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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2013 January 11 at 16^{h} 00^{m}
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. Welcome to the Ordinary Meeting of the Society. One of the happiest jobs of the President is to announce the recipients of the Society's awards for 2013: the Astronomy Gold Medal goes to Professor Roger Blandford of Stanford University; the Geophysics Gold Medal to Professor Chris Chapman, of Schlumberger Gould Research; the Chapman Medal to Professor Steve Milan, from the University of Leicester; the Eddington Medal to Professor James Binney, of the University of Oxford; the Herschel Medal to Professor Michael Kramer, of the Max Planck Institut für Radioastronomie and the Jodrell Bank Centre for Astrophysics, Manchester; the Price Medal to Professor Kathy Whaler, of the University of Edinburgh; the Jackson-Gwilt Medal to Professor Vikram Dhillon, of the University of Sheffield; the Patrick Moore Medal to Dr. Bernie Tedd of King Edward High School for Girls, Birmingham; the Fowler Award for geophysics to Dr. Iain Hannah, from the University of Glasgow; and the Fowler Award for astronomy to Dr. Mark Swinbank of the University of Durham. The Winton Capital Award for geophysics goes to Dr. Katherine Joy of the University of Manchester, and the Winton Capital Award for astronomy to Dr. Baojiu Li of the University of Durham. The Group Awards: the geophysics award goes to the UK MHD consortium, led by Professor Alan Hood, while the astronomy award goes to the *SAURON* team led by Professor Roger Davies, Tim de Zeeuw, and Roland Bacon, from, respectively, the Universities of Oxford, Leiden, and Lyon. The Service Award goes to Professor Mike Hapgood from RAL Space. There are four new Honorary Fellows: Professor Françoise Combes from the Observatoire de Paris (astronomy); Professor Sandra Faber from the Lick Observatory (astronomy); Professor Alik Ismael-Zadeh from the Russian Academy of Sciences in Moscow (geophysics); and Professor George Miley from Leiden (astronomy). The George Darwin Lecturer is to be Professor Eline Tolstoy of Groningen University, the Harold Jeffreys Lecturer, Professor Bob White of the University of Cambridge, and the James Dungey

Lecturer, Professor Peter Cargill of Imperial College London and St. Andrews. I am happy to say that that lecture has been brought forward to coincide with this meeting, simply because it is James Dungey's 90th birthday in 19 days from today.

I'd like to start the programme, and the first speaker is Dr. Jane Greaves from the University of St. Andrews, who is speaking on 'Fitting the Kuiper Belt into the debris-disc zoo'.

Dr. Jane Greaves. [The speaker described the results of recent studies of debris discs around other stars, to place the Solar System's Kuiper Belt in context. A recent search was described for the sub-millimetre emission from the debris of comet collisions in the α Cen system, which is now known to host at least one planet; but no such material has been detected. However, a simulation of the Solar System as seen in the sub-mm from α Cen shows evidence for our own planetary system in the distribution of small particles, shepherded by the influence of the giant planets. These particles represent the end point of a collisional cascade in which larger bodies have been shattered, the fragments undergoing further collisions to yield a population of micron-to-millimetre-sized particles of grit. With temperatures of tens of Kelvin at a Pluto-like distance from the Sun, these particles are good emitters in the far-IR and millimetre wavebands, and should be able to be detected if they are present around nearby stars.

Some early results were presented from the SUNS (*SCUBA* Unbiased Nearby Stars) legacy project, in which the *SCUBA2* camera on the *JCMT* is being used to make observations of hundreds of nearby stars; the aim is at least to triple the number of systems in which debris discs are imaged at sub-mm wavelengths. The discs detected show a great diversity in brightness, in size, and in the host stars, which are all normal main-sequence field stars; some discs are found to comprise a huge amount of orbiting debris.

To highlight some recent results, the speaker also referred to a *Herschel* image and spectrum of the β Pictoris disc in which olivine is found to be present, and appearing very much like primitive materials in our own Solar System: rich in magnesium and poor in iron. This is a strong clue that at least one system is compositionally like our own. Water has also been found: the *Herschel* team working on gas in protoplanetary systems have detected water in discs a few million years old. And to highlight the similarity of disc material to our own Solar System, two IR spectra were compared which show evidence for water ice, silicates, and olivines: one spectrum was of an extrasolar debris disc, and the other the spectrum of cometary debris from the *Deep Impact* mission.

The nearby G-type star 61 Virginis has been discovered to have a system of low-mass planets, closer to their parent star than in the Solar System, and the star has also been discovered to have a much more massive debris disc: while the mass of the Solar System Kuiper Belt has been estimated by various methods to be about 10^{-5} Earth masses, the debris disc of 61 Vir contains 30 times as much material. If one ranks the debris discs of nearby stars in terms of their inferred mass, the Solar System falls in the lowest tenth percentile. While the Solar System must have once had a massive debris disc to explain the minor bodies of the Kuiper Belt, the evolution of planetary perturbations in the Solar System, according to the Nice model, appears to have perturbed and ejected about 90% of the Kuiper Belt objects, leaving the cleared low-dust-content belt that we have today. The speaker posed the question as to whether, if there were a more massive Kuiper Belt, there would be more impacts in the inner Solar System, and how would this affect, for example, the Earth's biosphere? In the

Hadean to Archaean periods, there is geological evidence for Earth impacts 1–2 orders of magnitude bigger than the K–T impact event, the kind of events which would affect greatly the Earth's atmosphere and crust, and from which the ecosystem would take longer to recover. During the late-Permian extinction event, it took some species about 30 million years to recover; multiple impact events of such magnitude could be expected to have a significant effect on the biosphere.

To explore the effect of a massive debris disc on a planetary system, the speaker described work with Sandra Jeffers, Jonti Horner, and Barrie Jones, investigating impacts in a simulated solar system containing one of the most massive debris discs detected and a realistic planet content (no Jupiter, but including Saturn and Uranus analogues). They counted the number of impacts occurring per million years: after an episode of initial stirring and most of the bodies are ejected, the impacts settle down to some tens of hits per million years on the giant planets in the system, much more than in the Solar System (resulting in an unexpectedly hot Saturn due to heating from the impacts). For a planet in an Earth-like orbit, preliminary work shows the odds against its being hit were actually pretty good, with about a 50% chance of being hit during the host star's main-sequence lifetime.

The speaker concluded by noting some statistics from the unbiassed sub-millimetre surveys: only about 10% of nearby Sun-like stars have massive debris discs; the rest have comparatively little material, which is difficult to detect with any significance in any individual case, but if the results for the 90% with no massive disc are averaged, there is something like a 3% signal excess above the stellar signal, which compares with about a 1% excess for the Sun at the same wavelengths.]

The President. Are there questions?

Dr. S. Giess. You discussed the belt systems as pre-existing around particular stars. The Earth has formed around a star that is fairly young — the Sun has been formed from recycled material — so what do you think are the chances that there could be some larger planets or planetesimals flying around in the Milky Way from what were exploded star systems? People have claimed now they have seen rogue planets — do you have any thoughts on that?

Dr. Greaves. Yes, I don't know so much about rogue planets, but there are signs of those in one of the big microlensing surveys. What I do know is that people have looked for both debris belts and planets around some of the older stars in the Galaxy, and some with quite unusual space velocities, which you can identify in the galactic thick disc which formed in the first Gy or so when the Galaxy was a very turbulent place. We haven't found any debris belts, but there are actually a couple of thick-disc stars known with giant planets, which messes up quite a lot of the theory! But it does suggest that planetary systems could occur even around old, very low-metallicity systems — you just have enough stuff by chance in the initial state to make some planets. And so there could have been potentially iron-using civilizations on planets 5 billion years before the birth of the Sun.

The President. I'm going to have to bring down the guillotine at this point and introduce the next speaker. Thank you, Jane! [Applause.] It is my pleasure to introduce Professor Chris Russell, from the University of California, Los Angeles, to speak about 'Vesta in the light of *Dawn*'.

Professor C. T. Russell. Vesta is the second-most-massive body in the main asteroid belt, and *Dawn* is NASA's ninth Discovery mission that has just completed its orbital mapping at Vesta and is now well on its way to Ceres, the most-massive asteroid. No other spacecraft has orbited a body, mapped it, and left that body for another outside of Earth orbit. This is enabled by a very efficient ion engine and is accomplished very economically. *Dawn* is less expensive than any NASA planetary mission currently operating, including all the recent Discovery missions.

I am *Dawn*'s principal investigator. When I asked NASA for the funds to undertake *Dawn* I told them that it was "a journey in space and time", that we were going to travel to Vesta and Ceres, and by doing so we would unfold the history of the Solar System. In the absence of time-machines, today we use our telescopes to look at other stars and piece together a statistical history of solar systems, compiled from stars of different ages. That is not satisfactory in many ways. In particular we want to know how our Solar System evolved. Surely it has some special history, all its own. The paradigm for solar-system evolution has been personalized for our Solar System with meteoritic data. In the case of Vesta, the HED meteorites (howardites, eucrites, and diogenites) are identified as originating on this body by way of having similar reflectance spectra in the visible and near infrared. From the meteorite data we believe that a supernova seeded the pre-solar nebula with ^{26}Al which then heated the interiors of the bodies that were condensing at the time, thereby capturing ^{26}Al's radioactively-derived heat. These melted bodies were basaltic in crustal composition. The HED evidence suggested Vesta had an iron core with a eucritic and diogenitic crust.

Dawn's first task was to test this paradigm. While we did not doubt it, some did, so it was important to strengthen the foundations of our understanding of the Solar System's origin. Using *Dawn*'s gravity measurements we could determine that there was an iron core. The near-IR data showed that the outer crust was eucrite and the lower crust was diogenite. Vesta was indeed the HED parent body. This conclusion was backed up by gamma-ray and neutron data from the *GRaND* investigation. In order to determine the Solar System history in the period between 4·6 Gy and today, we have examined the scarring of the surface by impacts. We can follow the cratering record back in time for about 4 Gy until we start to see landscape saturated with craters. This tells us that the late heavy bombardment that scoured the Moon did not affect Vesta as badly as it did the Moon. This is in part because bodies move more slowly at Vesta's distance from the Sun. Earlier than that, when Jupiter formed it caused a cratering epoch called the Jupiter Early Bombardment. In the most violent scenarios this event could have excavated tens of kilometres of Vesta's crust, but it seems not to have been that intense.

Vesta's surface is very interesting. It appears to be peppered with dark carbonaceous material. It has diverse coloration, probably associated with internal processes. Protoplanet Vesta seems to have been working hard to be like its larger siblings. Some of this we knew from telescopes at 1 AU, but the high-resolution images available in orbit have been most enlightening. Most surprising were the rimless pits that indicated that liquids had boiled off after some more recent craters were formed. The most logical liquid for this devolatization is water. We see support for this idea in the morphology of the craters.

So we have been able to travel both in space and time. We have confirmed that our ideas about the origin and evolution of our Solar System were accurate as far as they went, but we have also learned new things, things we had not expected. *Dawn*'s stay at Vesta is now over and we are now well on our way to Ceres to arrive in early 2015.

The President. Questions?

Mr. M. F. Osmaston. In your isotopic set that you've shown in your table, I didn't see ^{41}Ca, which has a half-life of a 130 000 years. There was a paper, I think in 2003, by Goswami and colleagues, who said that the relationship they found between ^{41}Ca and ^{26}Al in meteorites required that it reach our system within one million years of being produced in the relevant stellar explosion.

Professor Russell. Yes, I didn't have that on the slide — I took that slide from Hap McSween. As I pointed out, the iron meteorites first appear within that one million years, so I think that's consistent with that constraint.

The President. Thank you very much. [Applause.] We now move to the first James Dungey Lecture, from Professor Peter Cargill: 'From Flares to nanoflares: magnetic reconnection at work on the Sun'. [A summary of this talk has appeared in *Astronomy & Geophysics* **54**, 3.16, 2013.]

The President. Questions or comments?

Professor Carole Jordan. These are beautiful pictures you've shown, but what regions are they? Are they networks, active regions, or what?

Professor P. Cargill. I should have shown the whole field of view, but that's embargoed too [referring to recent work which was about to be published]. There are bits of active regions in it; there is some structure that starts out very narrow and one can see something near a sunspot at the end of an active region, and then you can see it branching out to fill the whole corona — that's what it looks like to me. Again, you can see bits of the active region, and what look like structures moving away from it. The authors see *prima facie* evidence for reconnection in their movies, I think.

Rev. G. Barber. Have we resolved the problem of active coronal heating? Do these nanoflares give enough energy?

Professor Cargill. Well, if we can get this issue resolved here, I think we will be able to say something about the magnitude, the cadence of the nanoflares, and hence something about whether there is enough energy. I think there is enough energy in these events, but we need to sort out the problem on the time-scales. It's a cautious 'yes', and in a year's time I should be able to tell you.

Professor D. Lynden-Bell. Does that final answer mean that when Levine called his paper "A new theory of the heating of the corona" that he was right?

Professor Cargill. I think it does, yes, because there is a paper by Wallace Tucker, if you remember from the year before, who opened up the same ideas, but Randy Levine was the first person to propose this scenario in detail. I hope he was right, although there are some reconnection non-believers out there.

Professor Lynden-Bell. I always thought that the reason why the solar community did not accept his paper, which they didn't for many years, was that he was supervised by David Layzer, who was a cosmologist, and nothing to do with solar physics whatsoever. [Laughter.]

Professor Cargill. Well, let's see who's in the room here [laughter] ... there were other reasons it wasn't accepted, and that is because a very significant part of that community believed in wave heating; and they are still out there. But I think the original question concerned a new theory — we're now getting to the data and we can begin to be quantitative in what we say, and that's wonderful.

Professor Lynden-Bell. But it needs another factor of two or so at present.

Professor Cargill. It needs hard work, and it needs some thinking. Of course, Randy Levine left the field a few years after that.

Professor S. Schwartz. Let me just throw in the question of how you heat an open region, because the open coronal-hole regions are as hot, if not hotter;

there is not as much stuff there, but they're hot. So are you going to do that with reconnection as well?

Professor Cargill. Well, it depends on who you ask.

Professor Schwartz. I'm asking you! [Laughter.] I'll ask Donald later tonight!

