V1001 Cas*, comparison, and check stars

Star	<i>V</i>	<i>V</i> − <i>R</i>	<i>R</i> − <i>I</i>	<i>V</i> − <i>I</i>	Sp. Type
Var. ($\phi = 0$)	15.46 ± 0.21	0.56 ± 0.03	0.51 ± 0.02	1.10 ± 0.04	K4 ± 1
Comp.	12.03 ± 0.01	0.18 ± 0.01	0.14 ± 0.00	0.32 ± 0.01	F0 ± 1
Check	11.26 ± 0.01	0.13 ± 0.01	0.08 ± 0.00	0.21 ± 0.01	A6 ± 1

Standard magnitudes

Standard magnitudes were determined from observations of Landolt standard stars on 2009 December 26–27, by using standard procedures⁶. Extinction and transformation coefficients were calculated and standard magnitudes were derived for the variable, comparison, and check-star images. The results of our calculations are given in Table II. We find⁷ that the variable is a mid-K-spectral-type dwarf, while the comparison star is an early-F-type dwarf and the check star is a mid-A-type dwarf. Both transformation and standard-star errors are included in Table II. The *B* − *V* colour index results are not given in the table, since they are of low quality but they suggest that the binary is of F9 type rather than mid-K, as the *R*, *I* and infrared colour indices indicate. However, the *B* − *V* results of the comparison and check give the same spectral type as the ones in the table, indicating the binary system, itself, may be displaying an infrared excess. This could be due to the previous destruction of terrestrial planets in this binary system, as suggested by Matranga⁸ (circumstellar matter would serve as well). We mention this in passing, to be explored by other observers.

Light-curve characteristics

Phased light-curves using equation (1) to fold the light-curves, are given in Figs. 3a and 3b. The ΔB , ΔV , ΔR , and ΔI magnitude curves are typical of a classical Algol system with a primary amplitude of $\Delta V = 1^{\text{m}}.1$ and a secondary of only $0^{\text{m}}.14$. The out-of-eclipse portions are quite featureless. However, the colour curves do show some night-to-night variation, thus revealing that the system is spot-active, as expected for a K-type system. The system is definitely detached so the secondary component is likely to be in the M-type range. The light-curve characteristics are given in Table III.

Synthetic-light-curve modelling

We first hand-fitted each light-curve individually with BINARY MAKER 3.0, using standard convective parameters and limb-darkening coefficients from reasonable values dictated by the spectral type. In these models we used dark spots to fit the asymmetries in the curves. Using our starting values and the primary-component temperature from our standard magnitudes (4500K) and temperature-related values as dictated by the standardized photometry, we proceeded to compute a simultaneous four-colour light-curve solution with the 2004 Wilson Code^{10–13}, which includes Kurucz stellar atmospheres rather than black-body, two-dimensional limb-darkening coefficients, and a detailed reflection treatment. Our fixed inputs included standard convective parameters, gravity darkening, $g = 0.32$, and albedo values of 0.5. We used Mode 2 in our analysis (the detached configuration). We did some of our intermediate modelling steps with the Wilson-code-based PHOEBE¹⁴ program to speed up the process. Adjustable parameters include those accompanied by errors (see

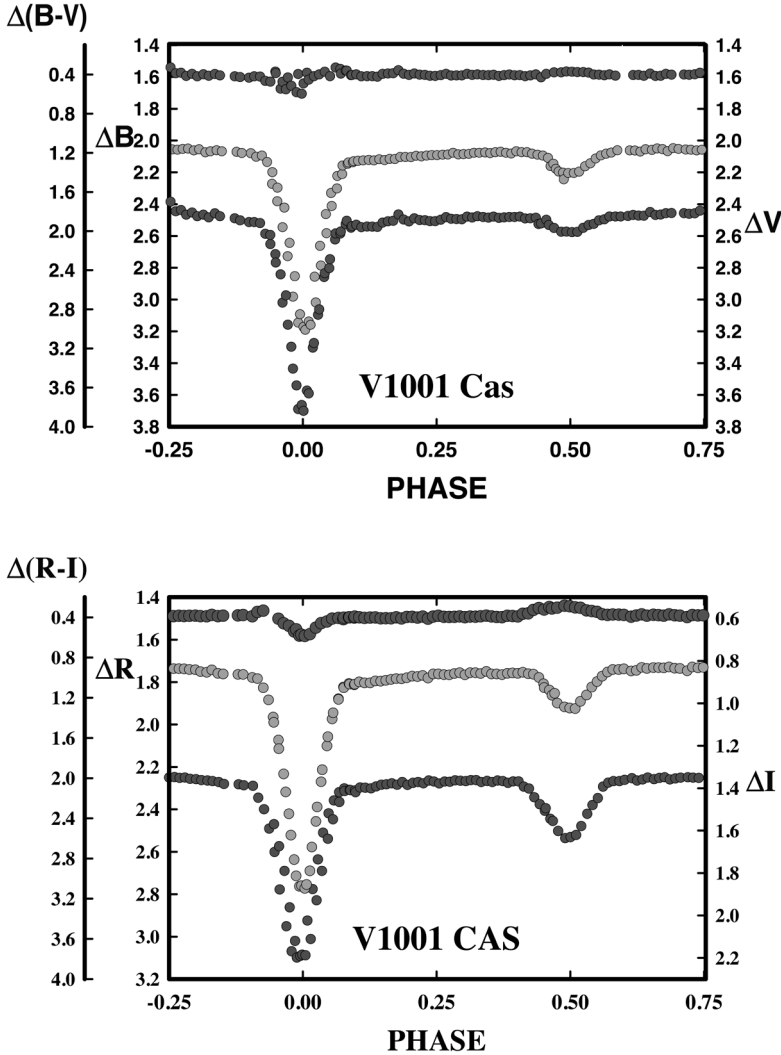


FIG. 3a, b

Phased ΔB , ΔV & ΔR , ΔI differential-magnitude light-curves and $\Delta(B-V)$, $\Delta(R-I)$ colour curves.

Table IV), the inclination, i , the temperature of the secondary component, T_2 , the potentials, Ω_1 and Ω_2 , the mass ratio, $q = m_2/m_1$, and the normalized flux (at 4π) in each wavelength, L , the phasing ephemeris, \mathcal{JD}_0 and the period P , and the four spot parameters. The I -curve solution did not fit the light-curves well, so 3-colour BVR and I solutions were performed separately. Full synthetic-light-curve solutions follow. Our initial, trial solution gave a mass ratio near 1 so we ran a mass-ratio or q search, a sequence of solutions at fixed mass ratios — see Fig. 4 — which minimized at $q \sim 0.38$. From this solution, we allowed the

TABLE III
Light-curve characteristics

Phase	0 ^h 00	0 ^h 25	0 ^h 50	0 ^h 75
ΔB	3.66	2.50	2.57	2.45
ΔV	3.15	2.09	2.20	2.06
ΔR	2.75	1.76	1.92	1.73
ΔI	2.15	1.37	1.63	1.35
$\Delta(B - V)$	0.51	0.41	0.37	0.39
$\Delta(R - I)$	0.60	0.39	0.29	0.38
Amplitude:	0.00–0.75	0.50–0.75	0.50–0.25	0.25–0.75
B	1.21	0.12	1.15	0.05
V	1.09	0.14	1.06	0.03
R	1.01	0.19	0.99	0.03
I	0.80	0.28	0.77	0.02

mass ratio to iterate along with the other variables to achieve our final BVR and I solutions. Our complete solutions are given in Table IV and shown in Figs. 5a and 5b, where the solution is overlying the normalized-flux light-curves. The Roche-lobe surfaces arising from the calculation are displayed in Fig. 6.

Discussion

Our models shows that V1001 Cas is a rare, dwarf, detached, Algol binary with a mass ratio of ~ 0.38 . The system parameters from our models include a temperature difference of ~ 800 K, with Roche-lobe fill-out factors of about 70% and 85% of its critical contact lobe for the primary and secondary

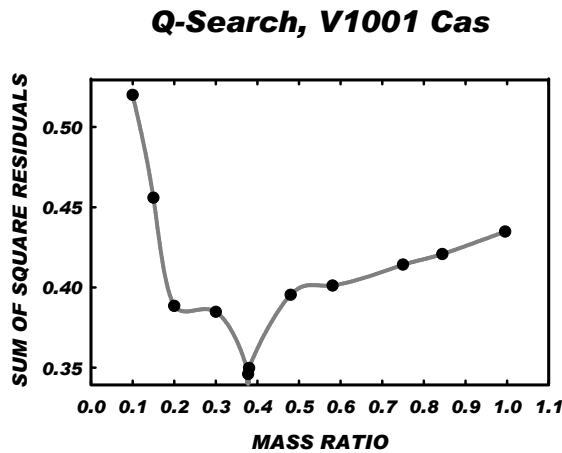


FIG. 4

Mass-ratio (q) search: fixed q -solutions vs. sum of square residuals to determine the best q -value.

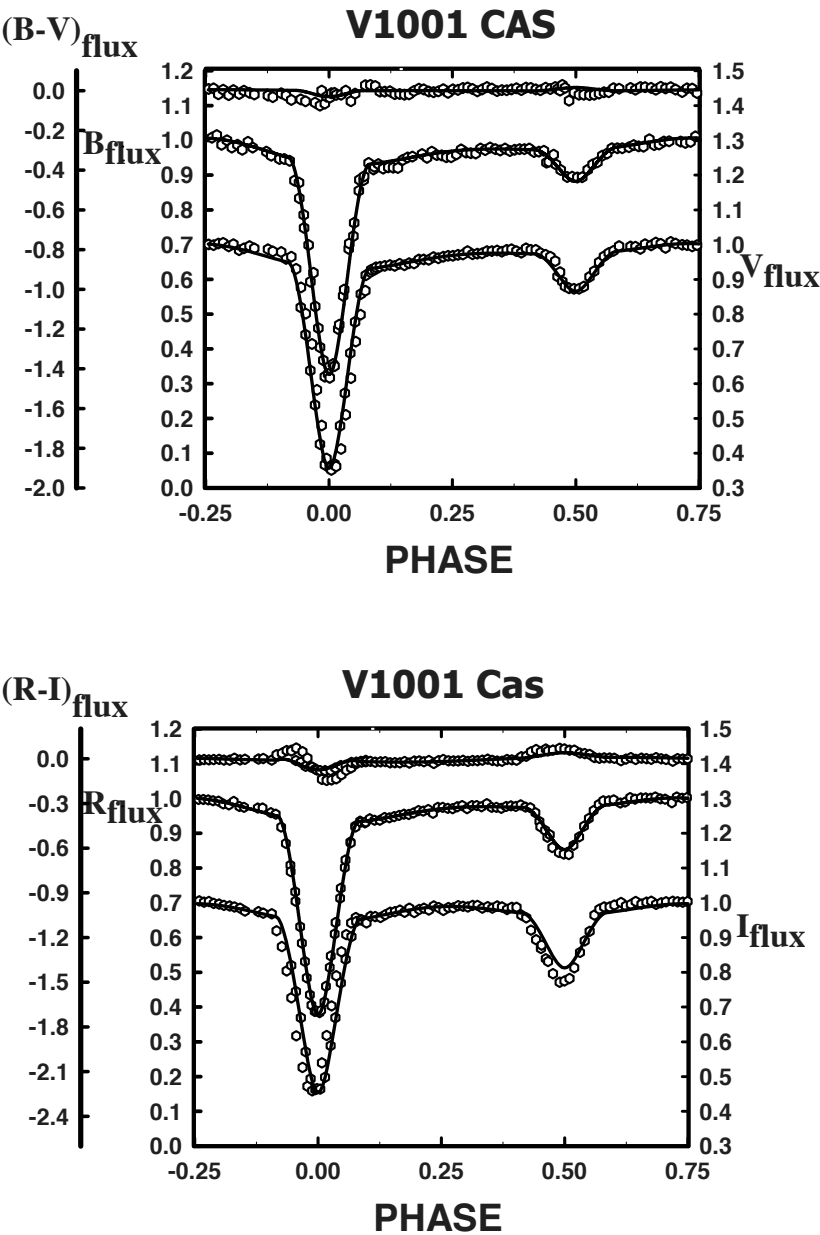


FIG. 5a, b

B, V & R, I normalized flux curves overlain by our solution.

TABLE IV
Synthetic-light-curve solutions

Parameters	BVR Solution	I Solution
$\Delta B, \Delta V, \Delta R, \Delta I$ (nm)	440, 550, 640	790
$xbol_{1,2}, ybol_{1,2}$	0.630, 0.578, 0.146, 0.224	0.630, 0.578, 0.146, 0.224
x_{1P^2P}, y_{1P^2P}		0.668, 0.685, 0.144, 0.235
x_{1R^2R}, y_{1R^2R}	0.754, 0.767, 0.096, 0.191	
x_{1V^2V}, y_{1V^2V}	0.799, 0.797, 0.006, 0.137	
x_{1B^2B}, y_{1B^2B}	0.840, 0.827, 0.155, 0.016	
g_1, g_2	0.32, 0.32	0.32
A_1, A_2	0.50, 0.50	0.50
Inclination ($^\circ$)	87.8 \pm 0.2	85.0 \pm 0.1
T_1, T_2 (K)	4500(fixed), 3689 \pm 2	4500(fixed), 3733 \pm 0.5
Ω_1, Ω_2	3.806 \pm 0.006, 3.064 \pm 0.003	3.77 \pm 0.01, 3.039 \pm 0.007
q (m_2/m_1)	0.3775 \pm 0.0008	0.386 \pm 0.002
Fill-outs: F_1, F_2	70%, 86%	71%, 87%
$L_1/(L_1+L_2)_I$		0.835 \pm 0.011
$L_1/(L_1+L_2)_R$	0.979 \pm 0.010	
$L_1/(L_1+L_2)_V$	0.963 \pm 0.004	
$L_1/(L_1+L_2)_B$	0.949 \pm 0.026	
JD ₀ (days)	2455101.68869 \pm 0.00005	2455101.6884 \pm 0.0001
Period (days)	0.428928 \pm 0.000035	0.4309 \pm 0.0008
r_1, r_2 (pole)	0.290 \pm 0.002, 0.212 \pm 0.003	0.296 \pm 0.004, 0.217 \pm 0.006
r_1, r_2 (point)	0.302 \pm 0.002, 0.229 \pm 0.005	0.308 \pm 0.004, 0.233 \pm 0.009
r_1, r_2 (side)	0.296 \pm 0.002, 0.217 \pm 0.004	0.302 \pm 0.004, 0.221 \pm 0.007
r_1, r_2 (back)	0.299 \pm 0.002, 0.225 \pm 0.004	0.306 \pm 0.009, 0.230 \pm 0.008
<i>Starspot parameters</i>		
Colatitude	9.8 $^\circ$ \pm 0.4 $^\circ$	2.7 $^\circ$ \pm 0.3 $^\circ$
Longitude	325 $^\circ$ \pm 1 $^\circ$	315 $^\circ$ \pm 2 $^\circ$
Spot radius	40.8 $^\circ$ \pm 0.2 $^\circ$	40.9 $^\circ$ \pm 0.2 $^\circ$
T-Factor	0.761 \pm 0.008	0.68 \pm 0.02

components, respectively. We needed one moderately sized, $\sim 40^\circ$ radius, dark spot on the primary component to model the light-curve asymmetries. Our photometry reveals that the primary component is a main-sequence mid-K type and the secondary component is M1 V by temperature and M4.5 V type by mass (assuming our mass ratio and the primary is of mass $\sim 0.67 M_\odot$). So, from the light-curves, we predict that the secondary lies in the range, M1–M4.5. We have to await the determination of radial-velocity curves for the system to make a more definitive statement. The spectral types of the variable suggested by the 2MASS $J-H$ and $H-K$ colours are $\sim K7$.

With the given mass ratio, period, and estimating the primary mass to be $0.67 M_\odot$, from Newton's form of Kepler's law, we compute the semi-major axis to be $a = 2.33 R_\odot$. The solution gives average radii of the primary component and secondary component of $0.69 R_\odot$ and $0.21 R_\odot$, respectively. This is consistent with normal K4.5 and M5 field stars, respectively, for the primary and secondary components. These values lie within one spectral type of the previous estimate. We conclude that the stars have interacted little and retain their main-sequence characteristics. Again, radial-velocity curves combined with light-curves are needed to confirm our values calculated from light-curve analyses alone.

Since the binary is of solar type (magnetically active), we expect this system will eventually become a very-short-period W UMa contact-binary. Thus, the system could be classified as a pre-contact W UMa system. The process of coalescence is caused by magnetic braking due to stellar winds leaving the

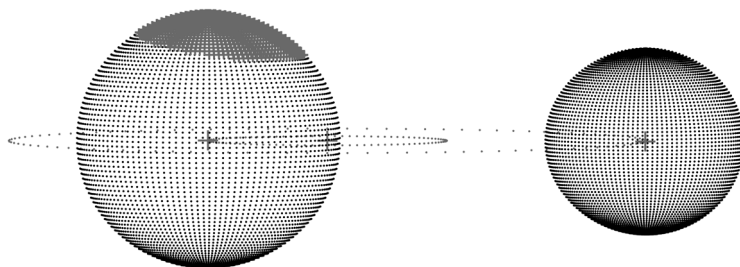


FIG. 6
Roche-lobe surface of V1001 Cas at phase 0.24.

system *via* stiff dipole-magnetic-field lines. This system should be monitored for the next ten years to determine the period behaviour of the system.