Professor Cargill. There is a lot of evidence from the *UVCS* instrument on *SOHO* that there is a range of high-frequency ion-cyclotron waves in those regions. Other people have put forward turbulent cascades, Jim McKenzie and Ian Axford proposed small-scale reconnection, so take your pick. I'll stick to active regions myself.

The President. On that uncertainty [laughter] I will draw this to a close, before it becomes embarrassing for all of us! We can relax in the drinks reception in the library of the Royal Astronomical Society across the courtyard, immediately following this meeting. And lastly, I give notice that the next monthly A&G open meeting of the Society will be on Friday, 2013 February 8.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday 2013 February 8 at $16^{h}\ 00^{m}$
in the Geological Society Lecture Theatre, Burlington House

D. J. SOUTHWOOD, *President*
in the Chair

The President. The first speaker today is the Fowler Award winner, Dr. Hiranya Peiris from UCL: 'Hunting for early Universe relics in the cosmic microwave background'.

Dr. Hiranya Peiris. With this research we are asking the question: how big is the Universe? If we combine modern themes in cosmology such as dark energy and inflation with modern themes in theoretical physics such as symmetry-breaking and extra dimensions we come to the extraordinary idea that our observed Universe might just be one small part of a large multiverse.

We know that matter and radiation dilute in an expanding universe. However, dark energy dilutes very slowly or not at all. It acts like a fluid with negative pressure and this can cause space to undergo accelerated expansion. Does this expansion ever end? If there is another phase or vacuum, yes it can. Is the phase or vacuum that we inhabit in our Universe unique? Theories of particle physics with a unique vacuum are hard to come by, and spontaneous symmetry-breaking gives rise to multiple vacua. An extreme example of this arises in string theory where compactification of extra dimensions needed for consistency of the theory gives rise to a landscape of 4D vacua of different solutions. These extra dimensions are hidden from our view and this compact space can assume many sizes and shapes. Each of these configurations gives rise to different 4D physics, and in particular they give rise to different values of the cosmological constant.

In this picture, dark energy is the energy stored in the configuration of these extra dimensions and these configurations of compact space can be deformed into one another at some energy cost, a bit like protein folding. This gives rise to a potential landscape which is very complex, where different places in the landscape correspond to different underlying compact dimensions, and you can go from one place to another at some energy cost. Dark energy is just the potential energy stored in the configuration of compact dimensions at the place in the potential where you live.

We could be stuck in a local minimum with a positive non-zero cosmological constant but it is possible to get out. This is just a phase transition rather like water turning into steam by adding heat, which happens *via* the formation of bubbles in the liquid phase; in our picture, bubble formation happens by quantum-mechanical tunnelling through potential barriers, so it is very slow and the pressure gradient is constant so the bubble grows indefinitely. In an expanding Universe, you can have accelerated expansion within the bubble ending but the space in between is still accelerating. The idea of eternal inflation occurs when the rate of bubble formation is less than the rate of expansion so that accelerated expansion never ends everywhere. If eternal inflation can happen in our future, it also could have happened in the past. Cosmic inflation is just another epoch of accelerated expansion.

So the answer to the initial question of 'how big is the Universe?' is that an eternally inflating universe is infinitely big. Current fundamental theories do not produce a unique vacuum. There is observational evidence for accelerated expansion both in the early and late Universe and these two facts strongly motivate the idea that we inhabit an eternally inflating universe. Is it experimentally verifiable? Our bubble does not evolve in isolation: soon after it formed it could have undergone collision with other bubbles, which might have left some statistical signature in the cosmic microwave background; what we would see in this case would be a long-wavelength fluctuation in the CMB and it would have azimuthal symmetry because the collision signature has the same symmetry as that of intersecting spheres. Thus, a disc-like region on the sky is affected by the collision; the region outside it is not affected. It is localized in real space, and at the transition edge of the disc-like region that is affected by the collision we do not have to have a continuous temperature of the microwave background, so that there is possibly a causal edge. We can characterize the collision using a multiplicative cosine modulation of the CMB temperature. The model has an amplitude, an angular size, and the possibility of a causal edge.

There is a hard data-analysis problem here. Firstly we should note that the CMB is a very large dataset and it is very easy to find weird features in it. The human eye is very good at detecting patterns where there are none in random data, and there is a history in the field of using *a-posteriori* statistics which promote wrong inferences. It is worth reflecting on the effect of *a-posteriori* statistics. For example, here is a simulation where we toss a coin 1000 times. I can see that there is a chain of eleven heads in a row, and if we just focussed on that, the probability of getting eleven heads in a row is 0·1%. Is the coin therefore biassed? The correct way to pose this question is to ask what is the probability of getting a chain of eleven heads somewhere within 1000 trials, and the answer is 38%. We are therefore encouraged to follow the example of the particle physicist and to perform blind analysis with no *a-posteriori* selection effects. We designed a pipeline with a model and a specific dataset such as *WMAP* data in mind. Then we tested it extensively on simulations to determine the sensitivity of the pipeline and then we calibrated it using the best instrumental simulation we can get to perform a null test to show that there are no bubble-collision signatures in the simulation. We don't want any false detections. Finally and most important, we freeze the pipeline with no free parameters before we look at the data.

Another important aspect is that we do a Bayesian analysis; typically p values are used to quantify how discrepant a data statistic is under the 'null hypothesis', but we cannot use that to perform a model selection, which is the problem at hand. In using Bayes' theorem the evidence is the model-averaged likelihood, where the exact pixel likelihood includes the CMB, spatially varying noise, and the instrument beam. We have used a daring approximation which we have demonstrated to work in practice. We conservatively approximate the full (computationally intractable) problem by assuming that the likelihood is zero outside candidate collision regions (blobs) and each blob is uncorrelated with the rest of the sky. The blobs are detected using optimal filters. This algorithm may sound complicated but in fact it is an approximation to a numerical integral. The observational signatures are parameterized as follows: we have N_s collisions each described by the parameters of size, shape, position, and amplitude. We then calibrate the effects of systematics, but we can't include all systematics in the likelihood function as some of the instrumental and processing systematics have not yet been released. We calibrate their effects using a *WMAP7 W*-band end-to-end simulation. This includes simulated time-stream data, diffuse and point-source foregrounds, and realistic instrumental and data-processing effects. The *WMAP* science team kindly supplied us with the simulation but it is now publicly available. We applied the pipeline to these simulated data and we do not get any false detections. We then freeze the pipeline and apply it to the real data which is the *WMAP* 94-GHz highest-resolution band which has already been foreground subtracted — further, the Galactic plane has been masked. We apply optimal filters and we detect eleven regions where the likelihood is high for the bubble-collision theory. Then we can use Bayes' theorem to obtain this constraint on the expected number of collisions (N_s) on the sky.

The conclusion we reach is that $N_s < 4$ at the 95% confidence level. This is the first observational constraint on the theory of eternal inflation or the theory that we could be living in a multiverse.

Coming in the next few months will be the results from *Planck*, which is ten times more sensitive and has three times the resolution of *WMAP*. Hopefully this will provide some more information about the physical origin of this very strange Universe in which we find ourselves. [Applause.]

The President. Questions or comments?

Professor D. Lynden-Bell. You've got two very non-round bubbles! Why did you assume the original bubbles were round?

Dr. Peiris. When the collision happens, the walls very quickly become co-moving and any initial perturbations in their shape are smoothed out. Actually, I'm very glad that you asked that, as it allows me to show a simulation! [Laughter.] This is a simulation in full GR. And, it's a 1+1 relativity simulation. This is space; that's time; these are co-moving coordinates. So the two bubbles are nucleated here.

Professor Lynden-Bell. Are they round initially?

Dr. Peiris. They can be non-round initially, and then they expand and eventually they become parallel to the time axis and that means they become co-moving, and the walls are moving at the speed of light. And so whatever initial perturbation there is, it's very quickly ironed-out. That's actually been shown in numerous studies, but this is the first time we've verified it in full numerical relativity.

Professor R. J. Davis. You talked about eternal inflation, so what does $N_s < 4$ mean in terms of eternal inflation?

Dr. Peiris. That's an extremely good question! The reason we parametrize eternal inflation by this expected number of collisions per sky is because there isn't any one theory of eternal inflation. There are different potential landscapes which you can have, and they give different predictions for the expected number. Like a lot of questions that can arise with this picture, I have to say that this is a question under investigation. This is what we are doing with these numerical bubble-collision simulations because we can put any shape of potential onto those simulations, and link the Lagrangian for those potentials with the observational signature. Once we get that constraint, we can then translate it into constraints on the Lagrangian, and that's the project that is currently under way.

The President. One more!

Professor W. Chaplin. Does the number that you get at the end depend in any way on the priors that you're using in your Bayesian analysis?

Dr. Peiris. I have not shown the slide on priors but of course it's really important. The main thing that is actually at issue here is that we don't have a theoretical prior on N_s itself. So, as in my previous answer, that is not uniquely predicted by theory, and that's why we have to assume a uniform prior on it. Alternatively, suppose you favoured ΛCDM, which corresponds to $N_s = 0$, much more than the bubble-collision theory, then you would peak the prior at zero and then have the prior falling off for $N_s > 0$. So all that can do is to skew the evidence towards ΛCDM and give a tighter constraint on the bubble-collision theory. So what we care about is that the limits we quote are conservative under the change of this prior.

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

I should remark that we have another Fowler Award winner speaking today. The Fowler Awards are early-career prizes, and the next speaker is the geophysics award winner: Matt Owens, from the University of Reading. The title of his talk is 'The solar cycle in the heliosphere'.

Dr. M. J. Owens. The approximately 11-year cycle in the number of sunspots visible on the Sun was first identified more than 150 years ago, by Samuel Schwabe, and has been well observed ever since. In fact, with the power of hindsight, the sunspot cycle can be near-contiguously traced back through historical records all the way to Galileo's first telescopic experiments in 1610. This is by far the longest record of direct solar observation, making it invaluable to understanding solar variability. In addition to the roughly decadal rise and fall of sunspot numbers, there are much-longer-term trends. Here, however, the record of observations must be treated with much care. In 400 years, there will have been many changes of observers, telescopic technologies, working definitions for what does/doesn't constitute a sunspot, *etc.* Even for a single observer, using a single counting method with a single telescope, one must account for declining eyesight with increasing age! This inter-calibration of sunspot records is an on-going process, but one useful approach is to count groups or clusters of sunspots, rather than individual spots. These were more easily viewable with the early telescopes and can frequently be seen with the naked eye. The group sunspot record shows a number of interesting features: the solar-cycle amplitude rose steadily from 1900 to 1950; the cycles from 1950–2000, which cover most of the space age, are the largest-amplitude cycles in the whole 400-year record, often referred to as a Grand Solar Maximum (GSM); there are two very-small-amplitude cycles from 1800 to 1820, referred to as the

Dalton minimum; the Maunder minimum from 1650–1710, contains virtually no sunspots, despite a large number of skilled solar observers at that time; solar cycle 24, which began at the end of 2009, looks set to hit maximum around mid-2013, but is likely to be the weakest cycle for approximately 100 years, suggesting the recent GSM is at an end.

Thanks to spectroscopic techniques, sunspots are now known to be strong concentrations of solar magnetic field. While the general solar surface, or 'photosphere', is a teeming mass of convection cells driven by heating from the solar interior, the magnetic fields in sunspots inhibit convection, allowing the plasma to cool and making them appear 'dark', at least relative to the rest of the solar surface. Thus the sunspot record potentially presents information about long-term changes in solar magnetism. For the space age, we can compare the photospheric magnetic-field strength with sunspot number: there is indeed a strong linear correlation, but, crucially, there is still photospheric magnetic field when there are no sunspots (the magnetic field is just in bundles too small to inhibit convection).

The solar wind is a constant outflow of solar plasma from the hot solar atmosphere, which drags the solar-wind magnetic field out into space. This forms the 'heliosphere', the region of space over which the solar magnetic field exerts a direct influence. The heliosphere extends out past the orbit of Pluto. Spacecraft launched beyond the Earth's own magnetosphere can make direct measurements of the heliospheric magnetic field (HMF). Since the start of observations in the mid-1960s, the HMF has varied in phase with sunspot number and exhibited similar cycle-to-cycle trends, though the magnitude of HMF variations are considerably smaller than those of sunspot number: the HMF solar-cycle amplitude has decreased since the mid-1980s; sunspot minimum at the start of cycle 24 (2008–2010) coincided with the lowest HMF yet observed; the HMF in 2013, sunspot maximum for cycle 24, is currently lower than the HMF at solar minimum at the start of cycle 23, again suggesting the space-age GSM is ending.

Evolution of the HMF is not smooth and continuous, but proceeds in episodic bursts. New HMF is added by large solar eruptions, called coronal mass ejections (CMEs), while old HMF is removed by magnetic restructuring close to the Sun. Observed CME rates during the space age can be used to model the HMF, providing a strong match with the spacecraft observations. Furthermore, as CME rates vary with sunspot number, such a model of the HMF can be extended back to 1610. We just don't have any Renaissance spacecraft observations with which to compare!

Proxies for the HMF, however, do allow the pre-space-age HMF to be inferred. One such proxy is the intensity of galactic cosmic rays (GCRs) reaching Earth. GCRs are near-relativistic charged particles which originate far outside the Solar System. As charged particles are deflected by magnetic fields, the HMF partially shields Earth from GCRs. When GCRs do enter the Earth's atmosphere, they collide with air molecules and create a shower of exotic fundamental particles (like CERN, but cheaper to run). Since the 1950s, GCR intensity has been measured using ground-based neutron monitors, which hit record high values in 2009. But GCRs also produce isotopes which do not naturally occur, such as carbon-14 and beryllium-10. These 'cosmogenic' isotopes are removed from the atmosphere and deposited in biomass and ice sheets, respectively, providing natural records of GCR intensity, and hence HMF strength, over the last 9400 years or so. This proxy for the HMF shows excellent agreement with the sunspot-driven model back to 1610. Both show a rise and fall of the HMF in the 20th Century and significant HMF decreases during the Dalton and Maunder minima.

By combining the proxy data and the model, it is possible to show that the solar magnetic cycle continued through the Maunder minimum, despite the lack of sunspots. Thus the solar dynamo did not 'switch off' or 'stall' at this time.

The GCR record can also be used to investigate the previous behaviour of the Sun at the end of a GSM. In the 9400-year dataset, there are 24 such GSM ends. Two of those resulted in Maunder-minimum-like conditions within 50 years, while two returned to GSM-like conditions within 50 years. But looking at how sharply the HMF is currently declining, the Maunder-minimum scenario looks more probable. We live in interesting times!