Acknowledgements

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SPECTROSCOPIC BINARY ORBITS
FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 222: HR 4241, HR 7208, HR 8026, AND HR 8149

By R. F. Griffin
Cambridge Observatories

HR 4241 and HR 7208, whose spectral types have been reported as gG8 and K2 III, both have orbits that are indistinguishable from circles, although their orbital periods of 300 and 611 days, respectively, greatly exceed the ~ 120 -day limit above which the orbits even of giant stars are often found to be eccentric. HR 8026 and HR 8149, of types G8 II–III and K5 III, have orbits of longer periods (about 17 years and 1000 days, respectively), and modest eccentricities. All four stars appear to be single-lined. Except in the case of HR 8026 their mass functions are all in the range $0.005\text{--}0.007 M_{\odot}$, requiring minimum masses for the secondaries only around $0.3 M_{\odot}$ if the primaries are assumed to be near $2 M_{\odot}$. HR 8026 has a large mass function ($0.4 M_{\odot}$) which is difficult to understand otherwise than as indicating that the secondary object is itself a binary system.

Introduction

This paper gives orbits for four more ‘bright stars’, in which category, however, HR 4241 is fortunate to be included in view of its magnitude of $6^{\text{m}}.73$ being appreciably fainter than the $6^{\text{m}}.5$ ostensible limit of the *Bright Star Catalogue*.

HR 4241 (HD 94083)

HR 4241 is not only among the faintest stars to feature in the *Bright Star Catalogue*, but it was also one of the last to have its magnitude accurately determined: in 1986 Hauck & Mermilliod¹ listed it as one of only 86 ‘Bright Stars’ that were still lacking photometry at that time. The situation was remedied five years later by Oja², who obtained the results $V = 6^{\text{m}}.73$, $(B - V) = 1^{\text{m}}.09$, $(U - B) = 1^{\text{m}}.03$. There seems not to be, even now, any MK classification of the star; the nearest that there is to a two-dimensional classification is the type of gG8 listed by (R. E.) Wilson & Joy, in the context of their publication³ of a lot of radial velocities. The luminosity classification as a giant was supported by the early spectroscopic-parallax estimates made at the Dominion Astrophysical Observatory (DAO) by Young & Harper⁴, who independently obtained absolute magnitudes of $+0.5$ and $+0.7$ for HR 4241, indicating a spectroscopic parallax of $0''.006$; the *Hipparcos* value is 4.37 ± 0.77 milliseconds, and leads to $M_V \sim -0^{\text{m}}.1 \pm 0^{\text{m}}.4$.

The star has evidently enthused radial-velocity observers more than photometrists and classifiers, since three consortia had observed its velocity before the present writer did so. First, Plaskett *et al.*⁵ measured it four times at the DAO in 1919/20, shortly after that Observatory and its 72-inch reflector (then second only to the Mount Wilson 100-inch) were set up. The velocities had a mean of -12.6 km s^{-1} and a range of 4 km s^{-1} , which was not enough

to raise any suspicions of true variability. Then it was observed four times at Mount Wilson, in 1934–49; it was one of many stars that seemed not to be part of any systematic programme but whose velocities were published in 1950 in the paper by Wilson & Joy³ mentioned above, in a sort of clearing-up operation. They referred to four measurements that yielded a mean of -4.1 km s^{-1} with a ‘probable error’ of 1.7 km s^{-1} , and pointed out in a note that their value was discordant enough with the DAO one that the velocity was probably variable. The evidence for variability was, in truth, enhanced by what must now be seen as a mistake of some sort in the first Mount Wilson observation, which gives a residual of more than 10 km s^{-1} from the orbit determined below; we are indebted to Abt⁶ for publishing individually, with dates, the velocities that were given by Wilson & Joy only as a mean. Next came the 12 measurements by Beavers & Eitter⁷ with the photoelectric spectrometer⁸ on the 24-inch *Mather* reflector at the Fick Observatory of Iowa State University at Ames. They noted that the standard deviation of the mean of their velocities was nearly twice the value expected from the independently judged internal errors of the individual data. They demanded that it should be three times as great before they would claim real variability, although in fact three times the *variance*, which the HR 4241 data easily surpassed, would be appreciably beyond the 5% level of significance according to the variance ratio $F_{12,12}$. Beavers did, however, include the object, with the note ‘rapid’, in a table of ‘probable variable velocity stars’ that he presented at a 1984 conference⁹ on stellar radial velocities.

It was only in 2005 that HR 4241 was placed on the observing programme of the Cambridge *Coravel*, with which 42 radial-velocity measurements have been made. They are listed in Table I, along with the velocities to be found in the literature cited above, which have all been adjusted by $+0.8 \text{ km s}^{-1}$ in an effort to bring them more into accord with the zero-point commonly adopted in this series of papers. (The same adjustment has been made to published observations of the other stars treated in this paper.) The Ames observations have been attributed a weight of 0.05 in comparison with the Cambridge ones; their weighting has been further multiplied by 0.5 or 0.2 in cases where the Ames authors⁷ designated the observations as having quality B or C, respectively. The DAO and Mount Wilson velocities have been weighted 0.01, except for the first Mount Wilson one, which (as noted above) appears to be ‘wild’ and has been rejected. The orbit is shown in Fig. 1 and its elements are presented in Table VI below, after the descriptions of the other stars with which this paper is concerned.

The Cambridge observations carry such a large proportion of the weight of the whole data set that the elements that they give by themselves differ negligibly, either in their values or their standard deviations, from those given by the whole *ensemble*. The only exception is the period, where the enormous increase in the time base afforded by the early observations gives them enough leverage to offer an appreciable improvement despite their low weight. The Cambridge observations alone give a period of 300.22 ± 0.13 days, while the whole set gives 300.33 ± 0.06 .

The orbits of giant stars are often (though not invariably) found to be circular at periods up to a little over 100 days, such as those^{10,11} of HR 5983 (108 days) and HD 220636 (118 days), but beyond that they are usually eccentric. There is no doubt, however, that from the current data set the circular solution is the correct choice for the orbit of HR 4241: the sum of the squares of the weighted deviations falls only from 1.74 to 1.72 $(\text{km s}^{-1})^2$ when the imposition on the solution of zero eccentricity is relaxed, a reduction that is actually much *less*

TABLE I

Radial-velocity observations of HR 4241

Except as noted, the sources of the observations are as follows:
 1978–1984 — *Ames photoelectric velocity*[†] (weighted 0.05 in orbital solution);
 2005–2011 — *Cambridge Coravel* (weight 1)

<i>Date (UT)</i>	<i>MJD</i>	<i>Velocity km s⁻¹</i>	<i>Phase</i>	<i>(O–C) km s⁻¹</i>
1919 May 5.22*	22083.22	–11.1	104.435	+1.3
1920 Feb. 13.37*	22367.37	–14.4	103.381	–3.0
Mar. 1.32*	384.32	–12.1	.438	+0.4
Apr. 9.28*	423.28	–10.3	.567	+2.1
1934 June 1.19††	27589.19	+4.2	86.768	+10.7
1936 Feb. 9.43†	28207.43	–6.6	84.827	–2.1
May 12.17†	300.17	–5.0	83.135	–1.6
1949 Apr. 8.29†	33014.29	–5.7	68.832	–1.4
1978 Mar. 9.21	43576.21	–1.3	33.999	+0.1
28.22	595.22	–2.5	32.063	–0.7
May 16.11	644.11	–7.4	.225	–1.1
1979 Mar. 15.24	43947.24	–5.4	31.235	+1.2
Apr. 10.18	973.18	–10.2	.321	–0.6
1982 Feb. 6.35	45006.35	–7.6	28.761	–0.8
11.32	011.32	–5.4	.778	+0.8
Mar. 6.24	034.24	–4.1	.854	–0.4
28.17	056.17	–1.7	.927	+0.3
1983 Mar. 25.23	45418.23	–1.7	26.133	+1.6
1984 Jan. 25.36	45724.36	–5.4	25.152	–1.6
Feb. 21.29	751.29	–6.7	.242	+0.2
2005 Jan. 22.14	53392.14	–9.5	0.683	0.0
Mar. 18.98	447.98	–3.4	.869	–0.2
25.99	454.99	–2.6	.892	+0.1
Apr. 3.92	463.92	–2.0	.922	+0.1
21.93	481.93	–1.3	.982	+0.1
May 4.93	494.93	–1.4	1.025	+0.1
14.95	504.95	–1.6	.059	+0.2
21.91	511.91	–2.0	.082	+0.2
28.92	518.92	–2.4	.105	+0.2
June 8.91	529.91	–3.7	.142	–0.2
22.91	543.91	–5.1	.188	–0.1
Nov. 4.26	678.26	–11.0	.636	–0.1
19.19	693.19	–9.6	.685	–0.2
30.20	704.20	–8.1	.722	+0.1
Dec. 15.17	719.17	–6.2	.772	+0.2
2006 Jan. 27.25	53762.25	–1.9	1.915	+0.3
May 30.95	885.95	–9.8	2.327	0.0
June 9.94	895.94	–10.7	.361	+0.1
21.91	907.91	–12.1	.400	–0.3
30.94	916.94	–12.4	.430	0.0
Nov. 26.26	54065.26	–2.6	.924	–0.6
2007 Mar. 2.06	54161.06	–6.7	3.243	+0.2
Apr. 14.96	204.96	–11.7	.389	–0.1

TABLE I (concluded)

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O - C) km s ⁻¹
2007 May 5 ^h 9 ^m 2	54225.92	-12.6	3.459	+0.1
18 ^h 9 ^m 2	238.92	-12.7	.503	+0.2
29 ^h 9 ^m 3	249.93	-12.7	.539	0.0
June 10 ^h 9 ^m 3	261.93	-12.2	.579	0.0
20 ^h 9 ^m 3	271.93	-11.6	.612	-0.1
July 6 ^h 9 ^m 0	287.90	-10.2	.666	-0.1
Nov. 24 ^h 2 ^m 5	428.25	-3.2	4.133	+0.1
Dec. 8 ^h 1 ^m 6	442.16	-4.9	.179	-0.2
2008 Jan. 6 ^h 2 ^m 4	54471.24	-7.9	4.276	+0.2
2009 Mar. 26 ^h 9 ^m 6	54916.96	-6.6	5.760	+0.2
Apr. 5 ^h 9 ^m 4	926.94	-5.6	.793	0.0
19 ^h 9 ^m 0	940.90	-4.2	.840	-0.1
May 23 ^h 9 ^m 0	974.90	-1.7	.953	0.0
2010 Feb. 6 ^h 0 ^m 9	55233.09	-5.0	6.813	-0.1
May 21 ^h 9 ^m 0	337.90	-4.3	7.162	-0.2
June 4 ^h 9 ^m 2	351.92	-5.8	.208	-0.1
July 4 ^h 9 ^m 3	381.93	-9.4	.308	-0.2
2011 Apr. 6 ^h 9 ^m 7	55657.97	-6.5	8.228	-0.1
May 10 ^h 9 ^m 1	691.91	-10.1	.341	+0.2

* DAO photographic velocity⁵; weight 0.01.
† Mt. Wilson photographic velocity^{3,6}; weight 0.01.
‡ Mt. Wilson photographic velocity, rejected.

than could be expected to arise by chance for a truly circular orbit. The cost per degree of the extra two degrees of freedom represented by e and ω is only about a third of that of the other 55 degrees (there being 61 observations in the solution); they would need to cost three times as much as the others for the eccentric solution to be indicated at all strongly, the 5% point of $F_{2,55}$ being 3.17. The view of HR 4241 in the telescope is enlivened by the proximity (about 3'.7 due south) of the rather similar-looking star HR 4242, which has the immediately following catalogue number in the *Henry Draper Catalogue* and the *Bonner Durchmusterung* as well as in the *Bright Star Catalogue*. The latter records it as 6^m.44, K2 III, and notes its radial velocity as “- 3SB”. Such evidence as there may have been for its designation as a spectroscopic binary appears to be from three spectra⁶, all taken in the early months of 1932 at the Mount Wilson 60-inch, which all gave velocities that were marginally on the positive side of zero, whereas all other measurements have been near -6. HR 4242 has been observed occasionally at Cambridge (not *always* by mistake!), with the results shown in Table II; the mean is -6.53 ± 0.10 km s⁻¹.

TABLE II
Radial velocities of HR 4242

Date (UT)	RV (km s ⁻¹)
2004 Dec. 27.19	-6.5
2005 May 2.91	-6.4
4.93	-6.3
2007 July 6.90	-7.0
2009 Apr. 19.90	-6.5
2010 July 4.93	-6.5

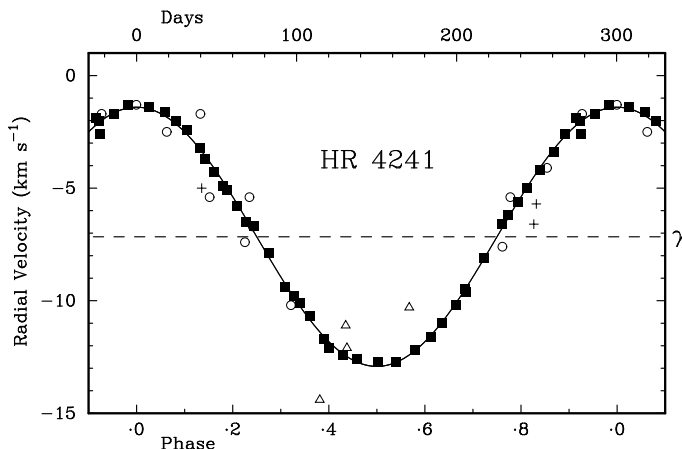


FIG. 1

The observed radial velocities of HR 4241 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The Cambridge observations upon which the orbit largely depends are plotted as filled squares. Open circles denote Ames photoelectric measurements⁷, which were attributed a weight of at most 0.05 (see text) in the solution of the orbit. Mount Wilson^{3,6} and DAO⁵ photographic velocities, given weight 0.01, appear as pluses and open triangles, respectively.

HR 7208 (HD 176981)

HR 7208 is a sixth-magnitude star in Aquila, about $5\frac{1}{2}^\circ$ south of ζ Aql; it is in almost the same declination as Altair, which lies the best part of an hour of RA to the east. It has been well observed by photometrists^{12–15} who (with the exception of Moreno¹⁵ whose paper has indications of referring to idiosyncratic scales of colour indices) have given accordant results close to $V = 6^m.29$, $(B - V) = 1^m.62$, $(U - B) = 1^m.83$. The star is so exceptionally red that it is a surprise to see its type given in the *Bright Star Catalogue* as K2 III. The writer has not been able to find such (or any MK) classification in the literature, and the ugly possibility presents itself that the *Catalogue* entry is nothing more than just the *Henry Draper* type (K2) with a luminosity class somehow assigned to it, possibly on the grounds that no K dwarf could be *that* red. Luminosity class III is actually correct: the *Hipparcos* parallax yields a distance modulus of $7^m.08$ with an uncertainty of about $0^m.4$, so the absolute magnitude is about $-0^m.8 \pm 0^m.4$. The colour indices, of which several have been measured¹⁶ far out into the infrared, are redder than those of most stars that have genuinely been classified on the MK system as K5 III, although there are a few (HR 79, θ Cnc, HR 6154, 28 Sgr) that are redder; HR 7208 is, however, not quite as red as most Mo III stars, although it is redder than some, for example 10 And. Thus, at any rate according to its colour, some type such as K5 III or K7 III (if such is admissible) would best suit it.

The star features in an early paper by Nassau & Blanco¹⁷ on ‘Bright late M-type stars near the Galactic equator’, but unless one reads their paper very superficially one sees that HR 7208 is not listed there as a ‘bright late M-type star’ itself, but as one of the nearest *BD*¹⁸ stars to one that *is*; the offsets of the star concerned (classified as M7) from HR 7208, measured in millimetres on the *BD* chart, are given. Then if one recalls that the scale of the charts is 20 mm

to the degree, the actual position of the M7 star can readily be estimated. Unfortunately, Akyol *et al.*¹⁹ have misidentified Nassau & Blanco's M7 star as HR 7208 itself. Naturally enough the very red star was picked up in other infrared surveys, including the *IRC*²⁰, in which it is no. 10395, and *IRAS*²¹ (18589+0815). *Simbad*, however, does not recognize the identity of IRC 10395 and IRAS 18589+0815 and has separate entries for them, each with three papers in its bibliography but with none of them in common. But we digress! HR 7208 has its own entries in those infrared catalogues, as IRC 10398 and IRAS 18599+0817.

The radial velocity of HR 7208 was first measured from plates taken with a one-prism spectrograph giving 33 \AA mm^{-1} at H γ on the 74-inch reflector of the David Dunlap Observatory; the observations were made in the course of a joint project of the staff there, and published²² under the name of the Director, Young, in 1945. The mean of four measurements was given as $-7.7 \pm 1.4 \text{ km s}^{-1}$; a flag drew attention to a note that the velocity was "more uncertain than for the general run of stars", and that there was a range of 11 km s^{-1} between the individual velocities. There the matter rested for more than fifty years, until de Medeiros & Mayor²³ published a result from the OHP *Coravel* in 1999; they gave a mean of $-8.26 \pm 2.77 \text{ km s}^{-1}$ from just two observations, so the same expression could be expected to give the two velocities individually. The projected rotational velocity of HR 7208 was also listed, as $1.8 \pm 1.5 \text{ km s}^{-1}$. Some years later the two radial velocities were in fact made available, with dates, through the CDS; although they differed by 5.52 km s^{-1} — very nearly the expected amount — their mean had changed to -7.65 km s^{-1} . They have been recorded at the head of Table III here. Meanwhile, de Medeiros, da Silva & Maia²⁴ had re-published exactly the same information as de Medeiros & Mayor²³, except for omitting the actual mean velocity.