The President. That was very good fun! Questions, comments?

Professor Chaplin. Could you comment on the recent unusual solar minimum?

Dr. Owens. The end of cycle 23 was one of the longer ones on record, although on a 100-year time-scale it's not particularly unusual. And it does seem to be part of this long-term decline that started in the 1980s and is continuing. If you take an 11-year running mean through the data then you've got a nice linear decline.

Professor I. Crawford. So in a warm-to-minimum situation, I presume that the heliopause comes closer into the inner Solar System. How close does it get?

Dr. Owens. I don't think it varies significantly, because the solar-wind speed, taken on a global average, is not expected to drop significantly, even though the magnetic-field strength drops. The density drops slightly, so we might be talking ~ 20% variation in the ram pressure in the solar winds. So probably a 20 % variation in the heliopause distance, I would imagine.

Professor Davis. You said that there was some evidence that the Sun still had a cycle in the Maunder minimum. I just didn't quite catch what was the observable that showed you that?

Dr. Owens. The model! No, that's not an observable. During the space age, when sunspot numbers increased, heliospheric magnetic-field intensity has also increased: they have varied in phase. But the model says that during the Maunder minimum, the heliospheric magnetic field should actually have been highest at solar minimum. And if you look at the ice-core data — there are a few high-resolution beryllium-10 observations available during the Maunder minimum — they do have the predicted anti-phase, *e.g.*, between beryllium and auroral occurrence. Previously, because [those variables] were in anti-phase, people had said, well, maybe it's El Niño/La Niña variations, or something like that. But the model shows it was a solar effect.

Professor Davis. So it was the ice-cores actually.

Dr. Owens. Yes, it was the ice-cores. That's a much shorter way of saying it! [Laughter.]

The President. Thank you very much! Now, we've got a treat: we have the Harold Jeffreys Lecture by Professor William Chaplin, from the University of Birmingham, and the title of his talk is 'Helioseismology: the solar interior revealed'.

Professor W. Chaplin. [It is expected that a summary of the talk will appear in a future issue of *Astronomy & Geophysics*.]

The President. Questions?

Mr. M. F. Osmaston. I'm very interested in your discussion of the tachocline. What are the changes in conditions across the tachocline if you get any reflection, or refraction by it? And to do that, can you monitor the depth of the tachocline, as possibly being one of the controls of the solar cycle?

Professor Chaplin. The data are good enough now to enable one actually to measure the extent of the tachocline. It's a little bit thicker at higher latitudes than it is at the equator. And it is certainly important for trying to explain why we get solid-body-like rotation in the interior. The tachocline has to mediate the transition from differential rotation outside to something like solid-body rotation within. And you've got to have something which keeps the tachocline confined in radius — so you've got to have some mechanism which is allowing angular momentum to, if you like, diffuse latitudinally but not radially. With improvements in data quality, that is hopefully going to provide clues that will enable us to piece together some of these theoretical ideas — including angular-momentum transport by gravitational waves, mixing, and other processes — to help explain the observations.

Mr. Osmaston. Thank you!

Mr. M. Hepburn. Have you got a correlation between this differential rotation and the strength of the magnetic field? I mean, if the Sun were just a rotating body, it wouldn't have a magnetic field because of Cowling's theorem. So presumably the differential rotation is intimately connected with the generation of the field.

Professor Chaplin. The differential rotation of course plays a key role in the action of the dynamo, stretching the poloidal field into something basically toroidal. The controversial bit is how you get back from a toroidal to a poloidal configuration. Whether that's something to do with cyclonic turbulence, with magnetic field getting lots of little twists from the Coriolis force, or whether one believes it's something to do with meridional circulation, remains controversial.

The President. Thank you again! [Applause.] There is little that remains for me to do but to remind you of the drinks reception in the RAS Library immediately following the meeting; and I also give you notice that the next monthly A&G Open Meeting of the Society will be on Friday 2013 March 8, four weeks from now.

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 231: HD 936, HR 1198, HR 1360, AND HD 38750

By R. F. Griffin
Cambridge Observatories

Orbits are given for four single-lined systems whose binary natures were first recognized at Haute-Provence. HD 936, HR 1198, and HR 1360 all have periods of about 1000 days; that of HD 38750 is about half as much. The eccentricities are moderate, except that of HD 936 which is small. None of the systems appears an attractive prospect for direct resolution on the sky.

Introduction

This paper presents four more orbits for stars whose spectroscopic-binary nature was first detected by de Medeiros & Mayor[1] in 1999. Paper 229[2] outlined how, after those authors had placed a listing of all their individual radial-velocity measurements at the Centre de Données Stellaires (CDS) in 2002, 60 such stars were selected for observation at Cambridge with a view to determining their orbits; the present paper brings to 38 the number whose orbits have actually been presented, while Paper 229 gave details of three of the 60 that have proved not to be binaries after all.

HD 936

HD 936 is a 7^{m} star in Cassiopeia, about 1° north-following β Cas. It was first observed by the writer and his Ph.D. mentor, Redman, in an investigation[3] into the strengths of the luminosity-sensitive violet CN band in a lot of late-type stars. The *HD*[4] type of HD 936 is K0. The star had been noted by Nassau & Morgan in the Case survey* that looked for high-luminosity stars in the northern Milky Way; they 'sold' it to Bidelman[7], who obtained a slit spectrogram (its character and even provenance not disclosed) of it and classified it G8 II. The parallax[8] now shows its absolute magnitude to be $-1{\cdot}0^{-0{\cdot}6}_{+0{\cdot}4}$, which puts it at the upper margin of luminosity class III in Keenan & Barnbaum's diagram[9] of *Hipparcos*-based luminosities. The CN strength[3] proved to be close to the maximum found among a lot of stars classified as G8 III but well down among those called G8 II. Repeated re-classification by Keenan and collaborators in the 1980s resulted successively in types of G8 IIab[10], G8 IIb[11], and G7·5 IIab[12–14]; the second of those appears to be most accordant with the parallax-based luminosity (itself none too well determined), as well as with the CN strength, for what that is worth. The CN work[3] was only the first of a number of programmes of narrow-band spectrophotometry that were carried out at Cambridge under Redman's direction. Quite a few of the stars on the listing for those programmes lacked *UBV* photometry at the time, and Redman deputed one of his staff, Argue, to make good the deficiency on visits as a guest investigator to use small telescopes at Kitt Peak. It was he[15] who made what seem still to be the only measurements of HD 936, $V = 6^{m}{\cdot}88$, $(B-V) = 1^{m}{\cdot}12$, $(U-B) = 0^{m}{\cdot}88$. The colour indices are seen to be substantially redder than would be typical of a class III star of its type, and thereby support the idea that it is considerably more luminous. Colour indices are intrinsically larger for more-luminous stars at a given spectral type; additionally, such stars are consequently at relatively great distances that may lead to significant interstellar reddening, especially at low Galactic latitudes such as that of HD 936 (–2°·5).

Redman was additionally responsible for the first radial-velocity measurements of HD 936: in his Victoria days he obtained two spectrograms of it with the 72-inch reflector and a 1-prism spectrograph giving 90 Å mm^{-1} at Hγ. He obtained velocities[16,17] of +4 and –5 (summarized in the *Supplement to the Bright Star Catalogue*[18] as a mean of "−0"!). The dates were not published, but it has been possible to enter those observations at the head of the list of velocities in Table I from Redman's original working papers, which are in the author's possession. There is an annotation regarding the first plate, "very

*They described the plan in outline in an abstract[5] of a contribution at the 81st AAS meeting (Ottawa, 1949), but gave a more comprehensive account[6] in the proceedings of a conference held at the University of Michigan in 1951.

washy lines"; it is apparent from the handwriting that the 'very' was added as an afterthought. One velocity measurement was published by Beavers & Eitter[19] from the Fick Observatory[20] of Iowa State University. What prompted the present author to observe the star, as related in the *Introduction* above, was the set of measurements made by de Medeiros & Mayor. In their actual paper[1] those authors referred to eight velocities giving a mean of +0·14 ± 2·60 km s^{-1}, but when they came subsequently to lodge the individual values with the CDS there were 12 of them. The original eight measures dated from 1979 to 1987, but there appears to have been a recrudescence of interest in 1997/8 when the four extra ones were obtained, still well before their authors submitted their paper but presumably after they had finalized the table that constitutes most of it. Although the 12 measures are not well distributed in phase, they can, by themselves, produce an orbit that is quite similar to the one determined below.

Radial-velocity observations began at Cambridge shortly after de Medeiros & Mayor had deposited their data with the CDS; they now number 41 and are set out in Table I. They provide the main weight of the data; in the solution of the orbit they have been given full weight, the de Medeiros ones ¼, and the Victoria and Ames ones nil. All the 'outside' ones have been adjusted by +0·8 km s^{-1} in the table in an effort to place them all on the zero-point[21] commonly adopted in this series of papers. The orbit proves to have a period of just over 1000 days and a small but definite eccentricity. Its elements are given in Table V towards the end of this paper, with those of the other stars treated below, and the orbit is plotted in Fig. 1.

In an orbit of low eccentricity the epoch of periastron is quite uncertain merely because its longitude is not well defined. In the case of HD 936, the standard error of the eccentricity is about an eighth of the eccentricity itself, so the longitude of periastron is necessarily uncertain by about an eighth of a radian (over 7°), which in turn makes the epoch uncertain by an eighth of $P/2\pi$ (about 20 days). That does not imply, however, that there is anything like such an uncertainty in the positioning of the velocity curve along the time axis. It is useful in such a case — indeed, it could be argued that it would be useful in almost *any* case, since $(P/2\pi)\varepsilon(e)/e$ almost always provides the principal contribution to the uncertainty of T — to give in addition the epoch T_0, the time of maximum velocity of recession. That is added as a footnote in Table V, where it is seen that what is in one sense the real uncertainty of T is less than a day — 25 times smaller than that given by the straightforward orbital solution.

If the primary star were supposed to have a mass of 2 $M_\odot$, the companion would be required by the mass function to be not less than 0·9 $M_\odot$ — so not later than mid-G if it is a main-sequence star. If the primary's higher luminosity encouraged us to suppose that it has a mass of say 4 $M_\odot$, then the secondary would have to be at least 1·35 $M_\odot$ — about F5 or above. In either case it could easily be so much fainter than the primary as not to be observable in radial-velocity traces. Being two or three times less massive than its primary, it would be separated from that star by three or four times the distance of $a_1 \sin i$ shown in Table V, or something like 3 AU, so the apparent angular separation would be limited to about three times the parallax or 0″·007 — not an attractive proposition for direct resolution, especially in view of the likelihood of a very substantial Δm.

The lines in the spectrum of HD 936 are very noticeably broadened; the quantification of the broadening in the Cambridge traces repeats very well, producing a mean $v \sin i$ estimate of 8·15 ± 0·12 km s^{-1}, although the standard error is to be taken only as a measure of the internal agreement of the 41

TABLE I

Radial-velocity observations of HD 936

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

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
1929 Sept. 15·47*	25869·47	+4·7	$\overline{27}$·967	−3·4
1930 Sept. 25·36*	26244·36	−4·6	$\overline{26}$·339	−4·7
1979 Dec. 12·79†	44219·79	+7·6	$\overline{8}$·192	+0·1
17·76†	224·76	+7·6	·197	+0·3
1980 Aug. 31·08†	44482·08	−5·5	$\overline{8}$·453	−0·5
Nov. 18·84†	561·84	−6·4	·532	+0·6
1983 Nov. 21·12‡	45659·12	−6·8	$\overline{7}$·622	+0·3
1986 Aug. 18·14†	46660·14	−6·3	$\overline{6}$·616	+0·9
Nov. 8·89†	742·89	−5·1	·698	+0·3
1987 Aug. 13·10†	47020·10	+8·6	$\overline{6}$·973	+0·3
13·99†	020·99	+8·0	·974	−0·3
1997 Aug. 30·02†	50690·02	−7·5	$\overline{2}$·618	−0·3
Sept. 18·97†	709·97	−7·3	·638	−0·4
Dec. 14·81†	796·81	−4·6	·724	−0·2
1998 Aug. 18·11†	51043·11	+7·8	$\overline{2}$·969	−0·3
2002 Sept. 2·08	52519·08	−4·3	0·435	0·0
Oct. 22·02	569·02	−5·9	·484	+0·1
2003 Jan. 5·88	52644·88	−7·4	0·560	−0·1
Feb. 13·82	683·82	−7·7	·598	−0·4
Mar. 15·79	713·79	−7·5	·628	−0·4
May 24·09	783·09	−5·4	·697	0·0
June 21·08	811·08	−4·3	·725	+0·1
July 14·08	834·08	−3·1	·747	+0·3
Aug. 9·10	860·10	−2·1	·773	+0·1
Sept. 11·02	893·02	−0·5	·806	−0·1
Oct. 12·01	924·01	+1·4	·837	+0·1
Nov. 5·94	948·94	+2·9	·862	+0·2
Dec. 5·92	978·92	+4·4	·891	0·0
2004 Jan. 2·85	53006·85	+5·9	0·919	0·0
Feb. 7·80	042·80	+7·8	·955	+0·2
Mar. 1·78	065·78	+8·2	·978	−0·3
July 3·08	189·08	+9·8	1·100	−0·2
Aug. 17·15	234·15	+9·2	·145	+0·1
Sept. 14·06	262·06	+8·1	·173	−0·2
Nov. 4·99	313·99	+6·4	·224	+0·3
Dec. 16·84	355·84	+4·1	·266	+0·1
2005 Jan. 8·82	53378·82	+3·1	1·288	+0·3
Feb. 8·79	409·79	+1·1	·319	−0·1
May 12·12	502·12	−3·2	·411	+0·1
June 23·07	544·07	−4·8	·453	+0·2
July 20·10	571·10	−5·5	·479	+0·3
Aug. 15·13	597·13	−6·4	·505	+0·1
Sept. 8·06	621·06	−6·9	·529	0·0
2006 Jan. 25·82	53760·82	−6·0	1·668	+0·3

TABLE I (*concluded*)

Date (UT)	*MJD*	*Velocity* *km s⁻¹*	*Phase*	*(O – C)* *km s⁻¹*
2007 Jan. 20·79	54120·79	+9·7	2·025	−0·1
Mar. 3·82	162·82	+10·2	·067	0·0
Dec. 10·92	444·92	−0·7	·347	−0·4
2008 Jan. 16·84	54481·84	−2·2	2·384	−0·1
2010 Feb. 1·78	55228·78	+9·8	3·126	+0·2
July 30·11	407·11	+2·2	·303	+0·1
2011 Oct. 7·07	55841·07	−4·1	3·734	−0·1
Dec. 3·88	898·88	−1·5	·791	−0·3
2012 Jan. 2·76	55928·76	+0·5	3·821	+0·1
26·79	952·79	+1·7	·845	−0·1
Mar. 1·77	987·77	+3·7	·880	−0·1
Aug. 15·11	56154·11	+10·2	4·045	+0·2
2013 Mar. 2·77	56353·77	+4·8	4·243	−0·4

*Observed by Redman[16,17]; weight 0.
†*Coravel* observation[1] from CDS; wt. ¼.
‡Ames photoelectric observation[19]; wt. 0.