Radial-velocity measurements were begun in Cambridge in 2005, as for HR 4241, and now number 39. They are listed in Table III, whose data readily give the orbit that is plotted in Fig. 2; its elements are given later in the paper, with those of the other three stars treated here, in Table VI. The two OHP velocities have been given the same weight as the Cambridge ones. If it seemed extraordinary that the 300-day orbit of HR 4241 proved to be circular, it must be even more so for the 611-day one of HR 7208 to be the same. Yet the sum of the squares of the residuals falls only from 2.77 to $2.51 \text{ (km s}^{-1}\text{)}^2$ when the condition $e \equiv 0$ is relaxed. The 2.51 represents the cost of 35 degrees of freedom (41 observations solved for six unknowns), so they cost about 0.072 each; the two degrees represented by e and ω cost $0.26 \text{ (km s}^{-1}\text{)}^2$ between them, or 0.13 each. The variance ratio $0.13/0.072$, about 1.8 , represents $F_{2,35}$, for which even the 10% point is 2.46 , so no significance can be attributed to the eccentricity.

HR 8026 (HD 199612)

About $4\frac{1}{2}^\circ$ north-following Deneb is the 6^m star HR 8026. Its V and $(B - V)$ magnitudes have been measured by Bouigue *et al.*²⁵ and Ljunggren & Oja²⁶, and those plus $(U - B)$ have been given by Argue¹³ and Apenzeller²⁷; the mean results are $V = 5^m.90$, $(B - V) = 1^m.03$, $(U - B) = 0^m.92$. The *HD* type is Ko, and the same was given by Adams *et al.*²⁸ in their large paper published in 1935 and listing spectroscopic luminosities, which for HR 8026 they found to be $+0^m.3$; previously a type of G9 had been given²⁹ from Mount Wilson. Bidelman³⁰ was the first to give an MK spectral type, G8 II–III. Bouigue *et al.*²⁵ proposed a type of G8 IV from objective-prism spectra, but said (in French) that it was approximate and should be regarded as provisional. Fehrenbach &

TABLE III
Radial-velocity observations of HR 7208

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

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O-C) km s ⁻¹
1987 Aug. 13·83*	47020·83	-4·1	11·869	-0·2
1988 June 4·03*	47316·03	-9·6	10·352	-0·3
2005 June 1·07	53522·07	-11·1	0·505	0·0
Aug. 22·01	604·01	-9·5	·639	0·0
Sept. 16·94	629·94	-8·7	·681	-0·1
Oct. 27·76	670·76	-6·6	·748	+0·2
Nov. 19·76	693·76	-5·8	·786	0·0
Dec. 11·70	715·70	-4·8	·822	+0·1
2006 Mar. 2·24	53796·24	-3·2	0·953	-0·5
Apr. 9·18	834·18	-2·7	1·016	-0·2
May 11·14	866·14	-2·7	·068	+0·2
June 23·08	909·08	-3·7	·138	+0·3
July 18·02	934·02	-4·9	·179	0·0
Aug. 8·04	955·04	-5·9	·213	-0·1
28·95	975·95	-6·7	·247	0·0
Sept. 19·90	997·90	-7·7	·283	0·0
Oct. 24·82	54032·82	-8·7	·341	+0·4
Nov. 9·76	048·76	-9·2	·367	+0·4
23·70	062·70	-10·1	·389	0·0
Dec. 9·70	078·70	-10·9	·416	-0·4
17·70	086·70	-10·4	·429	+0·2
2007 Apr. 12·17	54202·17	-9·8	1·618	+0·1
May 1·13	221·13	-10·2	·649	-0·9
19·12	239·12	-8·3	·678	+0·3
June 1·10	252·10	-7·9	·699	+0·2
28·06	279·06	-6·5	·743	+0·4
July 13·02	294·02	-5·9	·768	+0·4
Aug. 3·02	315·02	-5·6	·802	-0·2
30·99	342·99	-4·2	·848	+0·1
Sept. 22·91	365·91	-3·5	·885	0·0
Oct. 13·86	386·86	-3·0	·920	0·0
29·77	402·77	-2·8	·946	-0·1
Nov. 16·77	420·77	-2·5	·975	0·0
Dec. 15·70	449·70	-2·6	2·023	-0·1
2008 Aug. 30·92	54708·92	-10·8	2·447	0·0
Sept. 18·89	727·89	-11·0	·478	0·0
Oct. 27·81	766·81	-11·0	·541	-0·1
Nov. 18·75	788·75	-10·9	·577	-0·3
Dec. 2·73	802·73	-10·3	·600	-0·1
2009 Oct. 8·82	55112·82	-3·4	3·107	0·0
2010 Oct. 27·81	55496·81	-7·0	3·736	+0·2

*OHP measure²³ from CDS — see text.

Rebeiro³¹, from the same or similar material, put the type at G8 III. Boulon³², in what looks as if it is a Ph.D. thesis although it seems not to say so, gave it as Ko III. The parallax³³ is small and indicates an absolute magnitude as bright as -2, with an uncertainty of about half a magnitude, so Bidelman's recognition

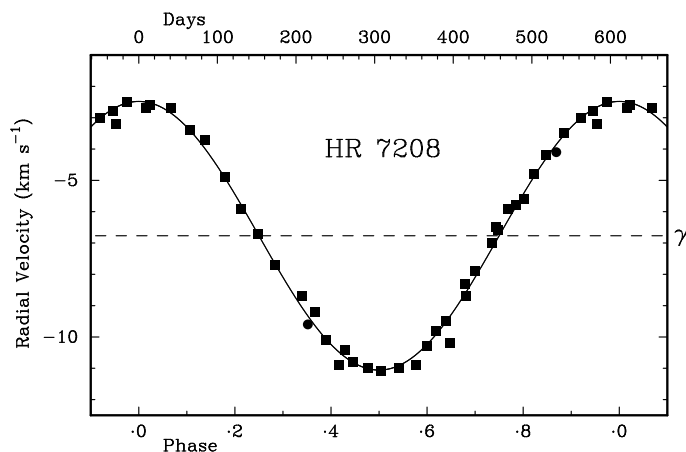


FIG. 2

As Fig. 1, but for HR 7208. The only addition to the Cambridge observations here are two measures from the OHP *Coravel*²³, retrieved from the CDS; they are plotted as filled circles and were given the same weight as the Cambridge data.

of the luminosity as being somewhat above that of a normal giant is vindicated. Boulon³⁴ proposed the star's distance to be 100 pc, Appenzeller²⁷ suggested 140 pc, but the parallax shows it to be more like 400 pc.

The radial velocity of HR 8026 was first measured²⁹ at Mount Wilson in 1921, from four plates taken at the 60-inch reflector at monthly intervals. The relevant volume of the *Henry Draper Catalogue* (*Harvard Annals*, vol. 98) had not then been published; the star was identified as Lalande³⁵ 40627. The mean result was given as -14.2 km s^{-1} with a 'probable error' of 1.8 km s^{-1} . Fifty years later the individual dates and velocities were published by Abt³⁶; modern arithmetic appears to produce a mean of -13.9 and a somewhat smaller 'probable error' than was found by the original authors. Fehrenbach & Rebeiro³¹ gave a mean velocity, from four plates obtained by their objective-prism method, of -36 km s^{-1} ; more than 30 years later Fehrenbach³⁷ listed a correction of $+3 \text{ km s}^{-1}$ for all the stars in the field containing HR 8026. Boulon³² gave the velocity as -36 km s^{-1} again, from a slit spectrogram obtained at the OHP 120-cm telescope at a dispersion of 77 Å mm^{-1} at $H\gamma$. None of the OHP velocities is dated, but Fehrenbach & Rebeiro³¹ noted that their velocities in the relevant field ('Champ 7') depended on five plates, of which four were taken in 1952 August and one in 1954 August, so at least three of their velocity measurements must refer to the former. Boulon's work appears to have extended over the period 1956–60.

Beavers & Eitter⁷ published a series of 19 velocities obtained at Ames for HR 8026 in 1976–83, and noted that they showed variation — indeed, they ranged over 19 km s^{-1} . Beavers⁹, however, listed the object in a table of only 'probable variable velocity stars' (*my italics*), though with a note saying " $K > 11$, slow". Five velocities, which demonstrated substantial variability, were reported as a mean from the OHP *Coravel* by de Medeiros & Mayor²³. Considerably later the five measurements were made available individually in a listing deposited with the CDS, which shows that three of them were all made in one week and

one of the others is 'wild' (it has a residual of about 20 km s^{-1} from the orbit), so there are really only two epochs validly represented. The star was placed on the Cambridge observing programme (probably in a belated response to the Ames discovery of its binary nature) in 1993, a time when the programme was dependent on the OHP *Coravel*; 15 measurements were made with that instrument, and 56 have been made at Cambridge since the *Coravel* there became operational. In 1999, when neither *Coravel* was available, the DAO stepped into the breach and kindly offered observing time on the spectrometer³⁸ at the 48-inch coude, where two measurements of HR 8026 were made. All the available data, amounting to a total of 101 observations, are listed in Table IV.

The orbit depends very largely on the 76 velocities obtained with the two *Coravels*, which have been weighted equally in the solution apart from the 'wild' OHP measure which has of course been rejected. The two DAO velocities, too, have been entered with full weight. The Ames data have also been included, with weightings of 0.05 for quality A and half that for quality B. Two of the B-quality ones give residuals that are very much larger than any of the others or are at all typical of Ames observations, and have been rejected. The orbital period given by the writer's own observations, which barely cover a complete cycle, is 6464 ± 61 days; it is greatly refined by the Ames and OHP measures from the preceding cycle — some of them are two cycles old now — to 6328 ± 14 days, about 17.3 years. The change of just over two standard deviations to the Cambridge period is a bit uncomfortable and may well suggest that the standard errors of all the elements in the final orbit are optimistic — but that could be seen as part of the price to be paid for the use of heterogeneous data, as becomes increasingly unavoidable as longer and longer orbital periods are to be established. The orbit is illustrated in Fig. 3, and its elements have been included in Table VI below.

The Mount Wilson observations²⁹ dating from 1921 are very ragged and have not been included in the solution of the orbit; two of them fall off the top of Fig. 3. The mean of the OHP objective-prism velocities³¹, even after their belated correction³⁷, is too low by 8 km s^{-1} for those obtained in 1952 and 13 km s^{-1} if there is one that depends on the 1954 plate. Such errors are considerable in relation to the claimed 'probable error' of less than 2.5 km s^{-1} . Boulon's single OHP slit spectrogram³² must have a residual of nearly -20 km s^{-1} , some 15 times his claimed 'probable error', depending relatively little on its exact (unknown) date because for the duration of the relevant observing programme the actual velocity of HR 8026 was changing only slowly.

A significant feature of the orbital elements of HR 8026 is the large value of the mass function. It shows that, if the G8 II–III primary star were supposed to have a mass of $2 M_{\odot}$, the secondary would need to be at least $1.8 M_{\odot}$. If the primary were more massive, so would the secondary need to be to satisfy the mass function: for example, if the primary were $4 M_{\odot}$, then the minimum for the secondary would be $2.6 M_{\odot}$. If the secondary were another evolved star with a mass in such a range, the system could be expected to appear double-lined; otherwise, if the secondary were a main-sequence star it would have to be at least of type A, and the colour indices of the system — if not indeed its spectrum — could not fail to demonstrate its composite nature. Not for the first time in these pages it seems necessary to postulate, therefore, that the secretive companion is itself a binary system, whereby it could consist of stars that jointly possess the necessary mass without at the same time having anywhere near the luminosity that would be associated with such a mass in a single star. A pair of solar-type stars, for example, would be something like six magnitudes fainter than the primary in V , and the difference would still be more than four magnitudes in the ultraviolet.

TABLE IV
Radial-velocity observations of HR 8026

Except as noted, the sources of the observations are as follows:
1976–1983 — *Fick Obs.*¹ (weight 0.05); 1993–1995 — *OHP Coravel* (weight 1)
1996–2011 — *Cambridge Coravel* (weight 1)

<i>Date (UT)</i>	<i>MJD</i>	<i>Velocity km s⁻¹</i>	<i>Phase</i>	<i>(O – C) km s⁻¹</i>
1921 Aug. 22.40*	22923.40	–17.9	0.345	–0.1
Sept. 12.23*	944.23	–8.9	.348	+8.9
Oct. 10.20*	972.20	–10.2	.353	+7.6
Nov. 11.13*	23004.13	–15.3	.358	+2.5
1976 July 13.33	42972.33	–18.5:	3.513	–0.2
Aug. 3.33 ^R	993.33	–14.6:	.516	+3.7
1977 July 22.30	43346.30	–20.0:	3.572	–0.8
Aug. 11.97 [†]	366.97	–19.6	.575	–0.4
12.95 [†]	367.95	–19.2	.576	0.0
18.89 [†]	373.89	–19.4	.576	–0.2
25.24	380.24	–18.1:	.577	+1.2
Sept. 23.18 ^R	409.18	–14.6:	.582	+4.8
Oct. 14.17	430.17	–18.4:	.585	+1.0
1978 July 27.30	43716.30	–22.6	3.631	–2.2
Aug. 14.23	734.23	–21.4:	.633	–0.9
24.21	744.21	–21.4:	.635	–0.9
Nov. 8.07	820.07	–21.7	.647	–0.9
1980 Aug. 22.20	44473.20	–24.0	3.750	+0.4
1982 July 13.36	45163.36	–31.1:	3.859	–0.8
Aug. 12.26	193.26	–31.5:	.864	–0.9
18.23	199.23	–30.2:	.865	+0.4
1983 July 22.33	45537.33	–33.8	3.918	0.0
26.30	541.30	–32.5:	.919	+1.3
Aug. 26.20	572.20	–34.1	.924	0.0
Sept. 7.18	584.18	–32.3	.926	+1.9
11.24	588.24	–33.4:	.926	+0.8
1986 Aug. 17.96 [†]	46659.96	–28.0	4.096	–0.3
1987 Aug. 9.13 ^{†R}	47016.13	–3.7	4.152	+19.9
1993 Feb. 11.75	49029.75	–18.0	4.470	–0.1
Mar. 23.17	069.17	–18.1	.476	–0.2
July 9.06	177.06	–18.0	.493	+0.1
Sept. 12.00	242.00	–18.3	.504	–0.1
13.88	243.88	–18.0	.504	+0.2
Dec. 24.75	345.75	–18.1	.520	+0.3
1994 Aug. 2.02	49566.02	–18.8	4.555	+0.1
Dec. 10.76	696.76	–19.4	.576	–0.2
1995 Jan. 1.76	49718.76	–19.7	4.579	–0.4
June 6.10	874.10	–19.9	.604	–0.1
Dec. 31.76	50082.76	–20.7	.637	–0.1
1996 Nov. 15.76	50402.76	–22.0	4.687	0.0
Dec. 17.78 [‡]	434.78	–22.5	.692	–0.3

TABLE IV (continued)

<i>Date (UT)</i>	<i>MJD</i>	<i>Velocity km s⁻¹</i>	<i>Phase</i>	<i>(O - C) km s⁻¹</i>
1997 Apr. 1·20	50539·20	-22·7	4·709	+0·1
June 17·10	616·10	-22·8	·721	+0·4
Sept. 8·99 [‡]	699·99	-23·7	·734	0·0
Dec. 21·80 [‡]	803·80	-24·5	·751	-0·1
1998 July 8·06 [‡]	51002·06	-26·0	4·782	-0·1
1999 July 9·40 [§]	51368·40	-29·3	4·840	-0·2
Nov. 5·21 [§]	487·21	-30·1	·859	+0·1
Dec. 28·77	540·77	-30·5	·867	+0·3
2000 Jan. 8·75	51551·75	-30·2	4·869	+0·7
Apr. 10·17	644·17	-32·0	·883	-0·2
July 19·06	744·06	-33·0	·899	-0·3
Sept. 16·97	803·97	-33·3	·909	0·0
Nov. 13·85	861·85	-33·7	·918	+0·1
2001 Jan. 7·76	51916·76	-34·0	4·926	+0·2
Mar. 3·24	971·24	-35·4	·935	-0·8
June 8·10	52068·10	-35·1	·950	0·0
Aug. 15·01	136·01	-35·3	·961	0·0
Oct. 18·98	200·98	-35·2	·971	+0·2
Dec. 1·83	244·83	-35·3	·978	+0·1
2002 Mar. 28·17	52361·17	-35·2	4·997	-0·2
May 27·07	421·07	-34·7	5·006	-0·1
July 14·06	469·06	-34·2	·014	+0·1
Sept. 9·02	526·02	-33·6	·023	+0·1
Nov. 7·89	585·89	-33·2	·032	-0·1
2003 Jan. 11·73	52650·73	-32·1	5·042	+0·2
May 24·12	783·12	-30·5	·063	0·0
July 15·06	835·06	-29·6	·071	+0·2
Sept. 17·90	899·90	-28·9	·082	0·0
Nov. 27·86	970·86	-27·8	·093	+0·2
2004 May 23·10	53148·10	-26·0	5·121	-0·3
Aug. 13·05	230·05	-25·0	·134	-0·2
Oct. 25·88	303·88	-24·0	·146	0·0
Dec. 16·74	355·74	-23·4	·154	+0·1
2005 May 28·07	53518·07	-22·1	5·179	0·0
July 20·09	571·09	-21·4	·188	+0·3
Sept. 12·00	625·00	-21·3	·196	0·0
Nov. 9·85	683·85	-20·9	·206	0·0
2006 May 30·12	53885·12	-20·2	5·237	-0·4
July 4·09	920·09	-19·4	·243	+0·2
Sept. 8·03	986·03	-19·2	·253	+0·1
Nov. 25·80	54064·80	-18·9	·266	+0·1
2007 Jan. 15·71	54115·71	-18·7	5·274	+0·1
May 31·10	251·10	-18·7	·295	-0·3
July 25·07	306·07	-18·3	·304	0·0
Sept. 29·96	372·96	-18·2	·315	0·0
2008 Jan. 7·78	54472·78	-18·1	5·330	-0·1
July 4·11	651·11	-17·7	·358	+0·1
Sept. 18·95	727·95	-17·7	·371	0·0
Nov. 25·86	795·86	-17·8	·381	-0·1

TABLE IV (concluded)

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O - C) km s ⁻¹
2009 Jan. 20.77	54851.77	-17.4	5.390	+0.2
July 2.10	55014.10	-17.6	.416	0.0
Sept. 4.00	078.00	-17.7	.426	0.0
Nov. 7.87	142.87	-17.6	.436	+0.1
2010 Jan. 3.73	55199.73	-17.6	5.445	+0.1
July 18.06	395.06	-17.9	.476	0.0
Sept. 15.00	454.00	-17.9	.485	+0.1
Nov. 11.84	511.84	-18.0	.494	+0.1
2011 Jan. 18.75	55579.75	-18.1	5.505	+0.1
June 27.11	739.11	-18.1	.530	+0.4
Aug. 8.05	781.05	-18.4	.537	+0.2

* Mount Wilson observation²⁹; weight 0.
† OHP measure²³ from CDS — see text; wt.1.
‡ Observed with Haute-Provence *Coravel*; wt.1.
§ Observed with DAO 48-inch telescope; wt.1.
R Rejected observation.