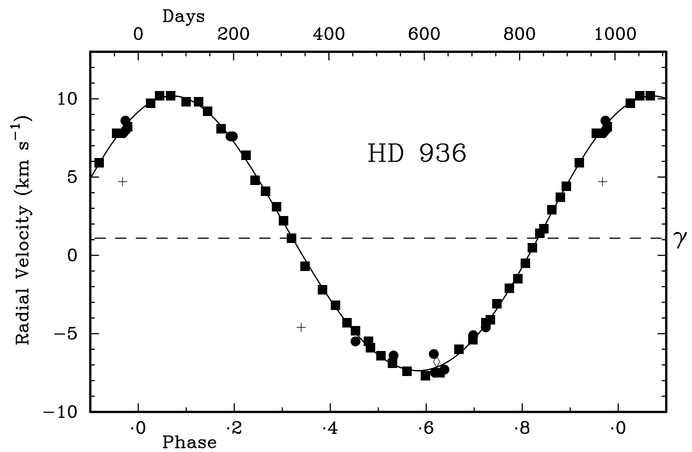

FIG. 1

The observed radial velocities of HD 936 plotted as a function of phase, with the velocity curve corresponding to the adopted orbital elements drawn through them. The orbit depends on the observations plotted with filled symbols — squares for Cambridge, circles for the Haute-Provence ones that were deposited at the CDS by de Medeiros & Mayor[1]. Not used in the solution were the two observations made at the DAO by Redman[16,17] (plusses) and one from Ames by Beavers & Eitter[19] (open diamond).

traces and not as a true external error. It agrees exactly with the value given by de Medeiros & Mayor from the Haute-Provence *Coravel*, of 8·1 ± 1 km s⁻¹. It is not possible to identify the reason for the broadening. High-luminosity stars exhibit broadening caused by 'turbulence' of some sort, but it seems unlikely that HD 936 is sufficiently luminous for that to be the explanation. Equally, attribution to (pseudo-)synchronous rotation appears implausible for the same reason, since it would imply a projected stellar radius of about 160 $R_{\odot}$. The broadening has therefore to be regarded at present simply as an observational fact. It is probably not what galvanized Redman's comment about 'washy lines', however, since it is much too small to have been apparent on spectrograms of low dispersion. In fact the lines in HD 936 are of reasonable strength for its spectral type, the mean 'equivalent width' of the dips seen in radial-velocity traces being about 5·5 km s^{-1}, much the same as those of the (K0 III) Hyades giants.

HR 1198 (HD 24240)

HR 1198 is a 6^{m} star to be found about 2° north-following δ Per. There appears to be only one set of its broad-band magnitudes available for it, and only one MK classification. The magnitudes were measured in the Crimea[22] in the 1950s, and subsequently re-reduced and later even re-published anew by Rybka[23,24], as $V = 5^{m}{\cdot}76$, $(B-V) = 1^{m}{\cdot}05$, $(U-B) = 0^{m}{\cdot}95$. The MK type of K0 III was given by Appenzeller[25].

The star featured in three of the early Mount Wilson papers. Soon after the development of the method of 'spectroscopic parallaxes', in 1921 Adams[26] listed its absolute magnitude as $+0^{m}{\cdot}8$ and the corresponding parallax as $0''{\cdot}009$ — extraordinarily close to the (revised[8]) *Hipparcos* value of $0''{\cdot}00828 \pm 0''{\cdot}00038$. The spectral type was estimated at K0 (as it is in the *HD*). In a much larger work in 1935, Adams *et al.*[27] gave the M_V as $+0^{m}{\cdot}2$, the parallax as $0''{\cdot}007$, and the type as K2. Meanwhile, they (Adams & Joy[28]) had published the radial velocity of HR 1198 as +8·5 ± 0·9 km s^{-1} ('probable error') from four plates. Long afterwards, Abt[29] published the individual velocities, with dates, enabling Table II here to include those observations; we note with approval that they do give the mean and 'probable error' that Adams & Joy reported.

There are just two other radial velocities of HR 1198 to be found in the literature — those that led de Medeiros & Mayor[1] to assert that the object is a spectroscopic binary. They gave the mean value as +8·94 ± 2·65 km s^{-1}, and since there were only two velocities the same expression should give the individual values, as +11·59 and +6·29 km s^{-1}. The actual values given in the listing that those authors deposited later with the CDS, however, were +11·97 and +6.69 km s^{-1}, and it is those that have been entered in Table II, after the usual adjustment (equally applied to the analogous observations of the other three stars treated in this paper) of +0·8 km s^{-1} to account for the difference in zero-point with respect to the one adopted[21] at Cambridge. The present writer's observations occupy the rest of the table; they number 48 over the interval 2002–2013. The two measures from the CDS have been given half-weight in the solution of the orbit, which is illustrated in Fig. 2 and whose elements appear in Table V below. The orbital period is just over 1000 days, like that of HD 936, but the eccentricity is much higher, at 0·48. Although the Mount Wilson radial velocities are plotted in Fig. 2, it is obvious that they could not usefully be included in the solution, their variance being about 500 times that of the Cambridge photoelectric observations; the fact that they were all obtained quite close together in time (four months overall) is also apparent.

The mass function is very small and demands a minimum of 0·5 $M_{\odot}$ (the mass of a main-sequence star with a type of about M0) for the mass of the secondary if the primary is deemed to have a mass of 2 $M_{\odot}$. If the companion

TABLE II

Radial-velocity observations of HR 1198

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

Date (UT)	*MJD*	*Velocity* $km\ s^{-1}$	*Phase*	*(O–C)* $km\ s^{-1}$
1915 Oct. 27·38*	20797·38	+6·8	$\overline{31}$·938	−3·2
Nov. 24·32*	825·32	9·1	·965	+0·1
1916 Jan. 24·17*	20886·17	12·9	$\overline{30}$·024	+5·5
Feb. 24·16*	917·16	8·3	·054	+0·9
1988 Aug. 30·10†	47403·10	12·8	$\overline{5}$·669	−0·3
1989 Oct. 23·14†	47822·14	7·5	$\overline{4}$·074	−0·1
2002 Sept. 28·10	52545·10	13·2	0·642	+0·1
Oct. 28·10	575·10	13·2	·671	+0·1
2003 Jan. 5·03	52644·03	13·1	0·738	0·0
Mar. 15·88	713·88	12·9	·805	+0·2
Aug. 31·14	882·14	8·9	·968	0·0
Sept. 24·15	906·15	8·1	·991	0·0
Oct. 18·10	930·10	7·4	1·014	−0·1
Nov. 13·05	956·05	7·3	·040	0·0
Dec. 21·85	994·85	7·5	·077	−0·2
2004 Jan. 29·85	53033·85	8·5	1·115	+0·1
Feb. 27·88	062·88	9·0	·143	+0·1
Apr. 6·83	101·83	9·6	·181	0·0
Sept. 5·14	253·14	11·5	·327	0·0
Oct. 7·13	285·13	11·8	·358	+0·1
Nov. 5·04	314·04	12·1	·386	+0·2
Dec. 17·97	356·97	12·3	·427	+0·1
2005 Jan. 13·87	53383·87	12·2	1·453	−0·2
Feb. 8·82	409·82	12·6	·478	+0·1
Mar. 18·84	447·84	12·7	·515	0·0
Apr. 6·84	466·84	12·9	·534	+0·1
Sept. 8·18	621·18	12·9	·683	−0·2
Nov. 25·03	699·03	13·0	·758	0·0
2006 Jan. 25·85	53760·85	12·7	1·818	+0·1
Apr. 8·85	833·85	11·3	·889	−0·2
July 25·11	941·11	8·2	·992	+0·2
Sept. 11·18	989·18	7·2	2·039	−0·1
Oct. 25·06	54033·06	8·0	·081	+0·2
2007 Feb. 14·86	54145·86	9·5	2·190	−0·3
Mar. 26·85	185·85	10·3	·229	−0·1
Sept. 26·15	369·15	12·1	·406	0·0
Oct. 19·10	392·10	12·3	·428	+0·1
2008 Mar. 7·89	54532·89	12·9	2·565	0·0
30·83	555·83	12·9	·587	−0·1
Apr. 23·84	579·84	12·7	·610	−0·3
Oct. 17·10	756·10	13·2	·780	+0·3
Dec. 26·96	826·96	12·2	·849	−0·1
2009 Jan. 18·94	54849·94	11·7	2·871	−0·2
Mar. 4·84	894·84	10·9	·915	+0·1
Apr. 8·83	929·83	9·4	·948	−0·2
Dec. 22·91	55187·91	+10·2	3·198	+0·3

TABLE II (*concluded*)

Date (UT)		*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2010	Feb. 17·85	55244·85	+10·4	3·253	−0·3
	Apr. 8·84	294·84	11·0	·301	−0·2
2011	Oct. 7·13	55841·13	12·6	3·830	+0·1
	Nov. 18·03	883·03	11·9	·870	0·0
2012	Feb. 7·79	55964·79	9·9	3·949	+0·3
	Mar. 7·83	993·83	8·5	·977	0·0
	Sept. 13·19	183·19	9·4	·161	+0·1
2013	Jan. 31·80	56323·80	+11·2	4·297	0·0

*Mount Wilson observation[28,29]; weight 0.
†*Coravel* observation[1] from CDS; weight ½.

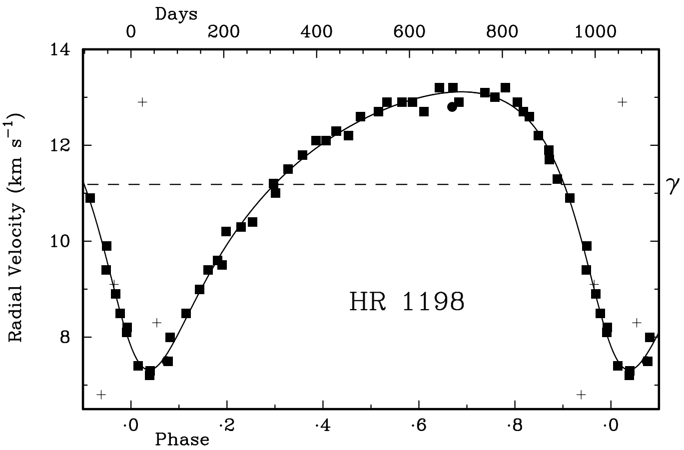

FIG. 2

As Fig. 1, but for HR 1198. Here the plusses represent the only measurements other than those from Cambridge and Haute-Provence, and show the four that were published from Mount Wilson as a mean by Adams & Joy[28] and individually by Abt[29]. They were not included in the solution of the orbit.

is indeed near that minimum, its distance from the primary would be about five times the radius of the primary's orbit, which the orbital elements show would amount only to about 1·25/sin i AU. That would subtend an angle of only about 10 milliseconds of arc if favourably orientated athwart the line of sight, so the system cannot be recommended as a good prospect for direct resolution on the sky.

The primary star has an indeterminately small rotational velocity. Among the 48 Cambridge traces, each of which furnishes an estimate of $v \sin i$ quantized in half-km s^{-1} steps, 11 give values of zero, 17 give ½, and the other 20 populate a tail of values extending up to an extreme of 3½. The mean is 1·1, but it is biassed positive-wards because of the large contingent of zero values, negative ones being of course impermissible, so the best we can do is to say that $v \sin i$ must be less than 1 km s^{-1}.

HR 1360 (HD 27497)

HR 1360, another star of about the sixth magnitude, is about 10° south of the Hyades, or (much more accurately) 4° following ν Tau. Like HD 936, it had its broad-band magnitudes measured by Argue[15], but unlike the former star it had already been measured by Cousins[30] from South Africa, and several additional sets of observations[31–33] are now to be found in the literature. The results are in excellent mutual accord, all equal to or very close to $V = 5^{m}\cdot76$, $(B-V) = 0^{m}\cdot92$, $(U-B) = 0^{m}\cdot67$. Those exact values are also given in two papers[34] by Cutispoto, who used HR 1360 as the photometric comparison star in investigations of HD 26913 (V891 Tau), a 7^{m} BY Dra variable a little over 1° preceding, but it is not clear whether he *determined* his magnitudes for HR 1360 or merely quoted them from somewhere else.

The spectral type of HR 1360 was first classified on the MK system by Cowley & Bidelman[35] as G8 III–IV, and that is the type recorded in the *Bright Star Catalogue*; Harlan[36], and also Fekel & Watson[37], later gave it as G8 III. Eggen[38] obtained *DDO*-type narrow-band photometry from which he deduced $M_V(DDO) = +1^{m}\cdot85$, and thence a distance modulus of $4^{m}\cdot34$, although how he got from the first number to the second is a question that is now difficult to resolve. The modulus appears in Eggen's paper in Table 15, which has a column, not mentioned in the description of the table, that is headed V_0. It has an entry of 6·19 for HR 1360, which can be identified as the sum of 1·85 and 4·34; but surely it cannot be intended (as one might otherwise suppose) to represent the apparent magnitude corrected for interstellar absorption, since it is considerably *fainter* than the observed magnitude. Besides, the same table gives an estimate of reddening as $0^{m}\cdot012$ in $(b-y)$, which is very small but at least it is not negative. The distance modulus stemming from the revised *Hipparcos* parallax[8] is $5^{m}\cdot22 \pm 0^{m}\cdot12$; the corresponding absolute magnitude is $+0^{m}\cdot54$ (interstellar absorption neglected at $b \sim -30°$). Adams *et al.*[27] did well to determine an M_V of $+0^{m}\cdot7$ for HR 1360 nearly 80 years ago.

Other items gleaned from the literature on HR 1360 include a 1-magnitude infrared excess measured by *IRAS* at 60 μm and attributed by Zuckerman *et al.*[39] to orbiting dust. Fekel & Watson[37] found a logarithmic lithium (log ε(Li)) abundance of +0·5 on the usual scale with log ε(H) as +12. Jasniewicz *et al.*[40] also refer to lithium and appear to give it as < 0·4. Their remarks are difficult to follow, because they refer first to 29 stars, among which they find relatively high lithium abundances (> +1) for eight, and then they refer to "the other 22", which is more than there actually *are*, saying that they *all* have Li abundances < 0; but they tabulate abundances for 15 (only) of the 29 stars, and while eight of the 15 are indeed 1 or greater, there are others that are between 0 and 1 and still others (including HR 1360) whose upper limits are more than 0.

Christie & Wilson, in a 1938 paper[41], were the first to give a radial velocity for HR 1360, a mean of $+7\cdot4 \pm 1\cdot2$ ('probable error') km s^{-1}, from six Mount Wilson plates, probably including those from which Adams *et al.*[27] estimated the absolute magnitude. The six velocities were subsequently published individually by Abt[29] and feature at the head of Table III here. Sixty years after Christie & Wilson, Fekel & Watson[37] gave three velocities that they obtained with the 38-inch coudé-feed auxiliary to the spectrograph of the Kitt Peak 84-inch reflector, though two of them were obtained on consecutive nights; and just the following year, de Medeiros & Mayor stepped in with a mean of $3\cdot84 \pm 1\cdot56$ km s^{-1} from four measurements with the OHP *Coravel*. When those authors deposited their individual measures with the CDS, the four had grown to 11 (although all had been obtained before the paper that said four had been

TABLE III

Radial-velocity observations of HR 1360

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

Date (UT)	*MJD*	*Velocity* *km s^{-1}*	*Phase*	*(O – C)* *km s^{-1}*
1929 Jan. 30·13★	25641·13	+12·8	$\overline{28}$·610	+6·4
1930 Sept. 9·49★	26228·49	+1·1	$\overline{27}$·212	+1·0
1931 Aug. 26·52★	26579·52	+10·5	$\overline{27}$·571	+4·8
Oct. 2·41★	616·41	+8·4	·609	+2·0
1932 Sept. 13·47★	26963·47	+5·0	$\overline{27}$·964	–1·8
Oct. 16·52★	996·52	+11·6	·998	+6·7
1986 Nov. 18·01†	46752·01	+0·4	$\overline{6}$·234	0·0
1987 Sept. 29·04†	47067·04	+6·1	$\overline{6}$·557	+0·6
1996 Oct. 30·04†	50386·04	+6·9	$\overline{3}$·957	–0·3
Nov. 2·00†	389·00	+6·7	·960	–0·3
13·32‡	400·32	+6·7	·971	+0·3
1998 Jan. 18·29‡	50831·29	+3·2	$\overline{2}$·413	+0·2
19·24‡	832·24	+3·3	·414	+0·2
Feb. 6·76†	850·76	+2·9	·433	–0·5
7·81†	851·81	+3·4	·434	0·0
11·77†	855·77	+3·5	·438	0·0
Oct. 11·09†	51097·09	+7·4	·685	–0·2
12·09†	098·09	+7·3	·686	–0·4
13·11†	099·11	+7·7	·687	0·0
14·13†	100·13	+7·3	·688	–0·4
2002 Sept. 30·16	52547·16	–0·1	0·171	0·0
Nov. 2·13	580·13	+0·2	·204	+0·1
2003 Jan. 5·99	52644·99	+0·8	0·271	0·0
7·00	646·00	+0·6	·272	–0·2
Mar. 16·83	714·83	+1·6	·342	–0·3
Sept. 24·16	906·16	+5·2	·538	0·0
Oct. 18·14	930·14	+5·3	·563	–0·3
Nov. 13·10	956·10	+6·0	·589	0·0
Dec. 16·05	989·05	+6·4	·623	–0·2
2004 Jan. 16·97	53020·97	+7·1	0·656	–0·1
Feb. 23·83	058·83	+7·8	·695	0·0
Sept. 6·19	254·19	+9·3	·895	+0·2
Oct. 19·16	297·16	+8·0	·939	+0·1
Nov. 5·05	314·05	+7·3	·956	+0·1
Dec. 17·01	356·01	+4·8	·999	–0·1
2005 Jan. 2·04	53372·04	+3·6	1·015	–0·3
12·93	382·93	+3·5	·027	+0·2
Feb. 8·86	409·86	+2·1	·054	+0·1
Aug. 22·17	604·17	+0·4	·253	–0·2
Nov. 4·11	678·11	+1·9	·329	+0·2
Dec. 17·01	721·01	+2·5	·373	+0·1
2006 Jan. 28·87	53763·87	+3·1	1·417	0·0
Nov. 29·04	54068·04	+8·2	·728	–0·1

TABLE III (*concluded*)

Date (UT)	*MJD*	*Velocity km s⁻¹*	*Phase*	*(O – C) km s⁻¹*
2007 Jan. 11·89	54111·89	+9·0	1·773	+0·1
Feb. 7·83	138·83	+9·3	·801	+0·1
Mar. 3·84	162·84	+9·4	·826	0·0
Nov. 1·16	405·16	+1·2	2·074	0·0
Dec. 6·01	440·01	+0·4	·109	+0·1
2008 Jan. 5·96	54470·96	0·0	2·141	0·0
Nov. 26·07	796·07	+4·2	·474	+0·1
Dec. 26·97	826·97	+5·0	·506	+0·4
2009 Dec. 23·01	55188·01	+9·3	2·876	0·0
2010 Mar. 6·78	55261·78	+7·4	2·951	0·0
Nov. 27·04	527·04	+0·2	3·223	0·0
2011 Jan. 31·89	55592·89	+1·0	3·290	–0·1
Sept. 14·20	818·20	+5·1	·521	+0·2
Dec. 5·98	900·98	+6·3	·606	0·0
2012 Jan. 3·91	55929·91	+7·0	3·636	+0·2
Nov. 29·09	56260·09	+6·2	·974	–0·1
Dec. 10·10	271·10	+5·8	·985	+0·1
2013 Jan. 9·93	56301·93	+3·9	4·017	0·0
31·88	323·88	+2·6	·039	–0·1
Feb. 27·82	350·82	+1·5	·067	0·0

*Mount Wilson observation[41,29]; weight 0.
†*Coravel* observation[1] from CDS; weight ½.
‡Kitt Peak observation[37]; weight ½.

submitted for publication), but because they were bunched in observing runs the number of really independent epochs is only five. Anyway, they galvanized the writer into putting the star onto his observing programme, in the course of which he has made 43 measurements with the Cambridge *Coravel*; they are set out, together with all the published velocities (to which the usual of adjustment of +0·8 km s^{-1} has been made) in Table III. In the case of this star, the number of OHP velocities is large enough to demonstrate a significant discrepancy in zero-point between them and the Cambridge data; it has been removed by making an empirical correction of –0·5 km s^{-1} to the latter. It is of course known and admitted that the *Coravel*s have colour-dependent zero-points, which in the OHP case are corrected[42] at source according to an undisclosed recipe; the situation regarding the Cambridge instrument is not well organized, but the correction made in this case is not much out of line with expectation according to previous experience[43].

In the solution of the orbit the Cambridge velocities have been given full weight, the OHP[1] and Fekel & Watson[37] velocities half, and the old Mount Wilson photographic ones[40] zero weight (their variance is nearly a thousand times greater than that of the Cambridge velocities). Just like the two stars treated above, HR 1360 proves to have an orbital period near to 1000 days; the eccentricity in this case is 0·3, intermediate between those of HD 936 and HR 1198. The complete set of elements is given in Table V below, and the orbit is portrayed in Fig. 3.

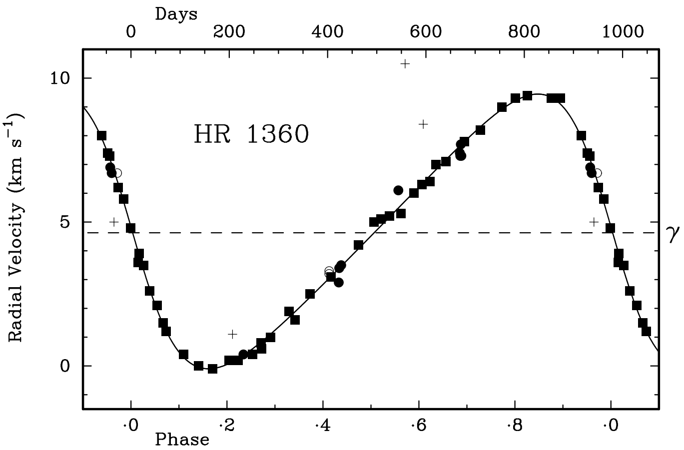

FIG. 3

As Fig. 2, but for HR 1360. Again there are zero-weighted observations from Mount Wilson, this time from Christie & Wilson[41] and given later in detail by Abt[29]. There are also three measurements made at Kitt Peak by Fekel & Watson[37]; they are represented by open circles and included in the solution of the orbit with half-weight, like the 11 Haute-Provence ones.

The mass function demands a secondary with a mass of no more than 0·4 $M_{\odot}$, corresponding to that of a main-sequence star of type M2, if the primary is supposed to be about 2 $M_{\odot}$. If the secondary is close to the minimum mass, it would have a projected distance ($(a_1 + a_2) \sin i$) of some 2 AU from the primary; in favourable circumstances the angular separation on the sky could be about 0″·02 — not an encouraging prospect for a pair whose Δm may be up to 8 magnitudes or so (a factor of more than a thousand).

HD 38750

This 7^{m} star is in the extreme east of Taurus, about 1° north of 132 Tau and little more than 3° north-preceding the position of the (northern summer) solstice. It has featured surprisingly rarely in the literature. There are measurements of its V and B magnitudes (only) by Bakos[44], at $V = 7^{m}{\cdot}20$, $(B - V) = 1^{m}{\cdot}44$, and an MK classification of K2 II made on plates (probably of 66 Å mm^{-1}) taken with the 74-inch reflector at the David Dunlap Observatory. The classification was made by Mrs. Gaizauskas there, but published under the name of the Director, Heard[45]. The parallax[8] of only 2·52 ± 1·04 milliseconds of arc translates to a distance modulus of $8{\cdot}00^{+1{\cdot}1}_{-0{\cdot}8}$ magnitudes and thus to an absolute magnitude in the range 0 to −2 (for limits of only ±1σ). Thus it is *consonant* with the classification of luminosity class II, but does not *demonstrate* that the star is more luminous than a normal giant. Another indication of high luminosity, however, is that the colour index is substantially larger than would normally correspond to a type of K2 among class III giants.

Heard's paper[45] also referred to the first radial-velocity measurements made of HD 38750: there were six of them, but the result was given only as a mean, −6·6 km s^{-1} with a 'probable error' of 1·9 km s^{-1}, from which we might deduce that the r.m.s. spread of the six observations was about 6·4 km s^{-1}. That may seem nowadays to be pretty bad for observations that were reported as taking

TABLE IV

Radial-velocity observations of HD 38750

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 Nov. 20·10*	47119·10	−8·3	$\overline{10}$·425	−0·1
1989 Oct. 23·21*	47822·21	−11·2	$\overline{9}$·698	+0·1
1992 Mar. 24·94*	48705·94	−5·5	$\overline{7}$·298	+0·1
1993 Jan. 24·95*	49011·95	−9·5	$\overline{7}$·852	+0·3
2002 Sept. 30·21	52547·21	−4·8	0·254	−0·2
Dec. 11·18	619·18	−7·2	·384	+0·2
2003 Jan. 10·09	52649·09	−8·3	0·438	+0·1
Feb. 13·93	683·93	−9·3	·501	+0·1
Mar. 16·91	714·91	−10·0	·558	+0·2
Apr. 7·84	736·84	−10·7	·597	−0·1
Sept. 24·21	906·21	−8·0	·904	+0·1
Oct. 18·18	930·18	−6·2	·947	0·0
Nov. 13·13	956·13	−4·6	·994	−0·4
Dec. 8·05	981·05	−2·8	1·040	−0·1
2004 Jan. 9·04	53013·04	−2·0	1·097	+0·1
Feb. 9·01	044·01	−2·4	·154	+0·2
Apr. 13·85	108·85	−5·1	·271	−0·1
Sept. 16·16	264·16	−10·3	·552	−0·2
Oct. 27·15	305·15	−10·9	·626	0·0
Dec. 18·11	357·11	−11·4	·720	−0·1
2005 Jan. 22·04	53392·04	−11·0	1·784	0·0
Feb. 12·91	413·91	−10·3	·823	+0·1
Mar. 17·89	446·89	−9·1	·883	−0·3
Sept. 17·19	630·19	−3·8	2·215	−0·1
Nov. 4·16	678·16	−5·3	·302	+0·4
25·11	699·11	−6·5	·340	0·0
2006 Feb. 8·90	53774·90	−9·0	2·477	0·0
Apr. 3·86	828·86	−10·5	·575	−0·1
Nov. 17·15	54056·15	−4·3	·986	+0·2
Dec. 9·12	078·12	−2·9	3·026	+0·2
2007 Feb. 1·85	54132·85	−2·1	3·125	+0·1
Apr. 7·86	197·86	−4·4	·243	0·0
Nov. 24·10	428·10	−10·9	·660	+0·2
Dec. 8·11	442·11	−11·4	·685	−0·2
2008 Feb. 11·01	54507·01	−10·9	3·803	−0·2
Mar. 17·93	542·93	−9·3	·868	0·0
30·87	555·87	−8·6	·891	0·0
Apr. 17·84	573·84	−7·1	·924	+0·2
Nov. 23·13	793·13	−6·2	4·321	−0·1
2009 Jan. 14·01	54845·01	−8·1	4·415	−0·1
Feb. 10·97	872·97	−8·8	·465	+0·1
Mar. 20·86	910·86	−9·7	·534	+0·2
2010 Oct. 20·20	55489·20	−10·6	5·581	−0·2