The minimum length of the semi-major axis of the primary orbit, $a_1 \sin i$, is almost 5 AU, so we can confidently expect the separation of the components in the 17-year orbit to be at least 10 AU, which at the ~400-pc distance of the system would subtend an angle of 0".025. HR 8026 is therefore not an encouraging object for speckle interferometry unless larger telescopes than have usually been used for that purpose can be brought to bear upon it.

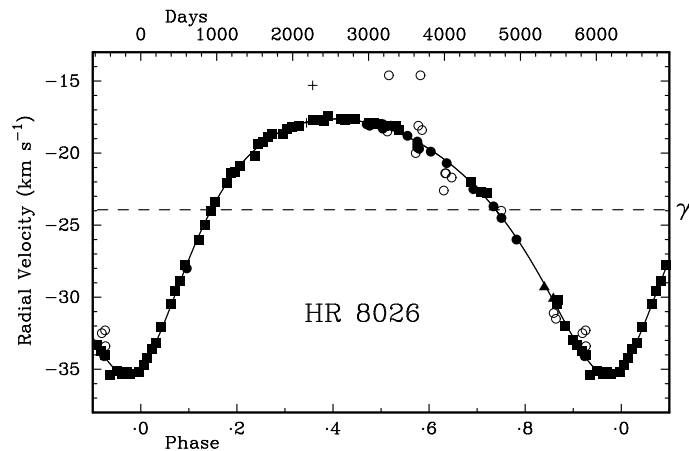


FIG. 3

As Figs. 1 and 2, but for HR 8026. The symbols have the same meanings as before, but, of the 20 OHP observations, 15 were made by the writer and five were retrieved from the CDS. There are in addition two velocities obtained by the writer with the DAO spectrometer³⁸, plotted as filled triangles (to be found near phase .85) and attributed full weight. The Ames velocities were weighted at most 0.05, as before, but in this instance the Mount Wilson ones were zero-weighted; two of them are off the top of the diagram.

HR 8149 (HD 202951)

HR 8149 is to be found in Equuleus — a distinction with a particularly small probability of achievement, since Equuleus is the smallest of all the constellations with the sole exception of Crux, and covers less than one five-hundredth part of the sky; our object lies about 100' north-following the $4\frac{1}{2}^m$ δ Equ. The V magnitude and $(B - V)$ colour index have been given by Häggkvist & Oja³⁹ as $5^m.96$ and $1^m.65$; the star has long been considered to have a spectral type of K5 III, which appears in the *Bright Star Catalogue*, but the source of that classification is not apparent, so there looms the unfortunate possibility that it was just the *HD* type of K5 with the luminosity class of a giant tagged onto it. All the same, Henry, Fekel & Hall⁴⁰ subsequently gave exactly that classification. The parallax³³ is small and yields a rather uncertain distance close to 400 pc; it corresponds to a distance modulus of $7^m.92$ that leads to an absolute magnitude of about $-2.0_{+0.6}^{-0.8}$.

Eggen⁴¹ included HR 8149 in a table of 'probable members of the Hyades group', which required it to have a distance modulus of $6^m.60$ and a distance of 208 pc; subsequent DDO photometry by Boyle & McClure⁴² slightly altered the absolute magnitude needed for the star's membership in the group to be $-0^m.44$ — so its membership seems unlikely, and even Eggen never referred to it again. Eggen did, however, include it in a paper⁴³ on 'Asymptotic giant branch stars near the Sun'. It first appears there in his Table 3, entitled "Photometry and apparent motions of stars in Table 1" — but HR 8149 does not feature in Table 1. It is a familiar feature (*cf.* ref. 44) of such papers that objects change identity and properties between one table and another, and it seems possible that HR 8149 may have been conflated with the 'HR 8145' that appears in Table 1 but not in Table 3. In Table 9 of that paper, HR 8149 is attributed a distance modulus of $7^m.2$.

Henry, Fekel & Hall⁴⁰, in the course of a search for photometric variability in chromospherically active stars, stumbled upon such variability in HR 8149 when they innocently adopted it as a comparison star for the active object HD 202908. At one epoch it showed changes to which they assigned a period of 21.6 ± 0.5 days, with a V amplitude of $0^m.03$, which though small absolutely is convincingly shown by their plot to be well above their measuring errors. They considered the photometric period to be the rotational period, and then, having found a rotational velocity of 4 ± 2 km s⁻¹ from two spectra, they obtained a lower limit of $1.7 R_{\odot}$ to the stellar radius. One of those⁴⁰ authors, Fekel, later gave⁴⁵ the rotational velocity as 4.0 km s⁻¹; it is not apparent that it is a different determination that warrants the greatly increased implied precision. Koen & Eyer⁴⁶ reckoned that they found a periodicity of 0.47645 cycles per day, with an amplitude of $0^m.0115$, in the *Hipparcos* 'epoch photometry', but (not for the first time; *cf.* refs. 47 and 48, to mention only papers in this series subsequent to no. 200) the writer suspects that they may have tried to sail too close to the wind and to have proposed mere mathematical artefacts as representing physical realities. Be that as it may, none of the evidence for photometric changes seems to have been persuasive enough to impel the naming authorities to grant HR 8149 a variable-star designation.

The first radial-velocity measurements of HR 8149 (like those of HR 7208) were made at the DDO and published by Young²²; he listed a mean velocity of -35.5 km s⁻¹ with a 'probable error' of 0.9 km s⁻¹. Again as for HR 7208, there was a flag, "more uncertain than for the general run of stars", and it noted

a range of 9 km s^{-1} between the individual velocities. The spectral type was listed as K6. The *Radial Velocity Catalogue*⁴⁹, in its *Introduction*, has a little table (Table 3), giving systematic corrections that had been found by the compiler to be appropriate to the velocities from the 11 observatories that had been the most prolific sources of the material being compiled. The DDO was one of them. The corrections are different for different spectral types, and for the DDO they are -1.0 km s^{-1} for K stars and -1.5 km s^{-1} for M types. Evidently HR 8149 was treated as an M star, because the DDO measurement, which was the only one existing, was reported as -37.0 km s^{-1} ; that is the value that has been widely quoted ever since and is *still* the one that is found by anyone who relies on *Simbad* for information.

The two spectra that were taken by Henry, Fekel & Hall⁴⁰ were reported as showing “nearly identical radial velocities” whose mean was -36.6 km s^{-1} . They commented, “Such a value is in excellent agreement with the average velocity found by Young (1945), supporting the view that its velocity is constant.” Fekel⁴⁵, too, gave an entry of “C:”, where C meant ‘Constant’, in a column headed ‘Duplicity’ in one of his tables. Analogously, in 2000 Smith & Shetrone⁵⁰ had an entry “Single” against HR 8149 in a table column headed ‘Binarity’*. They, with another collaborator, still listed the radial velocity of HR 8149 as -37.0 km s^{-1} in a paper⁵¹ in 2009.

De Medeiros & Mayor²³, however, reported in 1999 just two velocities obtained with the OHP *Coravel*, that had a mean of $-38.71 \pm 3.62 \text{ km s}^{-1}$, showed an apparent standard error per observation of 5.12 km s^{-1} (of course! — just $\sqrt{2} \times 3.62$), and a $v \sin i$ of $4.4 \pm 1.0 \text{ km s}^{-1}$. As in so many other cases (including that of HR 7208), de Medeiros, da Silva & Maia repeated exactly the same information, apart from omitting the actual mean radial velocity, in a subsequent publication²⁴ in a different journal.

In 2002 de Medeiros & Mayor deposited with the CDS a table of the individual velocities, with dates, that their published paper listed only as a mean; HR 8149 was one of the stars selected in the light of that table for observation at Cambridge. That work started immediately, and there are now 47 measurements to add to the two from OHP; all are set out in Table V and have been weighted equally in the orbital solution. They yield the orbit that is plotted in Fig. 4 and whose elements are given here in the last column of Table VI.

It may be significant that the velocity residuals of the two very red stars, HR 7208 and 8149, are larger than those of the other two; however, the two red ones are at low declinations, whereas the other two pass close to the Cambridge zenith, so it is also possible that the reason for the differences in residuals should be sought nearer home. That said, the existence in HR 8149, at least sometimes, of photometric changes that may well arise from star-spots indicates a potent mechanism for producing minor radial-velocity jitter. The $v \sin i$ values derived from the Cambridge radial-velocity traces of HR 8149 have a mean of $5.0 \pm 0.1 \text{ km s}^{-1}$, the r.m.s spread of the 47 individual values being less than 0.7 km s^{-1} . Owing to the neglect of differences between stars in respect of other sources of line-broadening, the standard error of the mean should not be taken at face value. The mean, however, does agree quite well with the 4 km s^{-1} given by Fekel⁴⁵ and the 4.4 given by de Medeiros & Mayor²³.

*Not a word to be found in the English dictionary, but evidently imported from France, in a rare reversal of the usual direction of spread of language.

TABLE V
Radial-velocity observations of HR 8149

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

Date (UT)		MJD	Velocity km s ⁻¹	Phase	(O-C) km s ⁻¹
1988	July 28.05*	47370.05	-41.0	5.597	0.0
1989	Aug. 2.01*	47740.01	-33.8	5.968	-0.4
2002	July 15.08	52470.08	-39.5	0.699	+0.3
	Sept. 1.99	518.99	-38.9	.748	0.0
	Oct. 4.92	551.92	-38.3	.780	-0.1
2003	Jan. 10.73	52649.73	-35.3	0.878	+0.1
	June 21.10	811.10	-33.3	1.040	+0.3
	July 15.06	835.06	-33.5	.064	+0.7
	Aug. 16.07	867.07	-34.6	.096	+0.5
	Sept. 14.97	896.97	-35.7	.126	+0.3
	Oct. 17.92	929.92	-36.9	.159	+0.1
2004	Jan. 9.73	53013.73	-39.4	1.242	-0.3
	July 5.11	191.11	-41.2	.420	+0.1
	Aug. 8.11	225.11	-41.2	.454	+0.2
	Sept. 1.03	249.03	-41.2	.478	+0.2
		25.95	-41.0	.503	+0.4
	Oct. 21.92	299.92	-40.8	.529	+0.5
	Nov. 26.80	335.80	-41.0	.565	+0.2
	Dec. 26.73	365.73	-40.8	.594	+0.2
2005	May 23.12	53513.12	-39.1	1.742	-0.1
	June 28.09	549.09	-38.3	.778	-0.1
	July 29.05	580.05	-37.1	.809	+0.3
	Aug. 22.04	604.04	-36.7	.833	+0.1
	Sept. 14.97	627.97	-35.6	.857	+0.5
	Oct. 25.91	668.91	-34.5	.898	+0.4
	Dec. 8.78	712.78	-33.2	.942	+0.6
2006	Aug. 10.07	53957.07	-38.0	2.186	-0.2
	Sept. 8.04	986.04	-38.8	.215	-0.3
	Oct. 24.93	54032.93	-40.3	.262	-0.8
	Nov. 23.80	062.80	-40.1	.292	-0.1
2007	Oct. 4.95	54377.95	-41.0	2.607	-0.1
	23.92	396.92	-41.1	.626	-0.4
	Nov. 23.86	427.86	-40.4	.657	0.0
2008	July 4.12	54651.12	-35.1	2.880	+0.3
	Aug. 4.04	682.04	-34.4	.911	+0.1
	Sept. 26.94	735.94	-33.8	.965	-0.4
	Oct. 22.88	761.88	-33.8	.991	-0.6
	Dec. 26.73	826.73	-34.5	3.056	-0.5
2009	Aug. 30.02	55073.02	-39.8	3.302	+0.4
	Oct. 8.92	112.92	-40.4	.342	+0.3
	Nov. 7.86	142.86	-41.1	.372	-0.1
2010	July 18.07	55395.07	-41.0	3.624	-0.3
	Aug. 19.10	427.10	-40.9	.656	-0.5
	Sept. 15.94	454.94	-40.2	.684	-0.2
	Oct. 11.95	480.95	-39.8	.710	-0.2
	Nov. 11.85	511.85	-39.3	.741	-0.3

TABLE V (concluded)

Date (UT)	MJD	Velocity km s ⁻¹	Phase	(O-C) km s ⁻¹
2011 Jan. 14.72	55575.72	-37.9	3.805	-0.4
May 13.14	694.14	-34.6	.923	-0.4
Aug. 8.06	781.06	-33.4	4.010	-0.1

*OHP measure²³ from CDS — see text.

TABLE VI

Orbital elements for the four HR stars

Element	HR 4241	HR 7208	HR 8026	HR 8149
<i>P</i> (days)	300.33 ± 0.06	611.2 ± 0.5	6328 ± 14	999.8 ± 2.4
<i>T</i> (MJD)	54087.99 ± 0.30	53824.7 ± 1.4	52383 ± 12	53771 ± 12
<i>γ</i> (km s ⁻¹)	-7.16 ± 0.03	-6.77 ± 0.04	-23.93 ± 0.03	-38.24 ± 0.05
<i>K</i> ₁ (km s ⁻¹)	5.76 ± 0.04	4.29 ± 0.06	8.89 ± 0.04	4.09 ± 0.08
<i>e</i>	0	0	0.309 ± 0.004	0.229 ± 0.018
<i>ω</i> (degrees)	—	-199.7	±0.8	3 ± 5
<i>a</i> ₁ sin <i>i</i> (Gm)	23.77 ± 0.17	36.0 ± 0.5	736 ± 4	54.8 ± 1.1
<i>f</i> (<i>m</i>) (<i>M</i> _⊙)	0.00595 ± 0.00012	0.00500 ± 0.00022	0.397 ± 0.005	0.0066 ± 0.0004
R.m.s. residual (wt. 1) (km s ⁻¹)	0.17	0.26	0.21	0.33

It has been proposed that an equatorial rotational velocity of 5 km s⁻¹ is enough to encourage star-spots — and we know of a local example where spottedness arises with a lower rotational velocity than that. At the same time, it is difficult to accept Henry *et al.*'s assignment of the (very temporary) 21.5-day photometric period as the period of rotation. A star of the type and high luminosity of HR 8149 must be expected to have a radius of about 30 *R*_⊙, so rotation in 21.5 days would require *v*_{equ} ~ 70 km s⁻¹ — highly unusual for

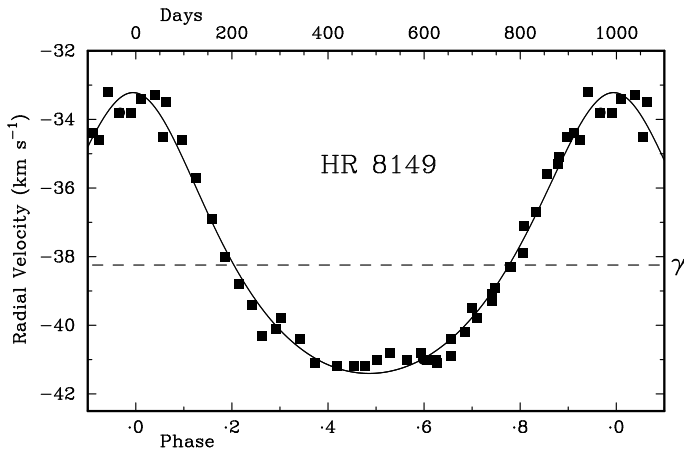


FIG. 4
Exactly as Fig. 2, but for HR 8149.

a late-type star. For $v \sin i$ to be only 5 km s^{-1} would evidently imply that $\sin i \sim 1/4$. That too is very unlikely, since it means that the rotational axis would have to be directed only $1/4$ of a radian, *i.e.*, 4° , from our line of sight to the star — a situation with an *a priori* probability of $(1 - \cos 4^\circ)$ or about 1 in 400.