TABLE IV (*concluded*)

Date (UT)			MJD	Velocity km s^{-1}	Phase	(O–C) km s^{-1}
2011	Jan.	18·97	55579·97	−11·1	5·746	+0·1
	Oct.	16·18	850·18	−4·5	6·235	−0·3
2012	Jan.	13·94	55939·94	−7·6	6·397	0·0
	Sept.	13·20	56183·20	−10·1	·838	0·0
	Nov.	3·16	234·16	−7·0	·930	0·0
		18·13	249·13	−5·9	·957	−0·1
		29·14	260·14	−4·9	·977	0·0
	Dec.	10·11	271·11	−4·0	·997	+0·1

**Coravel* observation[1] from CDS; weight 1.

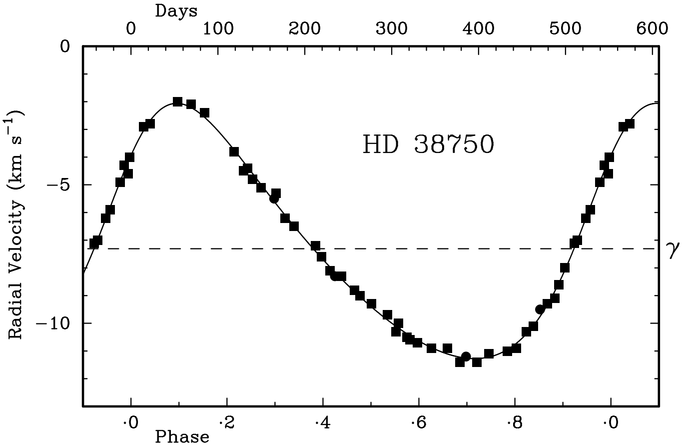

FIG. 4

As Fig. 2, but for HD 38750. In this case there are no measurements other than those from Cambridge and Haute-Provence.

between half an hour and two hours on the 74-inch telescope, but it does not necessarily imply real variability — there are ten worse values of the 'probable error' of the means among the 45 stars on the same page of the listing as HD 38750. The only other radial velocities published for the star appear to be those of de Medeiros & Mayor[1], who reported that they had four measures that gave a mean of $-10{\cdot}24 \pm 1{\cdot}15$ km s^{-1}; it was those four, when they were made accessible individually through the CDS in 2002, that led to the appearance of the star on the writer's radial-velocity programme. There are now 47 Cambridge observations to add to the original four; all are set out in Table IV. The orbit has been solved with uniform weighting for all the velocities; it proves to have a period of about 18 months (already accurate to about four hours) and a modest eccentricity of 0·23. It is plotted in Fig. 4, and the full set of elements has been included in Table V here. The very small mass function demands a mass of no more than 0·3 $M_\odot$ for the secondary if the principal star is supposed to be of 2 $M_\odot$; even if the primary is a luminous star of 4 $M_\odot$ the secondary still does not have to be as much as half a solar mass, so even in that case it could still be an M dwarf, and the separation of the components could not be as much as 2 AU (0″·005, at best, in angular terms on the sky).

TABLE V

Orbital elements for the four stars

Element	*HD 936*	*HR 1198*	*HR 1360*	*HD 38750*
P (days)	1006·9 ± 0·4	1034·0 ± 1·1	976·3 ± 0·4	552·25 ± 0·17
T (MJD)	53088* ± 21	53949·1 ± 3·7	53356·9 ± 3·8	53511·5 ± 2·8
γ (km s^{-1})	+1·09 ± 0·03	+11·18 ± 0·03	+4·63 ± 0·03	−7·31 ± 0·03
K_1 (km s^{-1})	8·77 ± 0·05	2·90 ± 0·04	4·77 ± 0·04	4·61 ± 0·04
e	0·040 ± 0·005	0·403 ± 0·011	0·297 ± 0·007	0·234 ± 0·007
ω (degrees)	332 ± 8	146·1 ± 1·8	88·1 ± 1·7	306·3 ± 2·0
$a_1 \sin i$ (Gm)	121·4 ± 0·7	37·7 ± 0·5	61·2 ± 0·6	34·0 ± 0·3
$f(m)$ ($M_\odot$)	0·0704 ± 0·0011	0·00201 ± 0·00008	0·00960 ± 0·00026	0·00515 ± 0·00014
R.m.s. residual (wt. 1) (km s^{-1})	0·20	0·15	0·17	0·15

*T_0 = MJD 53165·9 ± 0·8

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THE STAR OF BETHLEHEM IS *NOT* THE NOVA DO AQUILAE (NOR ANY OTHER NOVA, SUPERNOVA, OR COMET)

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The Star of Bethlehem is only known from a few verses in the Gospel of Matthew, with the Star inspiring and leading the Magi (*i.e.*, Persian astrologers) to Jerusalem ultimately to worship the young Jesus Christ in Bethlehem. In the last four centuries, astronomers have put forth over a dozen greatly different naturalistic explanations, all involving astronomical events, often a bright nova, supernova, or comet. This paper will evaluate one prominent recent proposal, that the Star was a 'recurrent nova' now catalogued as DO Aquilae, and provide three refutations. In particular, (*a*) DO Aql is certainly not a recurrent nova, but rather an ordinary nova with a recurrence time-scale of over a million years, (*b*) in its 1925 eruption, DO Aql certainly never got brighter than $8^{m}\cdot 5$, and the physics of the system proves that it could never get to the required luminosity of a supernova, and (*c*) the Magi were astrologers who had no recognition or interpretation for novae (or supernovae or comets), so any such event is completely irrelevant and meaningless to them.

Introduction

In a 1999 book titled *The Star of Bethlehem, an Astronomer's View* by Mark Kidger[1], he started with a standard recounting of the story and reasonably concluded that the birth of Jesus (and hence the Star) was in the springtime of 5 or 6 BC. He then focussed on separate Chinese reports of two comet-like 'guest stars' from 5 BC and from 4 BC, finally concluding that those were both reports of a single *nova* eruption that appeared between Altair and the head of Capricorn in the spring of 5 BC. Then, based on the date of the alleged nova, he connected it to the Star of Bethlehem with the only logic being "Just by the coincidences in dates, the 5 B.C. object was — *must* have been — the Star." (his italics). On looking through the catalogue of variable stars, the closest nova is DO Aql (Nova Aquilae 1925), so he supposed that DO Aql is a recurrent nova with an eruption roughly two millennia earlier.

The first refutation; DO Aql is not a recurrent nova

Kidger's hypothesis requires that the Star erupted in 5 BC (to be reported by the Chinese as a concatenation of a "broom star" in 5 BC and a "fuzzy star" in 4 BC) as well as in 1925 (being catalogued as the ordinary nova DO Aql), and he calls the system a recurrent nova. Kidger offers no evidence to show that DO Aql is a recurrent nova, and he says only "it is definitely easier to limit the search to recent novas, just in case one of them happened to be a repeat outburst of the putative 5 B.C. nova explosion." However, there are strong reasons for knowing that DO Aql cannot be a recurrent nova, and that it has a near-maximal recurrence time scale.

In an exhaustive compilation of recurrent-nova properties[2] and nova properties[3], Ashley Pagnotta and I have recognized and quantified seven observable properties that can be used to distinguish recurrent novae (with eruptions separated by historical time-scales) from classical novae with long recurrence time-scales (of 10 000 to over a million years). Some nova systems might be recurrent with only one of the multiple eruptions in historic times being discovered, so our criteria for recurrence is useful to recognize such cases. For the particular case of DO Aql, we have observations that can test only four of our criteria. The first property is that only low-excitation lines were seen in spectra of DO Aql[4] (*e.g.*, the iron lines go no higher than Fe III), whereas *all* recurrent novae show very-high-excitation lines (*i.e.*, He II and Fe VII to Fe XIV). The second property is the amplitude and decline rate, with DO Aql having an amplitude of $9^{m}\cdot5$ and the time to fade by three magnitudes from peak (called t_3) being 900 days. With the very long fade time, DO Aql lies on the edge of the region for novae, far away from the recurrent novae. The third property is the presence of an evolved companion star, as indicated by an orbital period longer than 0·6 days, with 80% of the recurrent novae having evolved companions. DO Aql has an orbital period of 0·168 days[5], and thus it certainly does not have an evolved companion. The fourth property is the flat top in the peak of the light-curve, which is F-class for DO Aql, whereas all known recurrent novae have either S-class or P-class. Thus, all individual properties of DO Aql point consistently to properties that are the complete opposite of those for recurrent novae.

In my compilation and classification of nova light-curves[6], I had adopted DO Aql as the prototype of the 'F-class' novae, those with a long flat peak at nearly constant luminosity. The F-class novae are defined by their very long time at peak, with this requiring continuing nuclear burning on the surface of the white dwarf (which points to a very-low-mass white dwarf that must have a very long recurrence time-scale[7]). In addition, the long ejection of material throughout the peak makes for a high ejected mass such as requires a long recurrence time-scale to accumulate enough material. (I have measured a large ejected mass for the F-class nova BT Mon by means of the orbital-period change across its 1939 eruption[8].) Detailed models[9] of the long-duration peaks require low-mass white dwarfs, $\sim 0\cdot6\ M_{\odot}$, for which the recurrence time-scale must be at its maximum[7]. In addition, the F-class novae have low-energy emission lines, with this pointing to a low-mass white dwarf (that would have a very long recurrence time-scale). Thus, with good confidence, we know that DO Aql and all F-class novae have the maximal recurrence time-scale, over a million years, and so DO Aql is not a recurrent nova and did not go off roughly 2000 years ago.

The second refutation; DO Aql never came near to naked-eye visibility

I have already published the full light-curve of the 1925 eruption as based on magnitudes reported widely in the literature[6], with this being entirely in the V band. In late 2009, I used the *SMARTS* 1·3-m telescope on Cerro Tololo to measure late-time quiescent magnitudes of B = 18·55 and 18·63 for $(B - V)$ = 0·56 and 0·59, respectively. In late 2010, I travelled to the Harvard College Observatory and measured the B-band light-curve on their archival plates from 1899 to 1934. The first Palomar Sky Survey, in the 1950s, shows DO Aql at $B = 18^{m}\cdot 10$, while the POSS2 survey shows $B = 18^{m}\cdot 80$. The full light-curves in B and V for 1925 to 1930 are displayed in Fig. 1. The V light-curve shows a nova with a flat-topped light-curve with a peak that is at $8^{m}\cdot 5$ (and certainly not significantly brighter).

The second refutation is simply that DO Aql brightened only to a visual magnitude of 8·5 in its 1925 eruption, and any prior eruption would come only to the same peak brightness, so a DO Aql event in 5 BC could never have been detected by the Magi or by the Chinese. Kidger reasonably requires that any nova must be very bright, brighter than $V = 0$ or so, to be discovered and considered significant. So we have a huge gap from $V = 8\cdot 5$ to $V = 0$.

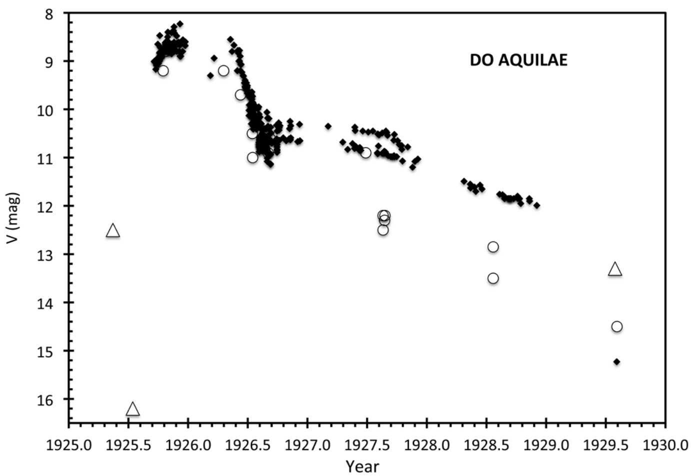

FIG. 1

This light-curve of DO Aql includes all published magnitudes plus my new measures from the Harvard plates. The small diamonds are for V-band measures, the circles are the B-band magnitudes, and the triangles are for upper limits in the B-band. The light-curve shows a flat-topped peak from nearly 1925·8 to 1926·4 at $V = 8^{m}\cdot 5$. The three magnitudes from 1926·19 to 1926·30 demonstrate that the nova did not have any significant brightening during the gap in observations in the middle of the peak, while the upper limits at 1925·37 and 1925·54 demonstrate that the peak is not just a late plateau for some earlier and much brighter peak. Thus, this light-curve proves that DO Aql never got significantly brighter than $V = 8\cdot 5$. The two streams of V magnitudes around 1927·6 are simply due to observers using slightly different sequences of comparison stars. (This is a ubiquitous problem before the 1970s when dealing with sequences fainter than tenth magnitude.) The $(B - V)$ colour starts out around zero, and increases to around one magnitude in the tail, with this being the normal development for novae.

Kidger was aware of this problem, but he addressed it with only one unsatisfying sentence, "If DO Aquilae really did have a huge eruption in 5 B.C., it is quite possible that this explosion removed the need for a further big eruption of the star for a very long time: successive ones might be smaller until, once more, the system reaches a new and massive crisis." That is, he gives no motivation, theory, or precedent to account for how an ancient eruption could possibly be more than 8·5 magnitudes brighter than in 1925.

Indeed, we have three strong modern astrophysical reasons for knowing that DO Aql could not possibly have had a significantly brighter eruption two millennia ago. First, all eruptions from a single recurrent nova always have the same light-curve and reach the same peak magnitude. We know this empirically from my comprehensive reconstruction of all known recurrent-nova eruptions[2]. And we know this theoretically because the light-curve is determined only by the white-dwarf mass, its magnetic field, the composition of the accreted material, and the orbital period (which determines the accretion rate), with none of these changing over the time-scale of millions of years. Second, even though I have found the first case where a classical nova event leads to a recurrent nova event[10], classical and recurrent novae both lead to approximately the same peak brightness. This is known both empirically[2,11,12] and theoretically[13]. The average peak absolute magnitude for both is $-8^{m}\cdot0$, with a 1-sigma scatter of $1^{m}\cdot3$ and a total range of $-6^{m}\cdot1$ to $-10^{m}\cdot7$. Third, we know that the 1925 eruption had ordinary spectral, photometric, and colour development for an F-class nova, so the peak absolute magnitude would have been approximately $-8\cdot0 \pm 1\cdot3$. Thus, the putative 5 BC eruption would have to have been at least 8·5 magnitudes brighter to be discovered by the Magi, for a peak absolute magnitude of $-16\cdot5 \pm 1\cdot3$ or more luminous. This explosive energy is that of a type II supernova, with the implication that any such explosion would have to destroy completely the system. We still see DO Aql, so it could not have had any eruption two thousand years ago. And to emphasize this more, we know that there is no way for a cataclysmic variable with a low-mass white dwarf to produce any explosion with supernova energies. From all three reasons, we know with high confidence that DO Aql could not have had an eruption in 5 BC that was significantly brighter than its 1925 eruption.

Third refutation; any nova is meaningless to the Magi

Historically, all the naturalistic explanations for the Star of Bethlehem invoke some astronomical event that would be impressive to modern astronomers. But this completely misses the point, because the only people to attach original meaning to the Star were the Magi, and they were *astrologer*s, not astronomers. Astrologers have no way to place a nova onto a horoscope. Astrologers have no interpretation for any nova. (We do know a lot about what the ancient astrologers actually practised and believed, for example, with roughly contemporary books by Ptolemy and Firmicus.) Novae are irrelevant and meaningless to astrologers. As such, any hypothesis of nova-as-Star is nonsensical, because the Magi could not have been motivated to travel to Judaea by a nova. This third refutation is as strong as any possible from ancient history.

This third refutation can be extended, with the same power, to any nova, supernova, or comet. That is, Persian astrologers had no way to place any nova/supernova/comet onto their horoscopes, nor would they have had any way of interpreting any nova/supernova/comet. All novae/supernovae/comets were irrelevant and meaningless to the Magi, and thus they cannot be the Star of Bethlehem.

This third refutation is taken from the 1999 book of Michael Molnar[14], titled *The Star of Bethlehem: the Legacy of the Magi*. Molnar has argued that the *astrological* orientation of the Magi has ruled out prior naturalistic explanations, because they are all *astronomical* events that excite *modern astronomers*, but not *ancient astrologers*. The bottom line is that no nova, supernova, or comet could ever have motivated the Magi to travel to Judaea.

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REVIEWS

A Single Sky: How an International Community Forged the Science of Radio Astronomy, by D. P. D. Munns (MIT Press, London), 2012. Pp. 247, 23·5 × 18·5 cm. Price £23·95/$34 (hardbound; ISBN 978 0 262 01833 3).

Munns is an historian with a particular interest in the sociology of science. Radio astronomy in the first decades after World War II provides a good subject, which he has explored by mining the archives of the main centres in USA, Australia, and England. An interesting selection of quotations is used to support his main theses, which concern relationships between "community-style" and "complex-style" science, by which he means science developing within a traditional university environment contrasted with centralized national institutions. The most interesting example is the development of the National Radio Astronomy Observatory in USA.

Unfortunately, Munns apparently did not supplement his material by interviewing astronomers who were involved in those formative days. In fact he disapproves of the interview technique, which was so well employed by Sullivan in his excellent history, *Cosmic Noise*; he regards that book and the sociological study *Astronomy Transformed* by Edge & Mulkay as "perpetuating myths" (p. 81). As he evidently does not understand the science and technology of radio astronomy, the result of such a stance is an unhappy succession of errors which could easily have been avoided by asking someone more knowledgeable to read his text.

Munns's social theory is more difficult to comment on, since the language is unfamiliar. For example he refers to "the radio astronomers' desire to reshape the social values of science" (p. 16), and "the radio astronomers inverted the standard technocratic superpower metanarrative of the Cold War state" (p. 174). Well, I can only say that I was there and I do not know what he is talking about.

If you want to know the early history of radio astronomy read Sullivan. If you are interested in the broader context of astronomical history in that era read Longair's *Cosmic Century*. If you are interested in sociology, read Munns, but brace yourself for some obscure jargon, and watch out for errors. — FRANCIS GRAHAM-SMITH.

The Book of Universes, by John D. Barrow (Vintage, London), 2012. Pp. 354, 19·5 × 13 cm. Price £9·99 (paperback; ISBN 978 0 0995 3986 5).

Having read most of Barrow's popular-science books, I approached this book with high expectations. The book is well written, discusses both historical and cutting-edge topics, has some pleasant surprises, and just the right amount of personal touch on the part of the author.

The first two chapters discuss the history of cosmology before Einstein's first paper on the subject. This cannot be a detailed history but is important to set the stage for the rest of the book. Considering that there are less than four dozen pages in which to cover a few thousand years, I was surprised that several are spent on spherical astronomy, but the reason for this soon became clear. The next chapter is an excellent summary of the first 20 years of relativistic cosmology, discussing the work of Einstein, de Sitter, Friedmann, Lemaître, Eddington, Tolman, Milne, and McCrea. Before moving on to discuss the Steady State universe, two chapters explain more exotic universes in greater detail than in books covering roughly the same territory. A chapter on Bianchi models, *etc.*, then continues this theme. Most histories of cosmology at this level tend to leave out 'dead ends' which are not part of the current standard cosmological model, but the discussion is important both for the history itself and for introducing ideas which appear again later in somewhat different form. After a chapter on singularities and the hot Big Bang, three chapters give a good overview of more modern developments which started with the idea of the inflationary universe.

As has often been the case with Barrow's popular-science books, I learned new things even about topics with which I am quite familiar. For example, I had never come across Erik Holmberg's remarkable optical analogue 'computer' for simulating galaxy evolution. It was also new to me that there is a maximum force in General Relativity. But even familiar material is sometimes presented in a new light which makes the book more a coherent narrative than just a collection of facts.

The final chapter discusses the current standard cosmological model, now constrained quite well by observations. In some sense this follows directly from the third chapter, since nothing more than 1930s cosmology is needed to interpret current observations. On the other hand, the question arises as to why we observe the values of the cosmological parameters that we do, and not others; at least to some extent this might be explicable by the inflationary-universe paradigm and other recent developments. (Similarly, the anthropic principle, which Barrow touches on throughout the book — sometimes directing the reader to his book with Tipler for more details — might be important at some level. Often, discussions of this start with Brandon Carter's 1973 talk, but the overview of spherical astronomy at the beginning of the book also illustrates the fact that what we observe depends on where and when we observe it.) Such questions are sometimes deemed to be more philosophical than scientific, with no concrete predictions which can be tested. However, at the end of the book Barrow mentions recent work (not involving the anthropic principle) by himself and Douglas Shaw which predicts a precise value for the sum of the density parameter and the cosmological constant: $\Omega + \lambda = 1{\cdot}0056$. (Since the sum is greater than 1 this implies positive spatial curvature and a spatially closed universe, though one which will expand forever.) When the book was written, observations allowed values between 0·9916 and 1·0133. Today, the allowed range is between 0·9988 and 1·0065. It will be interesting to see if results from the *Planck* satellite rule out this precise prediction.

There are about the right number of photographs and diagrams and 44 pages of notes for 295 pages of text. I enjoy reading such notes but it would be nice if those which are not just literature references were provided as footnotes so that one does not have to flip back and forth. I appreciate the inclusion of an index, which also distinguishes between the main text, figures, and notes. There are a few typographical errors and other mistakes in the text but nothing serious; the casual reader won't notice them and they should be obvious to the expert.

Nevertheless, perhaps future editions can benefit from better proofreading. All in all, my expectations were met. This is a wonderful book. — PHILLIP HELBIG.

The God Problem: How a Godless Cosmos Creates, by Howard Bloom (Prometheus Books, Amherst), 2012. Pp. 708, 23·5 × 16 cm. Price $28 (about £18) (hardbound; ISBN 978 1 61614 551 4).

Howard Bloom's book, *The God Problem*, is exciting and takes the reader on an intellectual quest at breathtaking speed. The problem being considered is not that of how to continue to believe in a divine being in a scientific and secular age, but rather the converse; in this secular age how do we answer the most puzzling question of all, "How does a godless cosmos create itself?" In his quest for an answer Bloom writes about the thoughts of the deepest thinkers of all times, from the Babylonians to our own days, and then proposes new ideas of his own.

The book brings with it much promise: various reviewers on the dust jacket tell us it is a "work of genius", a great game-changing book on a par with Darwin's *The Origin of the Species*, Lyell's *Principles of Geology*, or Newton's *Principia Mathematica*, but personally I felt it fell short of the hype.

However, Bloom does begin with a brilliant thought experiment to illustrate the enormity of the problem in hand. We are to imagine that we are sitting at a café table at the beginning of the Universe in utter nothingness and observe the Universe bursting forth from a pinprick into a hailstorm of particles, galaxies, stars, molecules, cells, and DNA. He asks, "How in the world did the cosmos come to be?" In various ways and at various levels Bloom repeats the problem throughout the book, thus not only honestly emphasizing the scale of the mountain to be climbed but also of course he implicitly emphasizes the insightfulness of his explanation in conquering it.

In order to explain the creativity of Nature without recourse to a deity Bloom introduces what he calls five heresies that first have to be abandoned. Some of these 'heresies' are not so surprising — they are logical or mathematical aphorisms that simply do not hold outside of a particular context; however, the dismissal of others is much more problematic and contentious. These heresies are: 'A does not equal A' (things develop and change), 'One plus one does not equal two' (often a combination gives rise to new entities), 'The second law of thermodynamics is untrue' (the Universe has steadily become more ordered, not disordered), and 'Randomness is wrong' (if the Universe were governed by chance it would not be so structured). The final heresy to be overturned is 'Information theory is not about information, it is about *meaning*' ('meaning' is the response to a communicated stimulus that that leads to a particular outcome).

Bloom describes a brief history of the 'God Problem' in cosmology from the scientific revolution onwards, asking whether its authors, Kepler, Galileo, and Newton, were in fact creationists? They were, and yet today the problem without recourse to a creator is even greater because they were missing a bigger story about the origin of things, only known about in the last century, the Big Bang. Bloom then sets out on the major part of the book, a vivid account of the development of ideas, and hunts out the underlying axioms that we take for granted.

We go back 6 000 years to the Sumerians and Babylonians and before them to the builders of Catalhöyük and earlier still to the construction of the first city, Jericho. Those builders used only the simplest of tools and even simpler conceptual ideas. But they did iterate, that is, they repeated simple shapes, mud bricks, again and again, and so constructed impressive monuments. They used the concepts of straight lines, rectangles, and planes, and were able to build circular structures while lacking the concept of a circle. Or so Bloom confidently informs us, against the received wisdom of others such as Bertrand Russell.

From such simple beginnings we follow the ascent of mental concepts up to Einstein, modern cosmology, and beyond, into a rewriting of information theory, the rise of meaning, the natural emergence of higher levels of order and intelligence from the iteration of simple entities. Such iterations form a set of recruitment strategies that seek out higher principles from the simple. We learn that through the computer iteration of a simple rule the deep implicate order of the Mandlebrot set is revealed, and through the iterations of Conway's 'Game of Life' the patterns, the cellular automata, on a computer screen take off with a life of their own.

And so onto Stephen Wolfram, who developed cellular automata into producing 'A New Kind of Science'. Such an approach explains the complexity that arises in natural systems by not explaining them! That is, by not explaining them using the old methodology that develops basic principles with a mathematical theory using equations, for we are told such an approach is limited by the boundaries of what is possible with mathematics, just as the Babylonians were limited in their time by the lack of the concept of a circle. Instead Wolfram began with simple rules that brought whole computational universes, his cellular automata, to life by iteration after iteration. He spent 18 years running cellular automata for a cumulative total of roughly 10^{18} generations exploring the consequence of 10^{9} different rules. Most experiments produced nothing at all, but a few produced order with astonishing complexity.

And so apparently we can solve the God Problem. Inanimate matter, energy, space, and time have the property of an implicate complexity that is able to create an intricate universe by iterating simple rules. The answer to the mystery of a self-creating universe is to be found in the property of emergence. The emergence of unimaginable complexity from simple rules is to be understood by the unpacking of the mysteries that hide in the metaphor of the embryo.

Just as our bodies are not fixed objects but a changing and replenishing pattern of cells and biochemicals so the natural Universe is a machine of emergence that produces novelty, and we are the latest but not the last of the Universe's big leaps.

Bloom explains with great authority, and his self-confidence persuades the reader that the mystery has been solved. And yet one is left with an uncomfortable feeling that one mystery has only been explained by another, and at a cost. Do we really have to reject the Second Law? Do we have to reject the mathematical basis of theoretical physics? Can we really explain the Universe by the complexity generated by the computerized iteration of simple rules while contemplating an embryo as Bloom suggests? On whose computer do these iterations run, God's?

Alternatively it might be just as well to say that the Universe with all its implicate order sprang out of nothing because as 'nothing' was really 'nothing' then there was 'no-thing' to prevent it from doing so! The problem with such an explanation is not that it explains nothing but that it explains everything and anything and therefore nothing; the same may be said of Bloom's scenario.

Bloom's immense self-confidence in being able to resolve the God Problem would be more persuasive if his work did not contain several schoolboy howlers. On page 66 Bloom explains the original Steady State theory, and not only omits the essential point of the Perfect Cosmological Principle but confuses the original theory with the later cyclical Quasi-Steady-State model, and goes on to make the matter worse by thinking Hoyle used the Tired Light theory to explain cosmological red shift, thus confusing the Steady State (exponentially expanding) model with the Static model. Such ignorance about cosmology is inexcusable in a book about the origin of the Universe. Furthermore we read on page 323, "[Joseph Priestley] One with the most basic understanding of chemistry would know that mercury is an element that could not produce anything apart from mercury vapour on the application of only heat, and that at room temperatures it is a liquid, not a solid." (Priestley of course had used the Sun's rays to heat mercuric oxide and that was a solid.) On page 389 Bloom confuses the General Theory of Relativity (1915) with the earlier Special Theory (1905). Furthermore he is sloppy with his terms: on page 395 he writes that, "gravity is the equivalent of speed. That's the 'equivalence principle'" and then corrects himself by continuing, "Gravity and acceleration are equivalent", yet subsequently confuses acceleration with speed again. On page 401 he confuses dimensions by quoting the value of c^2 in units of miles per second.