Erratum in Paper 175

Dr. Omar Kurtanidze has kindly pointed out that in Paper 175⁵² I wrongly attributed to him⁵³ the mis-classification of BD +48° 1048 as a carbon star. *Simbad* had (and still has, at the time of writing this *erratum*) “BD+48 1048 — Carbon Star” in large type as the main heading for its entries for that star. At the time that Paper 175 was written, it had a bibliography consisting of just two papers, the first of which was Dr. Kurtanidze’s one listed here as ref. 53, entitled (in English translation) ‘New carbon stars in the selected Milky Way regions’. That naturally pre-disposed the present writer to imagine that that was the source of the *Simbad* assertion; and although he did look up the paper and see that it listed the *BD* star in a table, he was unable to read the paper, which is written in Russian. In fact, if he had looked at it more carefully, even without being able to read the text, he would easily have been able to divine that it was not the *BD* star but two much fainter ones nearby that were the actual subjects of the classification. Thus the writer fell into exactly the same error, and in exactly the same way, as Akyol *et al.*¹⁹, to whom reference is made in the section on HR 7208 above and to whom is obliquely imputed there the sin of having read the paper by Nassau & Blanco¹⁷ “very superficially”! He hereby apologizes to Dr. Kurtanidze and to readers for the mistake.

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CORRESPONDENCE

To the Editors of 'The Observatory'

*Eponyms, Hubble's Law, and the Three Princes of Parallax**

Just who should get credit for what we generally call Hubble's Law has been disputed since 1929. On balance, and perhaps in disagreement with some other recent discussions, I vote for Hubble.

The interface between observational astronomy and General Relativity seems to have had more than its fair share of disputed huzzas. Best known is the case of the gravitational deflection of starlight, first reported for the 1919 solar eclipse from two British expeditions, headed by Arthur Eddington and by Andrew Crommelin, who got the best data, but got left off the triumphant paper¹, and still hardly ever gets any credit. Neither the 1919 weather nor the telescopes, unused to such rapid temperature changes, were entirely satisfactory,

*Of course neither parallax nor the velocity-distance relation were in any sense "fortunate discoveries by accident". Each was the product of multiple searches, hard work, and improved equipment. But for each there were arguably three claimants for the title of discoverer, one of whom (deservedly, I think) gets most of the credit.

but the results were apparently much closer to the general relativistic prediction of $1''.74$ at the solar limb than to the Newtonian value of half that. Both technical and popular press proclaimed the confirmation. The muttering that Eddington had wrongly weighted the results from the three telescopes became scientifically irrelevant after a few more eclipses, but not, of course, historically or sociologically irrelevant. Stanley anyhow says² it wasn't true. The best solar deflection numbers now come from radio interferometry, and you don't even need to wait for a solar eclipse.

Somewhat similarly, Adams reported³ a value of $+19 \text{ km s}^{-1}$ for the "relativity displacement of the spectral lines" in the companion of Sirius in 1925, just in time for Russell, Dugan & Stewart to write⁴ that it was exactly what everyone was expecting from General Relativity plus the mass and radius of Sirius as then understood. Eddington in particular, who had asked that the measurement be attempted, was pleased as punch. When the best-buy radius of Sirius B crept downward from $0.03 R_{\odot}$ to somewhat less than $0.01 R_{\odot}$, however, there were suggestions that Adams had massaged his data to get what was wanted. Daniel Popper, who measured the first robust gravitational redshift (for 40 Eri B in 1954) denied this⁵ and said the problem was just scattered light from Sirius A.

Attacks on the nomenclature for relativistic results also come along from time to time. "Chandrasekhar did not discover the Chandrasekhar Limit" goes back at least to Shklovsky⁶ in 1978 who credited Frenkel⁷. I got entangled by quoting Shklovsky uncritically in a *Sky & Telescope* book review⁸. Scarcely had the *S&T* issue hit news-stands and mail-boxes when there came a largish envelope from Chandra (my *doktor grossvater*, the teacher of my teacher, Guido Münch). It contained reprints with marginal comments, making clear Chandra's disagreement. I sat down with Frenkel and a German-speaking friend, and concluded that what Frenkel's paper had calculated would have been of interest at the time, but was not the Chandrasekhar Limit. She read the short words and I read the long ones, which seemed to work well. The Chandrasekhar Limit issue has been raised yet again recently in the pages of *Physics Today*, among other places, with the most recent rebuttal that of Wali⁹.

Now in the December issue of *The Observatory*, we find Nussbaumer & Bieri saying that Hubble was not the first to recognize the linear velocity-distance (or redshift-magnitude) relationship that we now call Hubble's Law and so should not have it named for him¹⁰. They attribute the motivation for their discussion to an on-line *Nature* item, for which they do not give enough bibliographical information for me, at least, to find it. They also cite van den Bergh¹¹ and Block¹² with no indication of the papers' contents or where they are scheduled to appear. In fact both (and van den Bergh¹³) say that more credit should be given to Lemaitre. They are not the first to do so. Kragh¹⁴, for instance, says (on p. 58) "It could as well have been named Lemaitre's Law". Shapley¹⁵ was clearly voting for "Shapley's Law"¹⁶ and de Sitter¹⁷ for "Slipher's Constant" and "de Sitter's Law" (or perhaps "my grandfather's law").

Possibly there is some language chauvinism in this. The French-speaking Blanchard¹⁸ is pro-Lemaitre (a French-speaking Belgian); American Christianson¹⁹ is a Hubble person; German-speaking Duerbeck & Seitter^{20,21} favour Wirtz; and, by way of feeble humour in public talks, I have often spoken of "Hubble's Law, so called because it was discovered by Lundmark" who, if not Danish like my grandmother, was at least Scandinavian. But my more serious opinion is that Hubble's Law was, almost from the beginning, the right choice. That opinion was not weakened when Nussbaumer & Bieri¹⁰ took almost none of the advice I had offered under a not-very-anonymous referee's hat.

Eponyms and the division of credit

Stigler's Law²² of 1980 very properly credits to Robert K. Merton (who probably had it from Merlin^{*}) the idea that a scientific entity essentially never carries the name of the first man[†] to put it forward. It is not, of course, considered good form to eponymize oneself. We are reliably informed by William C. Saslaw that Martin Schwarzschild, when backed into a corner to come up with a synonym for "the criterion for convective instability", reluctantly said "my father's criterion". I then naturally asked Feynman what he called "the diagrams". "The diagrams", said he[‡]. Thus we should not be surprised not to find Hubble writing of Hubble's Law or Lemaître writing of Lemaître's Law.

But the phrase "Hubble's Law" appears in print starting in 1933 with Walker²³, himself commemorated in the Robertson–Walker metric. For what it is worth, the preceding paper²⁴ calls a homogeneous universe Friedmann–Lemaître. Humason²⁵ had already used "Hubble's velocity–distance relation" (and is blamed²⁶ by Nussbaumer & Bieri for starting the whole nefarious custom). When Lemaître²⁷ (in the text of a 1933 talk before the US National Academy of Science) wrote "Hubble's ratio", I think the issue should have been regarded as settled.

An earlier case, not involving eponyms, is perhaps instructive. I came to it as this was being written *via* Hirshfeld²⁸, our foremost expert on the discovery of parallax²⁹. He is commenting on an earlier article about Neptune having returned to the orbital position where it was first seen³⁰. The relevant words begin "their example of the measurement of stellar parallax might inadvertently leave readers with the impression that Friedrich Bessel, who measured the parallax of 61 Cygni in 1838, was scooped by both Thomas Henderson and Wilhelm Struve, who had previously measured the parallaxes of Alpha Centauri and Vega respectively." Hirshfeld²⁸ goes on to say, "The issue was analyzed in depth in the pages of *Sky & Telescope* (November and December 1956) by none other than Otto Struve, Wilhelm's great grandson and a frequent contributor to this magazine." Hirshfeld's conclusion was:

"Bessel justly receives credit for the first determination of stellar parallax.

After presenting the case with lawyerly precision, Otto Struve explains 'the most important thing, however, is not which parallax was determined first, but which parallax actually dispelled all doubts of the contemporary astronomers that the long-searched-for effect had finally been found ... I believe it is important to distinguish the result that appeared convincing to the contemporaries of Bessel, Struve, and Henderson, from what we, with more than a century of hindsight, can recognize as the first successful [measurement].' Establishing priority of discovery can be complicated, as we all know. First is not always first."

The case of the triple-alpha reaction, customarily (and I think rightly) credited to Edwin E. Salpeter, though Ernst Öpik considered it earlier, is similar.

One must, of course, not give Hubble too much credit. The *Y* (velocity) axis of 'the diagram' belonged to Slipher, then Humason and Pease, and later many others. But the *X* (distance) axis belonged to Hubble and the 100-inch telescope for a long time. His distances came from Cepheids, and then brightest stars

*Meyer Robert Schkolnick as a teen performed magic tricks at children's birthday parties under the name Robert Merlin. Persuaded that this was tacky, he switched to Robert Merton, and took this as his legal name when he started college at Temple University, in his home town of Philadelphia.

†This is not politically incorrect; I merely want to exempt Noether's Theorem and Leavitt's Law, the proposed new name for the Cepheid period–luminosity relationship.

‡Avoiding one's own eponym is not just an historical problem. At a meeting last week (2011 September 19–22), a very distinguished and gracious colleague worked around the issue by speaking, in his introductory talk, of tZ and kZ, for thermal Zel'dovich and kinetic Zel'dovich.

and whole galaxies calibrated on them. Perhaps one should say “miscalibrated”, since his scale was too small by a factor of 5–10 (somewhat distance-dependent), and it took us all many decades to sort things out and reduce H from about $500 \text{ km s}^{-1} \text{ Mpc}^{-1}$ to 72.37, or whatever your favourite number is.

Every polemic like this has to find a previously obscure hero. I shall pick Arthur Erich Haas (1888–1941), the Austrian–American Notre Dame physicist who wrote³¹ of “Hubble’s factor” in 1938 but the same year issued a conference press release (see ref. 14, p. 96) that spoke of “Canon Lemaître, ... Einstein, and Richard C. Tolman, ... as the great leaders in science’s most abstruse investigations”. The conference was attended by Lemaître and Shapley, but not by Hubble. Haas perhaps had a right to be sensitive about eponyms. According to a couple of anonymous web sites, he “prediscovered” Bohr orbits and, as is customary in such cases, was laughed off the stage with a remark that it was, after all, the first day of Carnival that year.

For what it is worth, neither Walker’s²³ nor Haas’s³¹ terminology made the cut for either Kragh¹⁴ or Nussbaumer & Bieri²⁶. Where did I find them? The *Oxford English Dictionary* of course, although the *OED* does not offer an opinion on the Chandrasekhar Limit.

The division of credit, part 2

So we return again to the legitimacy of the universal adoption of “Hubble’s Law” for the redshift–magnitude, velocity–distance, *etc.*, relationships. The underlying cause is that he was taken seriously, while Wirtz³², Lundmark³³, Silberstein³⁴, Lemaître³⁵, Robertson³⁶, and all between 1924 and 1928, were not. I’ve long thought that there were two reasons for this³⁷. First, it was the choice of Cepheids as distance indicators, so that his numbers, though wildly wrong, were self-consistent; and second, it was as Sandage³⁸ said “the manner of the man”, tall, broad-shouldered, formal and serious-looking, dressed and accented like a fellow of an Oxbridge college.

Earlier commentators have wondered about who had access to what, and when. Nussbaumer & Bieri²⁶ and others have noted that several of Lemaître’s key papers appeared in the less-than-widely-distributed *Annales de la Société Scientifique de Bruxelles* and wondered why. I think that anyone who has looked into what WWI did to Louvain and the Belgian scientific community in general cannot be surprised by Lemaître’s desire to ‘support the home team’. That the English translation³⁹ of his 1927 paper left out key passages and portions of “equation (24)” is not in doubt^{11,12}, and, perhaps, as Block, van den Bergh and Nussbaumer & Bieri say, it was deliberate and malicious. Apparently blame cannot be reliably assigned. There is no evidence that Lemaître complained, unlike Hubble, who “went nonlinear” according to Sandage¹⁶ when Shapley and de Sitter (separately!) tried to take credit for some of his work. [See note added in proof on p. 40.]

In contrast, I think who could read which languages is a *gerookte bokking* or *Ablenskungsobjekt*. My citing³⁷ of Lemaître³⁵ as “a homogeneous universe of constant mass and croissants made of rayon ...” was supposed to be funny. I’ve slogged through Wirtz³² and the paper in which Frenkel⁷ did not discover the Chandrasekhar Limit, and even the papers in Russian in early issues of the *Bulletin of the Astronomical Institute of Czechoslovakia* to see which telescopes were used, though I can claim to speak nothing but good American and a little English.

There is no particular reason to suppose that, if Hubble’s and Lemaître’s positions had been reversed, the latter would have done much to try to enhance

the reputation of the former. That Hubble⁴⁰ did not cite Lemaître in 1929 or later was noted by Nussbaumer & Bieri and many others. On the other hand, Lemaître's 1949 "Cosmological applications of relativity"⁴¹ has 11 citations, one each to de Sitter and to Freier *et al.* on heavy nuclei in cosmic rays, and nine to his own papers. In contrast, the next paper in that issue of *Reviews of Modern Physics* (which was an Einstein 70th-birthday festschrift) is by Gamow, who cites a dozen colleagues who are neither himself nor his students.

A bit more about Lemaître, his life, his times (mostly from McVittie⁴²)

Georges Lemaître first saw light of day in Charleroi, Belgium, in 1894 and went from the Jesuit school in Louvain into the engineering school at the University there in 1911. At the outbreak of World War I (not then so called), he immediately volunteered, serving for the duration and receiving the Belgian *Croix de Guerre*. He returned to the University of Louvain, but to study mathematics and physical science, receiving a degree in 1920, partly for work with de la Vallée Poussin on approximations to functions of many variables. He went on to Malines seminary and was ordained in 1923. This apparent change in direction suggests a considerable effect of the war on him, but I do not know this. A travelling fellowship carried him to Cambridge (UK), Harvard, and MIT, and back to Louvain, where he submitted a thesis in 1927 on the gravitational field in a fluid sphere of uniform, invariant density, according to the theory of relativity (which I have skimmed, thanks to Walter Lewin, who has, or had, a copy). The thesis made clear that the Schwarzschild horizon is only a coordinate singularity, not a real, physical one.

Lemaître spent nearly all the rest of his career at Louvain, teaching mathematical methods and history of physical and mathematical sciences as well as relativity. McVittie⁴² tells us that, when he was Eddington's student in Cambridge in 1930 and had been put to work finding expanding solutions to the Einstein equations, there came a letter from Lemaître to remind Eddington of the 1927 Belgian paper. McVittie reported Eddington's response as an apology plus an immediate letter to *Nature* designed to set the record straight. Eddington's 1930 item⁴³ is in fact a review of Silberstein's *The Size of the Universe*, with a paragraph at the end that says of Lemaître's solution: "It renders obsolete the contest between Einstein's and de Sitter's cosmologies. We can now prove that Einstein's universe is unstable [proof not given there]. The equilibrium having been disturbed, the universe will progress through a continuous series of intermediate states toward de Sitter's universe. By Lemaître's analysis, the history of the progress can be studied; and the intermediate stages (one of which must represent the present state of the universe) can be treated in detail." This seems fair enough. The same issue of *Nature* has the text of a 'tired light' talk given by Zwicky⁴⁴ in which he describes large redshifts as due to "a sort of gravitational analogue of the Compton effect". This was never popular, but in a sense was not entirely ruled out until supernova light-curves demonstrated time dilation in 1998⁴⁵.

Eventually, Lemaître's ideas also became unpopular, and that has perhaps also contributed to his relative non-recognition. From 1933 onward, his universe began with a primordial atom which began to break up, going through some 10^{11} years as nearly an Einstein static solution with a cosmological constant balancing the matter, then taking off in an exponential expansion, in which we now live, with Λ becoming ever more dominant. Cosmic rays in his view were left-over bits of the primordial atom. Even very late, he was not entirely convinced that there are protons among the cosmic rays, and he predicted that

the ratio of hydrogen to heavier nuclei in them would, at least, be very much smaller than it is in stars.*

Lemaître's universe, with primordial atom but continued neglect of any particular high-density, high-temperature processes, is intact in the text⁴⁸ of a talk he gave on stability of clusters of galaxies at a meeting in August of 1961 in Santa Barbara (just before the 1961 IAU General Assembly in Berkeley). He is definitely opposed to the clusters being stabilized by low-luminosity material. Rather, he wrote, they consist of galaxies being exchanged back and forth with the field.

Near the end of the paper, he describes the attitude of the community toward his model as "a strong prejudice against it, due to reverence of an authority whose influence can only be compared to that of Aristotle in older times." Kragh¹⁴ (see p. 58) thinks that he meant Fred Hoyle. My first thought (and that also of Paul Hodge, who was there) was Einstein, but over the days of writing this, I have come to think Hubble more likely. Neither, anyhow, could defend himself by 1961! Einstein had, of course, strongly repudiated his Λ from the 1930s onward; and Hubble (as is remarked upon by everyone who writes on these topics) was not much given to theoretical interpretation.

The verdict of history

Science is a self-correcting process if you wait long enough. This is surely true even for history of science, though 'long enough' may be generations. One stepping stone along the path is citation analysis. Since none of Nussbaumer & Bieri^{10,26}, van den Bergh^{11,13}, or Block¹² did this, I thought I would, picking out Lemaître, Hubble, Eddington, Friedmann, and de Sitter for examination and the time windows 1965–69 (the second flowering of cosmology following discovery of the 3K background) and 1980–84 (the latest period for which a five-year compendium of the *Science Citation Index* on paper is available in our library). You are welcome to add other folks and other time windows.

Table I shows what I found, after taking some care to filter out a few other people with similar names and dates and to catch citations to these five under all variants of their names. Most complex was Friedmann, who has been cited as A. Friedman, A. A. Friedman, A. A. Friedmann, and A. Freidman. Only his two cosmology papers were cited in those time frames. For the others, I did not attempt to separate cosmological papers from their others. Eddington was the superstar, but his most-cited items are the 1924 and 1926 books, *Mathematical Theory of Relativity* and *The Internal Constitution of the Stars*.

Everybody got more citations in the later period, mostly because the numbers of papers published and the numbers of citations contained in the average paper have grown monotonically for decades. Clearly Hubble trumps Lemaître, which is perhaps what Nussbaumer & Bieri¹⁰ are complaining about. I was surprised at how prominent de Sitter appears in those days before anti-de-Sitter space. More recent samples will undoubtedly reveal more of everything and Hubble

*The short biographies (see, e.g., ref. 46) all credit Lemaître for part of the discovery that cosmic rays are positively charged and, indeed, mostly protons. This is not quite as odd as it sounds given his other statements about the particles. A series of four papers, ending with Lemaître and Vallarta⁴⁷, addresses careful calculation of the paths of particles through the Earth's magnetic field, giving rise to a latitude-dependence in the flux received at the top of the atmosphere. The analysis requires only that the particles start out well away from Earth, so that a primordial egg or supernovae will do equally well. Manuel Sandoval Vallarta (who preferred to publish under his mother's rather than his father's surname) deserves a paper of his own, but this is not it. His post-war work with Luis Alvarez and others did indeed help to firm up the proton identification.

TABLE I
Citations to papers by five of the leading contributors to cosmology 1922–61

<i>Astronomer</i>	<i>1964–69</i>			<i>1980–84</i>		
	<i>Citations</i>	<i>Papers</i>	<i>Most-cited</i>	<i>Citations</i>	<i>Papers</i>	<i>Most-cited</i>
Lemaître	60	32	<i>Phy. R., ASSB</i>	6	25	<i>ASSB (1933)</i>
Hubble	168	25	<i>ApJ (1934)</i>	193	48	<i>ApJ (1936)</i>
Eddington	2·6 cols.	22	books	3 cols.	30+	books
Friedmann	32	2	equal	46	2	equal
de Sitter	48	24	<i>BAN, 8, 213</i>	88	43	<i>MN, 77, 155</i>

continuing to dominate as far as cosmology papers are concerned. I knew him only through the now-also-closed eyes of “Uncle Allan” (as he always signed his letters) Sandage, and am inclined to think he deserved it.

Many of the commentators have remarked on the interface between Lemaître’s science and theology. I have only one anecdote to add. It came from Willam A. (Willy) Fowler, describing an incident at a conference he and Lemaître had both attended in the era when “accompanying persons” were common and were called “ladies”. Fowler asked Lemaître when he found time to say his offices during crowded days like those of the conference. His response was that he waited until after breakfast, when the ladies said they were going upstairs to dress and would be back “in just a moment”. That, said Lemaître, gave him plenty of time.

Block¹² and perhaps others have suggested that someone, probably the Europeans, should name their next big telescope for Lemaître, to balance the *Hubble Space Telescope*. Let them, however, be warned. Shortly after the launch of *HST* and the discovery of the spherical aberration in its primary mirror, Jesse Greenstein remarked to all within hearing, “Well, Edwin Hubble finally got the telescope he deserved.”

Acknowledgements

I am indebted to the Editors of *The Observatory* for the opportunity to respond, at excessive length, to the Nussbaumer & Bieri letter¹⁰. There are no words for my enormous debt to George Abell, Allan Sandage, Dan Popper, Jesse Greenstein, Fritz Zwicky, Sidney van den Bergh, Vera Rubin, William McCrea, William A. Fowler, and Guido Münch, golden links to the era discussed here.

Yours faithfully,
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Note added in proof:

The *Nature* “mystery item” has just appeared. It is, very appropriately, by Mario Livio of the Space Telescope Science Institute (*Nature*, **479**, 171, 2011 November 10), who has established by documentary evidence that the paragraphs and equations not present in the *MNRAS* translation of Lemaître’s 1927 paper were removed by the Abbé himself. I would add only one word to Livio’s definitive statement, which is that a native speaker of French, writing in English, who uses the word “actual” almost certainly means the equivalent of “actuelle”, that is, “current”. Thus Lemaître was saying that some of the details of his 1927 paper were of “no current interest”, not that they were “of no real interest”.

REVIEWS

Newton and the Counterfeiter, by T. Levenson (Faber & Faber, London), 2009.

Pp. 318, 19.5 × 12.5 cm. Price £9.99 (paperback; ISBN 978 0 571 22993 2).

For anyone who follows the reviews in this *Magazine* closely, it may have become apparent that I've never been a huge fan of Sir Isaac Newton, principally because of his shabby treatment of the first Astronomer Royal, Flamsteed, and of the polymath Robert Hooke. My prejudice was nothing to do with Newton's science but more a reflection on his apparent arrogance and less-than-adequate social skills. On visiting Newton's family home at Woolsthorpe recently, I came across a book that I thought might shed more light on the character of the great scientist, not in his laboratory or while engaged in his amazing mathematical works but later in his life as Warden of the Mint. And having now read *Newton and the Counterfeiter*, I felt it worth encouraging readers of *The Observatory* to turn aside from science for a while to read a splendid tale in which our astronomical hero turns detective and saves England (if temporarily) from financial chaos.

As the 17th Century came to a close and England was engaged in yet another expensive war against France, the Treasury woke up to the problem of the diminishing amount of money, particularly silver coins, in circulation. The main driver was the disparity in the price of silver between England and the Continent, such that it became very profitable to export silver to Europe to buy gold (which could be used to buy even more silver back in England, and so on). This, coupled with the somewhat older problem of 'clipping' (trimming silver from coins) and counterfeiting the coinage, required urgent attention and, perhaps in the same way that the Victorian government often turned to Airy for help in such matters (politicians and their ilk being clearly useless — perhaps still the case?!), King William's ministers brought Newton on board to solve the problem.

And this he did with amazing vigour, first by instigating a massive re-coinage with the mechanized production of coins with milled edges that could not be clipped. (He also wanted to reduce the amount of silver in them to lower the intrinsic value to the face value — effectively a devaluation — but his political masters quashed that notion.) Then he set about cracking down on the 'coiners' (counterfeiters), which he did by establishing a network of spies and informers of which surely Queen Elizabeth I's Sir Francis Walsingham would have been proud.

The anti-hero of this particular story was one William Chaloner, a successful coiner who was sufficiently bold to offer his expertise to the Mint (then housed in the Tower of London) to help solve the coinage problem, but actually as a ploy to obtain the dies that would allow him to produce even better counterfeit coins. He was versatile and enterprising since he was also one of the first to forge paper money, actually the 'Malt Lottery' tickets (a kind of bond) issued by the government to raise money. However, he had met his match in Newton and after a rather dubious trial presided over by a fierce (*i.e.*, hanging) judge, he was sent to Tyburn for a grisly end.

This fascinating tale is properly set in context by a well-researched overview of Newton's early life and his contributions to science, theology, and alchemy (which, perhaps, engendered in Newton a particular interest in producing wealth from base metals!) that occupies the first third of the book. Rounded out

with an exhaustive set of notes, a valuable bibliography, and a good index, the book becomes not just a fine story but a splendid resource for historians and those wishing to delve more deeply into the activities and ideas of a remarkable man. The writing is exemplary and it's a delight to read. — DAVID STICKLAND.

The Inspiration of Astronomical Phenomena VI (ASP Conference Series, Vol. 441), edited by E. M. Corsini (Astronomical Society of the Pacific, San Francisco), 2011. Pp. 558, 23.5 × 15.5 cm. Price \$77 (about £50) (hardbound; ISBN 978 1 58381 762 9).

This volume is the proceedings of the sixth in the series of conferences on the 'Inspiration of Astronomical Phenomena' (INSAP VI) and apparently the first to be reviewed in *The Observatory*; previous meetings have been reported in special issues of a variety of serials. The origin of the INSAP meetings was recounted by the Chairman, Rolf Sinclair, and the inspiration of the night sky which underlies the INSAP idea vividly illustrated by quotations collected by Moore, Richman, and Chamberlain, including this by Llewelyn Powys: "No sight that human eyes can look upon is more provocative of awe than is the night sky scattered thick with stars". Poets have responded to astronomy through the ages, as surveyed in a fine contribution by Piero Boitani, who takes us all the way from *The Odyssey* to James Joyce's *Ulysses*, via Dante, Shakespeare, and Leopardi. Inspiration of the visual arts and architecture is described in many well-illustrated and referenced contributions. Amongst them, Giordana Canova surveys the influence of astronomical themes in manuscripts and painting in Padua in the middle-ages, when it was one of the most important scientific centres in Europe, and gives a detailed tour of the frescoes in the Palazzo della Ragione and their iconography. In our own time, Jim Cogswell describes the thinking behind his works, including *Meanwhile More Light*, created for the University of Michigan and incorporating astronomical images and line drawings. The portrayal of solar eclipses in western art over the centuries is surveyed by Olson and Pasachoff. There are contributions describing ancient monuments and megalithic observatories, notably that at Kokino in Macedonia by Gjore Cenev, and discussion of their successors: large-scale 'Earthworks', built recently and considered by John Hatch.

The conference was held in 2009 to celebrate the 400th anniversary of Galileo's first use of the telescope, and its first section focussed on Galileo and his time. George Coyne begins this with a detailed and balanced comparison of the positions of Galileo and Cardinal Bellarmine on Aristotle's natural philosophy and the interpretation of Scripture, while Melchor Sánchez de Toca concludes the section with a review of the Pontifical Commission on the Galileo case, which came to an end in 1992. In between, we learn about music in the Galileo family, Galileo's medical condition, and his opinions of other authors as documented by his annotations in his copies of their books. As traced by Ottavio Besomi, Galileo's reading was critical and his responses often intemperate.

There is a lot of history here. Amongst the byways were the attempts over the years, starting with Augustine and the Venerable Bede and given impetus by the production of the first star atlases, to replace the pagan names of the constellations with Biblical figures. As recounted by Mendillo and Shapiro, this was successfully resisted by Hevelius. This is a fascinating and rewarding book, more for dipping into than reading cover to cover, and I can't imagine anyone not finding something of interest in it. One for the library. — PEREDUR WILLIAMS.

The Role of Astronomy in Society and Culture (IAU Symposium No. 260), edited by D. Valls-Gabaud & A. Boksenberg (Cambridge University Press), 2011. Pp. 793, 25 × 18 cm. Price £73/\$125 (hardbound; ISBN 978 0 521 76477 3).

This is quite a book, representing the proceedings of IAU Symposium 260 held at the UNESCO building in Paris in 2009 January! It is not just the size but the range of topics covered that is daunting. There are papers dealing with overviews of the very concept of science to the practical details of teaching basic astronomy to primary-school children in some of the most deprived countries on the planet. The huge ambition behind the symposium title is not always realized in the contributions as the organizers appear to have adopted an open-door policy and any paper presented seems to have been accepted with very little editing in these proceedings. However, apart from the occasional dip in quality, the proceedings convey the excitement of the symposium. The 793 pages contain 104 papers on topics connecting astronomy, society, and culture covering as many concepts linking those three words as you could imagine. The proceedings are broadly divided into six themes: 'Setting the context', 'Astronomy in culture and the culture of astronomy', 'Astronomy and the arts', 'The social impact of astronomy', 'Astronomy as a tool for development and technologies', and finally, 'Education and public understanding of astronomy'. An additional 48 papers have been published on-line only.

So, in spite of my earlier trepidation after reading through this collection, my view is that, even with the horrendous proof-reading glitches and the clunky 'Eurospeak' of some papers, I found something interesting and often surprising in almost every one — it would have been every paper had my command of written French been enough to read a collection of doubtless heart-rending poems, and one paper, written in French.

Pretty much all aspects of culture across the World are represented: astronomy in visual arts, astronomy in music, and astronomy in literature. Astronomy in history and ancient history is well represented; Kepler's groping for an understanding of the orbit of Mars; the accidental stroke of luck that enabled Galileo's serendipitous discovery of the moons of Jupiter; and the origins of astronomy in astrology. As well as those traditional subjects there are also unexpected accounts; for example, 'The history of planetariums', 'The orientation of dolmens', 'Astronomy in the Bronze Age', and 'The Antikythera Mechanism', to name just a very few. Under the aegis of UNESCO there are, as you might expect, the obviously worthy tales of astronomy/astrology from far away and long ago: the grandmothers' stories from New Zealand, a country whose very existence depended on the astronavigational skills of its founders; tales from the dream time of Australian Aboriginal culture, one of the oldest cultures in the world; various foundation myths; origins of the names of the planets, *etc.*, *etc.* There are also some truly astonishing and genuinely heartening accounts: the French outreach programme called 'The Sky Belongs to Everyone', in which astronomers visit people whose access to the night sky is limited — prisoners, the elderly in care homes, disabled children — it would take a harder heart than mine not to be moved.

As well as these 'astronomy in culture' papers there are several on the 'culture of astronomy' itself. I found the paper by Martin Harwit on 'The growth of astrophysical understanding' the most engaging in the first section. He completely connected with the subject of the conference and it is worth getting hold of the proceedings for this contribution alone. He provides illustrations

on the nature of collaborative working and the dominant influence of a few individuals across wide swathes of knowledge, and as a rebuttal to those who think culture has nothing to do with science he points out that, although perhaps initially driven by enthusiastic interest, most professional astronomers do in fact 'follow the money', which is to say that their work is at least to some extent influenced by the cultural forces which provide the funding.

That early paper on the world of the professional astronomer nicely contrasts with one in the final section in which Kevin Nolan expresses a need for a new look at the way people are brought into science. He argues against much of the traditional aim of outreach programmes and indeed much of the thrust of many of the symposium papers. In Nolan's view there is a mismatch between the scientist's belief that the general public find astronomy and space research awe-inspiring and especially interesting, whereas in fact it holds no more special place than science in general. This mismatch he believes leads to the failure of many outreach programmes and he proposes that their aims be more thoroughly thought through. He suggests that the aim of outreach should be to disseminate the nature and value of science in an adult way across the whole range of human endeavours, and show its value to all society rather than presenting science in a specifically child-orientated way which simply aims to enthuse with fun and superficial drama.

Several papers address the perceived low esteem in which science and scientists are held by the general public, several mention the mad-scientist stereotype, and the rôle of the media in creating and maintaining false mythologies about science. Henri Boffin reports some astonishing statistics from an NSF survey which revealed that, although 75% of those questioned (in the US) knew the concept of a molecule, fewer than 50% knew that the Earth orbited the Sun. He notes that since its founding in 1923 the cover of *Time Magazine* has featured an astronomy story only 12 times, in contrast to medicine which appears 248 times, thereby revealing that well-known media truth that there are no such things as science stories, only people stories — some of which may be about scientists. Carlos Elias reports on the worrying trend in the decline of science students in UK universities, noting a survey in *The Observer* newspaper in 2006 March which reports that, since 2004, students registered for media studies have outnumbered those registered for any of the traditional sciences.

In summary, if you put up with the errors, the clunky text, the typos, and the self-important language, you will find this book a gem, and at £73 it is approaching the same price as jewellery, and it is certainly worth getting your institution's library to buy it — maybe on their outreach budget. It is perhaps too diverse for a cover-to-cover read but it should be made available for casual browsing over coffee, as every professional astronomer reading this will be well informed and inspired to do better at explaining why science and astronomy is just so important. — BARRY KENT.

Our Enigmatic Universe, by A. H. Batten (Melrose Books, Ely), 2011. Pp. 205, 21 × 14.5 cm. Price £8.99 (paperback; ISBN 978 1 907732 03 4).

Fundamental questions of purpose, existence, creation, origin, provenance, and destiny fascinate and possibly trouble all of us at times. In this book Alan Batten addresses all those questions and more besides, not so much by developing a singular set of reasonings towards his own personal persuasions but by weighing up the arguments offered by a host of other authors, philosophers, theologians, and scientists throughout recorded time. Starting with our

perceptions of the Universe (or multiverse) he goes on to embrace the need or otherwise for Divine Purpose, and *via* discussions of life and consciousness confronts the apparent or non-existent dichotomies between the mind and the brain, and progresses thence to (*inter alia*) different interpretations of revelation and afterlife and the reality of miracles and prayer. For anyone who finds even only superficial stimulation in pondering these (almost) imponderables there is material here for endless reflection. The 'solution', if such there be, may be the harder to recognize from our present human condition because we are still in the midst of working our way towards it (the 'inside' view), as Batten explains in his final word on the harmonization of science and religion.

This book is superbly well written, grammatically correct (except, perhaps, for one application of a semi-colon with which I felt uncomfortable, but I only spotted five such occurrences), and accurately produced. His arguments are well-formed, cogent, and logical, though the reader might need some background knowledge of both sides of the picture (contemporary science and theology), in particular their respective histories, to gain maximum advantage from each 'lecture'. The author draws from an astonishingly rich bibliography (mostly books, not just papers), reflecting a sound scholarship as well as providing a very valuable synopsis for lesser specialists.

In just a few places I wanted to argue with the author. The suggestion (not originally his) that 'Intelligent Design' would not have created a human eye which is capable of such frequent deficiencies misses the point that modern humans surely stretch the demands placed upon one's eyes far beyond their original design as organs to spot a predator, recognize a place, and differentiate between dark and day, and his discussion of prayer gave scant space to its parallels in the power of the subconscious or the persuasiveness of positive thinking. However, one point does need to be made: this book is not really about astronomy or cosmology. The first chapter offers a splendid summary of mankind's progress in learning, reasoning, discovering, and explaining the cosmos, but beyond that it concentrates on broader issues which it collectively labels "transcendent" — beyond perception by the five senses — and whether our human experience can support them. The enticing image of galactic dust-clouds on the cover is therefore a little misleading, and the sub-title, "One astronomer's reflection on the human condition", would be just as valid if 'astronomer' were omitted. Even so, it is still a seriously worthwhile read — comprehensive, informative, and challenging — and is highly recommended.

— ELIZABETH GRIFFIN.

The Living Cosmos, by C. Impey (Cambridge University Press), 2011. Pp. 393, 23 × 15 cm. Price £15.99/\$24.99 (paperback; ISBN 978 0 521 17384 1).

This updated edition of a book about astrobiology competes with other books on the topic which have appeared in recent years. Astrobiology, a multi-disciplinary science, is important for the possibility of finding alien life.

The first chapter, 'The unfinished revolution', focusses on Epicurus, Bruno, Copernicus, and Pre-Socratic thinkers — for example, Thales of Miletus and Pythagoras. The choice of material for this excludes the influence in the phase of human development that lay behind Pre-Socratic thinking. Sumerian ideas, for instance, were comparable to Thales', that (liquid) water is the source of the Cosmos and life.

The course of the book thoroughly recounts later assumptions and opinions on the cause and pace of life on Earth. Chris Impey showcases "Evolution

is a challenging concept” with realism. He presents the fact that, within the Darwinian frame, we know about intermediate stages, but can only speculate vaguely about the cause of terrestrial life. Darwin had also acknowledged that fact at the time of *The Origin of Species* in 1859.

At this point in the book, the breadth of the approach of the author ranges from extremophiles, life beyond biology, to ‘what if’s’. He goes on, by way of these, to discuss the quest for life elsewhere in the Solar System: Mars, Titan, Europa, and Callisto. In favour of discovering the presence of life, he continues with the challenging question of what are the contributing factors that determine a ‘microbial habitable zone’ and an advancing living-system-habitable zone, such as on the Earth. The question is too great for an answer at this time, because we need to know what organisms were present originally, and how to recognize exotic forms of life.

In the context of distant worlds of exoplanets and the use of new technologies, Impey inevitably broaches the search for technological alien civilizations. This part of the book is disappointing, because it re-chews well-known material — like the Fermi paradox, the absence of space visitors, belief systems, and the ‘Great Silence’. It also includes material that touches upon the mediocrity principle, from the pioneering times of Shklovskii and Sagan. Unavoidably, owing to the absence of confirmed signals from extra-terrestrial species, all this impels the conclusion that SETI is currently in a doldrums-like state, and that alien technological civilizations may be exceptional.

Taken as a whole, this lucidly written book is ideal for readers unfamiliar with astrobiology, and who have an appetite for suppositions and the exotic. — P. CHAPMAN-RIETSCHI.

Constructing Reality: Quantum Theory and Particle Physics, by J. Marburger III (Cambridge University Press), 2011. Pp. 287, 23.5 × 16 cm. Price £17.99/\$29 (hardbound; ISBN 978 1 107 00483 2).

John Marburger III has a sense of history, and in this slim book gives a sincerely-felt and non-mathematical account of the development of quantum theory, aimed at convincing the reader that it is a valid description of reality.

He starts his exposition with the modest if slightly ominous view that quantum mechanics is difficult to understand because its rules do not match our macroscopic perceptions. He proceeds to develop the rules as they are currently understood, but along the way dismantles much of the physics traditionally taught in universities, arriving at the conclusion that quantum theory operates in a kind of shadow world. For example, he discards Schrödinger’s waves and other easily-visualizeable things, and concludes that the real world is a vastly complicated and therefore confusing system of vectors in Hilbert space, whose interactions only make sense to us when we sift through the algebraic underworld and isolate simple events by cleverly designing sophisticated detectors. He arrives at the conclusion that the ‘real’ world is unseen and almost unknowable. Thus he says in his Epilogue (‘Beneath Reality’) that Bohr orbits are merely “historical curiosities” and do not exist. Am I the only one who finds this depressing?

There is a *penchant* these days for publishers to bring out popular books on quantum mechanics which din into the reader the view that the subject is mysterious and perhaps even non-understandable, while simultaneously vaunting how awesome it all is. The impression left is that the average layman is an ignoramus with barely enough I.Q. to open the book covers. The volume by Marburger is better than most, in that he admits to being baffled by much of the

subject even though he has worked in it most of his professional life. However, all the books of this type I have read have the same strange combination of boastfulness and pessimism. I for one do not share that. And neither do most of my colleagues, who are cosmologists. Yes, it requires some effort to master tensor calculus and the intricacies of Einstein's General Theory of Relativity. But nobody doubts the reality of stars and galaxies; and the Big Bang may be weird but can be understood. This does not mean that cosmologists are simple-minded people who believe that the Universe is just a vast clockwork. (Like Marburger, I published a book myself in 2011 called *Weaving the Universe* [see review 131, 404, 2011] which examines to what extent our description of the cosmos is influenced by subjective elements connected to the workings of the human mind, a view espoused by the great astronomer Sir Arthur Eddington.) Rather, most cosmologists believe that while their theories may be approximate they *do* describe the perceived set of things which we for shorthand call reality.

If there is anything depressing and pessimistic about science, it is the difficulty researchers in quantum mechanics and cosmology have in talking to each other. This is partly because of the differences in the language of the two areas (vectors in an abstract Hilbert 'space' *versus* the curvature of Einstein's space-time). However, there is also a philosophical difference, which was pointed out by Bohr and Einstein and has to do with whether the world is based on chance or law.

Marberger's book is a valiant attempt to persuade us that quantum theory is the best result of people's efforts to understand what is at base an illogical, complicated world. He *could* be right. But there have been other periods in the history of science when theory became hopelessly complicated and contradictory, before a new way was found. (Remember the aether?) A possibility is that quantum mechanics has wandered into a forest of abstractions, and that we 'cannot see the wood for the trees'. Human beings are *of* this world (even those who do quantum mechanics), so it seems likely that they can *understand* it. — PAUL S. WESSON.

Apollo 12 — On the Ocean of Storms, by D. M. Harland (Springer, Heidelberg), 2011. Pp. 522, 24 × 16.5 cm. Price £40.99/\$44.95/€44.95 (paperback; ISBN 978 1 4419 7606 2).

Half a century has passed since the US Apollo programme delivered 12 astronauts to the surface of the Moon. Since then, innumerable books have been written about this brief, but magnificent, episode of human lunar exploration, and it would seem that there is little new left to say. However, I would recommend that all serious space historians find room on their bookshelves for this in-depth retrospective.

David Harland is a prolific writer on space topics, including the Apollo programme, and this is his second contribution to a series of books on the lunar missions. The author has drawn on numerous primary sources, including the *Apollo Lunar Surface Journal*, a record of surface operations which includes a transcript of all recorded conversations between the moonwalkers and mission control. This inclusion of quotations and conversations made at the time gives the reader invaluable insights into the activities of the crew and the problems they faced.

Apollo 12 has generally been regarded as a sideshow after the headline-grabbing exploits of Armstrong, Aldrin, and Collins, but, as the author amply demonstrates, it was, in many ways, a more significant demonstration of America's technological and scientific prowess. The second manned mission

to the Moon included more than 31 hours of activity on the lunar surface — divided into two lengthy moonwalks, a pinpoint touchdown, the deployment of geophysical instruments, a geological traverse, and a visit to the *Surveyor 3* spacecraft.

The book is well illustrated with numerous diagrams and photographs, although the reproduction of the colour images is poor. The final chapter includes some details of the rock samples which were returned by the crew, and there is a glossary which explains many of the geological terms used in the text.

As a stand-alone historical account of an individual mission, the book is one of the best I have come across. Those who want to consider the rôle and contribution of *Apollo 12* in the overall context of the lunar programme will need to refer to another source. — PETER BOND.

Principles of Star Formation, by Peter Bodenheimer (Springer, Heidelberg), 2011. Pp. 343, 23.5 × 15.5 cm. Price £81/\$124/€89.95 (hardbound; ISBN 978 3 642 15062 3).

This is an absolutely superb book*, from the beginning of the preface, where the author provides a basic definition of star formation, to the last section where he reminds us that “efficiency” of star formation can mean at least three different things, with three different numerical values, depending on whether you are talking about a single cloud core, a giant molecular cloud, or a whole galaxy. Bodenheimer has taught a graduate course on star formation a number of times at the University of California, Santa Cruz, the lecture notes for which formed the basis for the text.

The book concentrates on the interpretation of observations in star-forming regions relatively nearby, in our own Galaxy, at the present time, where the most detailed and accurate results can be obtained. Chapter 7 is an excursion back to the early Universe and formation of Population III, including the possibility of dark-matter annihilation as an energy source. Massive stars and binaries each also get their own chapters. Chapters 2 to 8 each end with two to six problems. Most of the problems concern things I would definitely want students to understand — Hayashi tracks, radiation pressure, the brown-dwarf–star divide, specific angular momentum of stars *versus* molecular clouds, multiple-formation scenarios for binaries, and so forth (but you must tackle homology on your own in Chapter 8 without my help).

Just about everything you would expect to find is here — induced or triggered star formation, difficulties in measuring (let alone modelling!) the IMF, discs, winds, and jets, and a brief note on numerical methods, from which the reader can expect to emerge knowing the difference between Eulerian and Lagrangian methods, if not quite ready to start calculating alone. What I have always called the Courant condition is properly dubbed Courant–Friedrichs–Lewy, with reference to the original 1928 paper, along with 575 others, Abel to Zuckerman, or Jeans (1928) to many from 2010. All are proper references to journals, books, and conference proceedings without a DOI or an arXiv in the lot, though the DOI of the book is 10.1007/978-3-642-15063-0 in case you should want it for anything. — VIRGINIA TRIMBLE.

* Of course I looked first for any reference to my own work on statistics of binary systems. Yes, there is a discussion of the distribution of mass ratios, and yes, it has what I think, is the right answer (bimodal, with peaks at 0.3 or less and very close to one) but with credit only to more recent papers. And how a star decides whether to have a companion remains unclear. I think the range of primary masses, given as 0.6 – 0.85 M_{\odot} on page 225, must be a misprint for 0.6 – 85 M_{\odot} .

Neutrino Physics, 2nd Edition, by K. Zuber (CRC Press, Boca Raton), 2012. Pp. 448, 24 × 16 cm. Price £82 (hardbound; ISBN 978 1 4200 6471 1).

The second edition of this very complete and detailed overview of neutrino physics is a welcome update. The first version was published in 2003, only relatively shortly after the *Sudbury Neutrino Observatory* delivered the final proof that neutrinos change flavour and therefore must have mass. Since then, the field has increased exponentially and many new, exciting findings have been published. The book is a thorough introduction to neutrino physics for graduate students, but at the same time a good reference for researchers in the field. I have eagerly awaited an updated version; also because the first version contained a not insignificant number of minor mistakes and editorial errors.

The second version does not disappoint. A few minor editorial errors are still present, but on the whole the book is presented in a much better form. The content has been brought up to date to the same standard as the rest of the text. The author succeeds in again presenting a complete overview of this complex field in a concise and very clear manner. Wherever the discussion skips some detail that would be a distraction in the author's view, a number of excellent references are given.

The book starts off with a selection of the most important historical experiments. The following chapters give a detailed overview of those properties and a refresher of the relevant parts of the Standard Model of particle physics. The theoretical introduction is continued with a description of neutrino–nuclear interactions and possible relevant physics beyond the Standard Model.

The discussion of the experimental studies of neutrinos forms the major part of this book. This starts with the various methods used to determine the neutrino mass, among which the currently popular search for so-called neutrinoless double-beta decay. It then discusses in detail neutrino oscillations and the different experiments involved. The book concludes with three chapters on astrophysics-related topics: supernova neutrinos, cosmic neutrinos, and the rôle neutrinos play in cosmology.

To summarize, the book is a very complete and up-to-date overview of neutrino physics, with an emphasis on the experiments. Apart from some minor editorial errors, it is an excellent introduction to the various aspects of this exciting field. — SIMON J. M. PEETERS.

Advances in Plasma Astrophysics (IAU Symposium No. 274), edited by A. Bonanno, E. de Gouveia Dal Pino & A. G. Kosovichev (Cambridge University Press), 2011. Pp. 489, 25 × 18 cm. Price £73/\$125 (hardbound; ISBN 978 0 521 19741 0).

This volume contains a collection of review and research papers arising from IAU Symposium 274 on plasma astrophysics, held in Italy in 2010 September. It is a commonplace that most of the matter within the Universe exists in the plasma state, such that it comes as no surprise that the subject matter of this book addresses an eclectic variety of astrophysical systems, from the Sun and heliosphere in our own back yard, to pulsar magnetospheres, accretion discs and jets in AGN, and the large-scale structure of galaxies. Common physical themes nevertheless emerge over many of these topics, though in widely different parameter régimes, concerned with plasma instability, turbulence and its evolution, magnetic reconnection, shocks, and associated particle-acceleration processes, it being notable how often the name E. N. Parker appears in the reference lists as a seminal originator. Overall, the volume

contains just over one hundred papers in its 489 pages (together with a brief index), somewhat imperfectly organized into topic areas such as ‘Solar and stellar plasma’, ‘Compact objects’, and ‘Galaxies and galaxy clusters’, and so on, together with separate sections on laboratory experiments, related international projects, and simulations, much of which might better have been integrated into the relevant topic areas. The above statistics indicate then that the papers are typically pithily short, varying from three-page extended abstracts to a number of more extended (six- to eight-page) review contributions, including excellent condensed discussions of auroral acceleration, by Haerendel, galactic magnetism, by Beck, and simulations of astrophysical dynamos, by Brandenburg. Overall, the book provides an excellent flavour of the broad range of work, observational, laboratory, theory, and simulation that is currently being undertaken internationally in this interesting and important research area. — STANLEY W. H. COWLEY.

Annual Review of Earth and Planetary Sciences, Vol. 39, 2011, edited by R. Jeanloz & K. H. Freeman (Annual Reviews, Palo Alto), 2011. Pp. 681, 24 × 19.5 cm. Price \$240 (institutions, about £149), \$91 (individual, about £57) (hardbound; ISBN 978 0 8243 2039 3).

As always, this year’s *Annual Review of Earth and Planetary Sciences* is a superbly bound and produced volume. The series seems to include more and more colour every year, and this year’s papers are even more lavishly illustrated, providing extremely nice material for teaching. The book is printed on high-quality paper, and the pages turn and lay beautifully. Again, as always, it covers diverse subjects that range from the structure of the deep mantle to the evolution of ecosystems. It is almost 100 pages longer than last year’s volume — we must be progressing even faster than before!

The first chapter, by Burke, is unfortunately a disappointment, not least for its inaccuracies (mantle plumes were postulated by Morgan in 1971, not by Wilson in 1968). This article continues to re-iterate the standard, but now-doubted, plume hypothesis, ignoring most of the last decade of questioning. It emphasizes recent suggestions that (cold!) kimberlite pipes are due to plumes from the deep mantle, and mirrors a similar article by White last year. Fortunately, it is balanced by a more rigorous chapter on the seismological structure of the Earth by Lay & Garnero, which is heavier on facts and lighter on assumptions and speculation. I appeal to the editors of *Annual Review* to invite an article next year from someone who has the knowledge to excite the readership with the reasons why hundreds of scientists now seriously question the mantle-plume hypothesis. While I do not grudge celebration of the achievements of past giants in Earth science, scientific progress is all about looking forward and not constantly reiterating standard models.

Several fascinating, cutting-edge chapters focus on palaeontology, including soft tissue by Schweitzer, the origins of multicellularity by Knoll, palaeoecologic megatrends by Bush & Bambach, and Archaean microbial-mat communities by Tice and colleagues. Unlike last year, there are rather few chapters on planetary research, an exception being a chapter on extinct radionuclides. These give insights into the speed at which planets grew in our Solar System (fast). A chapter on slow earthquakes and non-volcanic tremor by Beroza & Ide explains that, while less headline-grabbing than great earthquakes of the ‘normal’ variety, these processes are no less important geologically.

Ecological issues are covered by chapters on Earth’s regolith by Brantley & Lebedeva and the evolution of grassland ecosystems by Strömberg. The

expansion of grasses since the Late Cretaceous replaced forests, but did not necessarily coincide with the spread of herbivores. The climate of the Neoproterozoic, including possible ‘Snowball Earth’ events where much of Earth’s surface was frozen, is discussed by Pierrehumbert and colleagues. There is apparently no satisfactory model for the breakdown of Earth’s silicate weathering (temperate) thermostat, which is expected to prevent the Earth from becoming very cold. One possibility is that dust could lower albedo and help deglaciation — the ‘Mudball Earth’ hypothesis. It is nice to know that there are still puzzles out there. This chapter is nicely balanced by ones by McInerney & Wing on the Palaeocene–Eocene Thermal Maximum (PETM), when the release of carbon into the ocean–atmosphere system resulted in global warming of 5–8°C, and by Mitrović & Wahr on ice-age Earth rotation (we got it wrong before, but we’ve fixed it now).

All in all, a great book, and an excellent summer read to catch up with some of the subjects in which progress in Earth science shows no sign of slowing. — GILLIAN FOULGER.

Asteroids and Dwarf Planets and How to Observe Them, by R. Dymock (Springer, Heidelberg), 2011. Pp. 261, 23.5 × 17.5 cm. Price £35.99/\$39.95/€39.95 (paperback; ISBN 978 1 4419 6438 0).

You get two books for the price of one here. The first 60 pages give the formal definitions of small Solar System bodies, along with their designations, groupings, orbits, physical characteristics, possible origins, and likelihood of impact. There’s nothing about their individual names: for that, the reader is sensibly referred to Schmadel’s *Dictionary of Minor Planet Names*. But the author tries hard to cover everything else — in some cases, rather too briefly; and there are no references, so the reader has no easy way to learn more.

The remaining two-thirds of the book discusses how the amateur can observe these objects. Spotting an asteroid with binoculars; doing astrometry, photometry, or spectroscopy; timing occultations; searching on-line images — Dymock explains how to go about it, usually describing case-studies. He writes for those who have “a general knowledge of matters astronomical”; terms such as ‘magnitude’ are not explained.

And almost everything requires a computer. Provided you are happy to use MEGASTAR, ASTROMETRICA, CANOPUS, and FIND_ORB, there is plenty of detailed information here — and you don’t need any real understanding of how the numbers emerge from your images. A 2009 paper by Dymock and Richard Miles, reproduced as an Appendix, describes a “new” method of accurate photometry by getting reference-star magnitudes from the CMC14 catalogue; this glimpse of “real” photometry is somewhat diminished by learning that it has now been incorporated into ASTROMETRICA.

There are certainly many excellent hints and tips. I noticed few errors, mostly trivial. The default slope parameter *G* is given as 0.015 when it first appears, but correctly as 0.15 thereafter. There are some omissions: the validity of taking flat fields with a home-made light-box isn’t discussed, nor how the characteristics of your CCD affect the precision of your photometry.

Relevant websites, software packages, and books are listed in the Appendices — and there are useful links to sources where you can select your own targets. Creditably, Dymock also shows you how to publish your results, and indeed urges you to do so.

There are monochrome illustrations on most pages, but many are computer screen-grabs, sometimes poorly reproduced; other diagrams are drawn

competently enough by the author. The Table of Contents is lengthy and detailed — which is fortunate, because the index is execrable: hard to navigate, appallingly incomplete, and occasionally wrong. The spelling is American, but the overall blend of enthusiasm and diffidence could only be British. I like this book. — FIONA VINCENT.

Star Clusters: A Pocket Guide, by C. A. Cardona III (Springer, Heidelberg), 2011. Pp. 188, 20.5 × 12.5 cm. Price £35.99/\$39.95/€39.95 (paperback; ISBN 978 1 4419 7039 8).

I like star clusters. As a researcher I spend most of my time examining what young star clusters tell us about star and planet formation. But when just looking at the sky, star clusters are some of the most amazing objects there. Their beauty comes from containing tens to millions of stars in just a few cubic parsecs; these stars are all the same age so it is possible to look at a globular cluster and see stars over 12 billion years old, or look at the Orion Nebula Cluster and see stars just after they have formed (and whilst they are still forming planets).

This book is an amateur's guide to observing star clusters; it tells you where they are with a picture of the cluster and a finding chart and suggestions for what instrument to use. What makes this book nice is that it also gives some background on the clusters — their age, and interesting facts and figures on each (for instance, Galileo was the first to resolve the Beehive cluster into individual stars). It also has a little introduction to some of the astrophysics of stars and star clusters.

It's a nice little book that makes finding clusters easy and tells you about what you're looking at, so it's interesting for anybody with a telescope; and possibly very useful for teachers as it shows where to find clusters to illustrate various basic astronomy topics like the H–R diagram, stellar ages, different masses, *etc.* — SIMON GOODWIN.

The Secret Deep, by S. J. O'Meara (Cambridge University Press), 2011. Pp. 483, 26 × 18 cm. Price £29/\$48 (hardbound; ISBN 978 0 521 19876 9).

With an intriguing title, this is the fourth book in the author's *Deep-Sky Companions* series which, like the previous three, is very much targeted at visual observers under dark skies with modest-sized telescopes. His previous volumes were: the *Messier Objects*, the *Caldwell Objects*, and *Hidden Treasures*. *The Secret Deep* provides a wealth of information on 109 objects not featured in those other volumes (apart from one) and whilst 'secret' may be somewhat of an exaggeration, they are generally well off the beaten track. Most of them deserve to be better known and this book aims to put that right.

O'Meara probably needs no introduction as he is one of the world's foremost visual observers and prolific authors. He came to my notice around 25 years ago when he was the first visual observer to recover Halley's comet — no mean feat. He is now located in a visual observer's dream location — 3500 feet up a mountain on a Hawaiian island! Whilst the location may be atypical, the telescope he used for all the visual descriptions in the book is more modest — a 5-inch Televue apochromatic refractor. It makes this volume slightly inconsistent with the other three books in the series as previous visual observations were carried out with a smaller 4-inch refractor. It is only a minor change and probably not a major concern for most readers.

The meat of the book, and I mean meat as it runs to almost 500 pages, are the entries for each of the 109 objects. This includes basic object data, finder charts,

an image (of variable quality), an object drawing, plus comprehensive descriptive text. The latter covers topics such as background/historical information, up-to-date technical explanations, how to locate the object, and that all-important visual description — what you can expect to see at the eyepiece. The author's drawings are commendably simple — not over-exaggerations. Probably because the observations were carried out under pristine skies there are very few references to using nebular filters. The selection of objects comprises 38 galaxies, 23 open clusters, 11 globular clusters, 18 planetary nebulae, 15 bright nebulae, one supernova remnant, one quasar, and one black-hole-companion star. For several of the objects the author invents his own names; examples include the Patrick Starfish Cluster, the Loch Ness Monster Cluster, and the Whirligig Galaxy. Personal taste will dictate whether this appeals or not. I felt cheated with only one of the objects, number 67, as this is our old friend NGC 5195 — the small galaxy interacting with M 51. However, even counting only 108 'new' objects, there is something for everyone.

In summary, *The Secret Deep* is a great book for visual observers with access to dark skies who are seeking out new objects. I emphasize dark skies as there is little guidance for those observing under more-typical light-polluted skies. I have a few minor niggles. The comprehensive descriptive text is so extensive that it would have benefitted from sub-headings. This would make diving in and out much simpler. I mentioned images of variable quality but this is not a 'wow'-type book so it is only a minor disappointment (for me). It is after all an observing database for visual observers — a task it succeeds in remarkably well. — DAVID RATLEDGE.

Atlas of Astronomical Discoveries, by G. Schilling (Springer, Heidelberg), 2011. Pp. 243, 30.5 × 25 cm. Price £27.99/\$39.95/€39.95 (hardbound; ISBN 978 1 4419 7810 3).

Open this impressive book at random, and there's a full-page colour image facing a page of text, which describes an astronomical discovery. Govert Schilling has picked around 100 of these — some familiar, others less so — arranged chronologically from 1609 (Galileo's lunar mountains) to 2007 (the 'habitable' planet Gliese 581c). Each description is self-contained; anyone reading from start to finish would notice a good deal of repetition, but that's probably not the best way to use the book. However, an overview is provided for each century (or, in the case of the 20th, half-century).

Schilling is an experienced science writer, and his text is a pleasure to read. The book was originally published in Dutch, but has been translated into almost flawless English — though the historical pages are consistently written in the present tense, which occasionally reads oddly, and the statement that "the orbit of Mars is not a complete circle" might be slightly misleading. One delight is that the author gives full weight to European contributions (especially those made by Dutch astronomers!): Fritz Zwicky features on a number of pages, and Fred Hoyle is treated fairly.

With a few exceptions, both the history and the science appear careful and accurate. Thomas Harriot, Johannes Fabricius, and Thomas Henderson are appropriately credited; the 'Face on Mars' and the micro-organisms in a Martian meteorite are included, but given no more credibility than they deserve. The explanation of gamma-ray bursts is perhaps rather superficial, and it's not easy to understand parallax and aberration without diagrams. Describing Betelgeuse as the "top left" star in Orion briefly betrays a northern-hemisphere bias.

I noticed only one spelling mistake, and only one entry missing from the very good index. The book is designed beautifully, but not always wisely. Each full-page image overlaps a couple of centimetres on to the opposite page: this edge really can't be seen properly, and in the illustration of Sirius, Sirius B — the subject of the article — disappears completely into the gutter! There is limited space for picture captions, which means that some details of the images are not explained, and there are no references for anyone wanting to find out more.

Although much of the book is historical, almost all of the pictures are modern. Certainly, the images from the *Hubble Space Telescope* and various planetary missions are spectacular. But without a simple picture of Saturn, it's hard to understand why observers before Huygens saw moons or ears rather than rings. There are also a good many artists' impressions, and the distinction between these and genuine science images is not always clear. I was particularly disappointed that Eugene Parker's discovery of the solar wind was illustrated by a painting of a hypothetical planet affected by a stellar wind, when we could have had a photograph of the Northern Lights.

However, this is a splendid picture-book, and the text should intrigue and enlighten anyone with even a passing interest in astronomy. — FIONA VINCENT.

The Radio Sky and How to Observe It, by J. Lashley (Springer, Heidelberg), 2011. Pp. 251, 23.5 × 18 cm. Price £31.99/\$34.95/€34.95 (paperback; ISBN 978 1 4419 0882 7).

Having spent my working life as a radio astronomer with telescope engineers and receiver experts to provide the tools that are needed, I have a very high regard for those who undertake amateur radio, rather than optical, observations of the heavens. There is very little that can be bought off the shelf so antennae and receivers must usually be crafted before use. There is no doubt that this nicely presented book will be of very great value to any who undertake this somewhat daunting task.

The book opens with chapters relating to the two strongest radio sources in the Solar System, the Sun and Jupiter. There is a very nice description of the source of the Jovian decametric radio emissions, but I was somewhat confused by a diagram illustrating the quantum tunnelling that allows the proton–proton cycle to take place in the Sun. However, the construction of suitable systems to make observations of these two objects wait until later in the book when the basic receiver-design fundamentals have been covered.

There then follows a chapter about observing meteors by the forward scatter from their ionized trails of signals from distant FM radio or TV transmissions. I was a little worried, though, that a recommended computer-controlled receiver must now be found second-hand, seemingly with its control software on two floppy disks. (I am sure that there is a work-around for this!) A section gives a comprehensive survey of the meteor showers observable month by month, along with a monthly observing record taken by radio amateur G7IZU, so completing a really excellent chapter.

Before moving on to the basic building blocks of a radio-astronomy receiver, a short chapter covers the source of radio emission received from beyond our Solar System — with a particularly nice survey of the rotating neutron stars called pulsars. There follows a substantial section on antenna theory and practice, covering simple dipoles, loop and log-periodic antennae, and even a feed horn for use in observing the 21-cm hydrogen line. This leads on to chapters about the overall design of radio receivers and an introduction to RF electronics and test equipment.

The final section of the book covers some specific projects that build on the knowledge gained in the earlier chapters. There is a simple starter project to monitor solar flare activity by its effect on the propagation of very-low-frequency transmissions. This is followed by a dish-based microwave receiver to monitor the Sun's radio emissions. This rather neatly uses a commercial low-noise amplifier and down-converter (LNB) with a modified satellite-finder acting as the receiver. The receiver output is then linked up to a data logger and computer. (The text implies the use of a serial interface cable but, should one be needed, serial to USB interface adapters are available.) A third, very interesting and not-too-difficult, project describes the building and use of the Radio Jove kit to observe the Jovian radio emissions.

The final, and most complex, of the projects presented in the book is the building of a broad-band solar radio telescope. The high-frequency — and difficult — part is covered by a commercial TV-tuner module which converts the signal down to 37.7 MHz for further down-conversion, amplification, and filtering. Again the output signal is digitized and logged by computer as described in the final chapter. This is followed by four useful appendices relating to radio-astronomy formulae, an excellent bibliography, a list of suppliers (such as for the Radio Jove kit), and finally a comprehensive glossary.

Jeff Lashley has obviously put in an immense amount of work in compiling this book and the result is a very worthwhile manual that should make it far easier for amateurs to take up this interesting area of astronomy. More than that, a colleague is seriously considering its use as a text for aspiring postgraduate radio astronomers. That is, in itself, real and deserved praise for a book that can be highly recommended. — IAN MORISON.

Practical Astronomy with your Calculator or Spreadsheet, 4th Edition,

by P. Duffett-Smith & J. Zwart (Cambridge University Press), 2011. Pp. 216, 24.5 × 20.5 cm. Price £24.99/\$40 (spiralbound; ISBN 978 0 521 14654 8).

Many years ago, I purchased a copy of the second edition of *Practical Astronomy with your Calculator*. Along with *Astronomical Formulae for Calculators* by Jean Meeus, this was the staple diet of the individual, like me, who wanted to perform his own astronomical calculations whether it was calculating sidereal times, coordinate transformations, or more complicated predictions like rise/set times or even solar-eclipse local circumstances. Before these books appeared, the subject was dominated by professional publications such as *The Astronomical Almanac*, which, by their very nature, were sources of geocentric rather than topocentric data. Nowadays, there are a plethora of software packages and books around to provide the interested amateur with this type of information for his own location.

Into this arena comes the fourth edition of *Practical Astronomy with your Calculator*. The new edition is a nicely produced book full of recipes for producing the types of information the amateur astronomer might need. Like its predecessor, the emphasis is on a recipe to do the calculation rather than presenting large amounts of description and explanation. This is a tried and tested formula in its own right and is ideally suited to the area of its latest revision — the inclusion of spreadsheets.

The spreadsheet has now become ubiquitous in modern life and Peter Duffett-Smith and his collaborator Jonathan Zwart have provided spreadsheets as the updated method of delivery for just about all the calculations in the book. Both Windows users and devotees of open-source software are catered for as spreadsheets for Microsoft EXCEL and Open Office CALC are provided

free to download on an associated web site. The use of screen shots of these spreadsheets is very useful and there is also a helpful general preamble on the use of spreadsheets for first-time users. There are a wide variety of basic spreadsheet functions which can then be combined into more complicated automated tools.

For those of you who have not used one of the previous editions of this book, there are sections on time, coordinate systems, the Sun, the planets, comets and binary systems, and the Moon and eclipses. Most of the questions posed by the amateur astronomer can be answered by using one or more of the spreadsheets provided.

The authors have tried to pitch their book more at a general readership rather than specialists. They also do not claim the algorithms to be as accurate as publications such as *The Astronomical Almanac*, but from the tests I performed they seemed to provide very acceptable results. There is also a useful glossary and list of symbols and abbreviations at the back of the book. I think this new edition will be a welcome addition to the amateur's bookshelf, bearing in mind its modest price and revised methods of calculation — especially after one has downloaded the zip files containing the pre-prepared on-line spreadsheets. — STEVE BELL.

Here and There

FAVOURABLE ALIGNMENT

The 120-seater [Royal Observatory Greenwich] planetarium's architecture cleverly reflects its astrological position ... — *Time Out: Essential London for Visitors* 2011, no. 12, p. 49.

A MAN OF MANY TALENTS

... some measurements of double stars in the 1930s by Reverend [sic] R Woolley. — *Astronomy Now*, 2011 August, p. 20.

A NEW TOOL IN THE ASTROLOGER'S BAG, NO DOUBT

Lowell Observatory in Flagstaff achieved a niche in astrological history when Clyde Tombaugh used its telescopes to discover the dwarf planet Pluto in 1930. — *Highroads* [magazine for AAA members], 2011 July/August, p. 31.