I also felt that it was distracting and unnecessary for Bloom to include his own personal cosmological theory, that of the 'big bagel'. Whereas the research community does explore the possibility of a toroidal cosmological topology as one of many such possibilities, it hardly merits the prominence Bloom gives it to explain dark energy and the beginning and end of things.

These mistakes are not mere typos and should have been spotted at the proof-reading stage. Their continuing presence leaves the reader wondering about what else the author might have wrong and therefore casts a shadow over the book's conclusions leaving the solution of the God Problem an open question. — GARTH BARBER.

Organizations, People and Strategies in Astronomy, Volume 2, edited by André Heck (Venngeist, Duttlenheim, France), 2013. Pp. 474, 24 × 16 cm. Price €87 (about £75) (paperback; ISBN 978 2 9542677 1 5).

This is the second volume in a series edited by André Heck appearing under the imprint of a new publisher [see **133**, 188, 2013 for a review of the first of the *OPSA* series]. A closely-related series, *Organizations and Strategies in Astronomy*, which began in 2000, was published first by Kluwer and latterly by Springer.

Who should read this book? Anyone who plans to apply for observing time at any of the places whose time-assignment processes are described (ESO, *Gemini*, the *Large Binocular Telescope*, *SALT*, and *Subaru*). Anyone who cares deeply about the particular countries whose astronomical efforts, history, facilities, and all are described here (Argentina, Brazil, Italy, Mexico, the Netherlands, Poland, Turkey, and Vatican City). Poland, has the most history, beginning with Copernicus (Mikolai Kopernik) and Hevelius (Jan Heweliusz); and the Netherlands has the largest number of current institutions, some with names like SRON that probably only a Dutchman can pronounce. You perhaps already knew that "varsovienne" is a folk dance (otherwise known as "put your little foot" and much loved by the late Conrad Hilton), but did you know that Cracovians are the matrix calculations carried out by Tadeusz Banachiewicz (1882–1954), director at Cracow Observatory before and after World War II? He was also a vice-president of the IAU from 1932 to 1938.

But the folks most likely to benefit from the volume are astronomers who contemplate establishing some new project with international collaboration. The observatory and national reports are obviously relevant, but even more so the beginning chapter on where new projects in astronomy come from and how they get realized. It addresses money and management in general as well as many specific (mostly European) facilities, past, present, and planned. But for sheer nostalgia, read the chapter on *IUE*, the *International Ultraviolet Explorer*, the only space facility for which astronomers went, well, not quite to the telescope, but at least to central ground stations to watch their photons pile up in channels in real time. Other topics include astronomy-related portraits and postage stamps, ethics and education, and the functions of professional societies and history of science. — VIRGINIA TRIMBLE.

Astronomy Photographer of the Year, collated at the Royal Observatory, Greenwich (Collins, Glasgow), 2012. Pp. 224, 27 × 27 cm. Price £25 (hardbound; ISBN 978 0 00 748280 1).

This book presents the results for the years 2009–2012 of the annual competition, run under the aegis of Royal Museums Greenwich and with the involvement of the *Sky at Night* magazine, with four image categories and three special prizes. The winners and runners-up are included for the years 2009–2011, but all short-listed images for 2012 are shown. There are a few images that might be described as 'meteorological' rather than 'astronomical', including one (on page 66) where an ice-crystal halo is described as being caused by reflected light from the Sun. (It is caused by refraction, of course.)

Each image is accompanied by a brief note from the photographer — either describing how they started taking astronomical photographs, or sketchy details about the image itself. There is a background note from the editors; technical details; and, occasionally, a comment from one of the judges.

Overall, the book has a tendency to fall between two stools, in that it is trying to cater for too diverse a range of readership. Much of the material is aimed at the astronomical novice (including the section on how to start taking astronomical photographs), whereas, obviously, many of the striking images and their technical details would be of particular interest to fairly experienced astronomers. The range of images is extreme, ranging from pictures obtained with simple cameras to images that consist of stacks of hundreds of individual frames. The background notes from the editors tend to be somewhat repetitive and simplistic, even for non-astronomers.

The images are obviously crucial in a book of this sort, and you would have thought that the designer would have given careful thought to the way in which they were displayed. No: he (or she) has shown the usual cavalier attitude that is particularly evident when designers deal with astronomical images. The impact of many has been completely ruined by their being placed across the gutter. A good designer will sometimes allow this if the image occurs in the centre of a signature, where the pages may be opened flat, but in most cases here varying amounts are lost. This even applies to some of the winning images. The worst instances are possibly Damian Peach's images of Mars on pages 134–135, and the mosaic of the Moon on pages 202–203, where the arc of the limb has an extraneous 'step' of about 14 mm from one page to the next. The orientation of images is also somewhat arbitrary and unexplained. The image of the Moon on page 34, for example, is inverted relative to the other lunar images. That on page 100 is left–right reversed, but general readers will not be aware that this was probably because of the use of a diagonal.

Regrettably, there are so many discrepancies between the images and the technical details that one is forced to wonder whether the errors arise from carelessness on the part of the compilers, or whether some photographers deliberately submitted incorrect information in the hope of misleading the judges. The most frequent error describes images with long star trails as having been obtained with exposures of just a few seconds. Some images simply cannot have been secured in the way described. Perhaps the most glaring error concerns the Highly Commended image by the Young Astronomy Photographer of the Year on page 39. This highly detailed image of the Moon simply cannot have been captured with just a DSLR camera and a 28–55 mm lens, without a telescope. (It may be compared with the image on page 47, obtained through a telescope by Anthony Ayiomamitis, an experienced photographer, which shows merely a suggestion of features on the lunar surface.)

If there will be future editions of this book with images from later years, I sincerely hope that more care will go into the preparation, descriptions, and technical details. In the meantime, enjoy the many stunning images, even if you need to contact the photographers to find out exactly how they were obtained. — STORM DUNLOP.

Near-Earth Objects: Finding Them Before They Find Us, by Donald K. Yeomans (Princeton University Press, Woodstock), 2012. Pp. 172, 24·5 × 16·5 cm. Price £16·95/$24·95 (hardbound; ISBN 978 0 691 14929 5).

Modern astronomers love acronyms and two of the more recent additions to the list are NEO and PHO. The first term, near-Earth object, applies to those asteroids and comets that pass within 1·3 AU of the Sun, and the latter, the potentially hazardous object, is a much more dangerous subset that gets to within 0·05 AU of the Earth's orbit. The word 'danger' is apposite because any PHO larger than 1 km has the potential to destroy civilization as we know it, if it impacts the Earth's surface. And this danger is being taken seriously. Large groups of astronomers with dedicated telescopes scour the sky hunting for these objects, and when found, their orbits are carefully established and their potential trajectories investigated for the possibility of a future close encounter with our planet.

At the hub of this endeavour is a team of orbital dynamicists at NASA's Jet Propulsion Laboratory in Pasadena, California, led by Don Yeomans. And now is the time to say "thank you". The book-reading astronomer owes a huge debt to the professional who turns aside from their daily graft and puts pen to paper.

Yeomans has done this twice. First with a superb history of comets[1], and now with the book under review. Yeomans has the gift of taking what is essentially an extremely complex subject, sifting out the important aspects and explaining them succinctly to a non-specialist readership. The book is authoritative, pacey, gripping, and eminently readable. It is also well illustrated and referenced, and as such is an ideal introduction to further study.

What I liked especially was the way in which it tempered the known with the huge amount of information that is still to be gathered in. Take a simple question like "how many of the 1-km impactors are comets and how many asteroids?" Well, the jury is still out. It is extremely difficult to estimate the size of a cometary nucleus by just looking at the way a comet's luminosity varies as it passes round the Sun. And it is no use just pointing out that an asteroid might hit our planet if you then do not spell out what one might do to stop this happening. Here Yeomans again provides an excellent introduction to how we might nudge asteroids to one side, assuming that we know exactly where they might hit Earth, and in which direction to nudge them. Then there is the additional problem: it is difficult to push something aside if you do not know whether you are applying force to a pile of rubble or a solid body; and with most asteroids it is very difficult to tell the difference. Likewise with comets: no comet has yet had its mass measured. Our ignorance is such that we do not know if we are dealing with dirty snowballs or dirty ice-balls. The strengths and densities of NEOs and PHOs are still an unknown.

Three things did surprise me somewhat with this book. Yeomans mentions a few times the possibility that comets and asteroids provided the building blocks for primitive life to the early Earth, bringing with them carbon-based minerals and organics. I was left wondering what special material source and physical conditions comets and asteroids had for these components that were not also available to the accreting and core-forming Earth. I was also somewhat surprised that Yeomans always looks skyward at the incoming flux of impactors, and did not spend a small time looking down at the old areas of the Earth's surface to use our past impact history to assess the future hazard. And thirdly, a little aside to the publisher, Princeton: the book is littered with footnotes which break up the flow, are usually just as interesting as the general text, and are printed in rather too small a font. I would much prefer it if these were simply incorporated in the text.

But all in all I loved this book. There are asteroids and comets out there with our name on. They are coming to get us. It is not a matter of 'if', it is a matter of 'when'. Read this book and join in the endeavours of those who are trying to do something about it. — DAVID W. HUGHES.

Reference

(1) D. K. Yeomans, *Comets: A Chronological History of Observation, Science, Myth and Folklore* (Wiley Science Editions, New York), 1991.

Exploring the Solar System, by P. Bond (Wiley-Blackwell, Chichester), 2012. Pp. 462, 27·5 × 22 cm. Price £37·50 (paperback; ISBN 978 1 4051 3499 6).

This is an excellent account of our present knowledge of the Solar System. The text is clearly written, and the full-colour illustrations are magnificent; each one has a substantial caption. The blurb on the back cover includes "this book is the ideal introductory text for non-science students and other readers with little or no science background", which, in my view, is an accurate description, and distinguishes it from my *Discovering the Solar System* (2nd Edn., Wiley, 2007), which is "essential reading for all undergraduate students for whom astronomy or planetary science are components of their degrees as well as those at a more advanced level approaching the subject for the first time. It is also suitable for anyone with a keen interest in astronomy." In accord with the target readership of *Exploring the Solar System* there is almost no algebra and a negligible amount of arithmetic. There are, however, well-presented graphs and tables. The author does not shy away from advanced concepts, such as Einstein's theory of General Relativity, illustrated very well, for example, in Figure 5.3 (plus caption) in relation to the precession of the perihelion of the orbit of Mercury.

The topics, and their order, are more or less conventional for books on the Solar System, starting with an overview of the Solar System, followed by a chapter on the Sun, but then chapters on the Earth, and the Moon, before returning to chapters on Mercury, Venus, and so on, ending with chapters on Pluto and the Kuiper Belt, one on Comets, Asteroids, and Meteorites, with a final chapter on Exoplanets. There are appendices of tables with data on the planets and their satellites, comets, trans-Neptunian objects, and space missions to the planets and smaller bodies. There is also a glossary and an extensive list of 'Further Reading'. There are end-of-chapter questions, but alas, no answers that I could find.

There is one topic I feel merited more discussion, and that is the basis upon which Pluto was reclassified as a dwarf planet (along with the largest asteroid Ceres, and three other large Edgeworth–Kuiper Belt objects, Haumea, Makemake, and Eris). A mini-study of the role of classification in science would have been apposite (see my book, *Pluto: Sentinel of the Outer Solar System* (CUP 2010), presumably published too recently to be included in 'Further Reading').

But this is an excellent book, and I recommend it to all who know little or nothing about the Solar System. — BARRIE W. JONES.

First Magnitude: A Book of the Bright Sky, by J. B. Kaler (World Scientific, Singapore), 2013. Pp. 239, 23·5 × 15·5 cm. Price £22/$34 (hardbound; ISBN 978 981 4417 42 6).

This latest Kaler book is a little gem. Its purpose is "to show readers with brightly-lit skies ... that they can have an appreciation of the heavens no matter where they live." Kaler takes those readers in turn through the wonders of the Sun, the Moon, the five bright planets, and the 23 brightest stars, cleverly incorporating both facts and anecdotes into his narratives so that (as the end-cover blurb concludes) "The concept ... serves as an introduction to general astronomy". In fact it goes further and provides an equation-free introduction to astrophysics, but it never loses sight of its prime objective of kindling an awareness of what we can see in even a light-polluted night sky. It persuades people to 'come look'.

Writing and speaking are quite different vehicles for conveying a message. Even so, Kaler achieves the impossible by writing in a decidedly conversational style, and that is what helps make this book a compelling read. His personalized, hand-holding descriptions and explanations keep the science deceptively simple without ever talking down, while the quiet passion that accompanies them makes the book a masterpiece of public outreach. It is remarkable how so much good-quality science can be packed into an innocuous-sounding description without using any mathematics, be it stellar evolution, supernovæ, planetary formation, Solar System debris, or biographies of the brightest stars. Perhaps one clue is in the anguished exclamation, "Oh what we have lost" — a lament over the damage caused by light pollution, an involuntary outburst that reveals just how close to the surface that passion actually is.

The book is attractively produced, though very slightly marred by a few slips and typos that managed to evade the proof-reader. I was mystified by the order in which the individual bright stars are treated; although the ordering is explained as generally linked to "order of rising" for the benefit of the observer, from the reader's angle I found it a bit difficult to locate the right page when I wanted to refer back — and that section occupies one-quarter of the whole book. However, that was only a trivial point. The numerous photos that are included in the book are by intention instructive. Most are in fact fairly stunning too, though are not overtly presented as such. That is a second clue: there is no hard sell here, just an invitation to share. It's very effective.

First Magnitude is a fine ambassador both for astronomy and for quelling light-pollution. Kaler's love of the stars and the sky is irresistible, and his sadness over the visual loss caused by light pollution is heartfelt and sincere. The non-specialists for whom the book is intended are far more numerous and probably more appropriate as a light-pollution lobby group than are astronomers, who are likely to appear biassed in the matter. Catch the attention of the general public, and the rest of *our* battle will be much easier. This book is an excellent tool for that purpose; it is one to share, to recommend, and to circulate. — ELIZABETH GRIFFIN.

The Second ATST-EAST Meeting: Magnetic Fields from the Photosphere to the Corona (ASP Conference Series, Vol. 463), edited by T. R. Rimmele *et al.* (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 460, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 808 4).

The next decade is likely to see an impressive improvement in our knowledge of the Sun's atmosphere — from the photosphere to the corona — through the introduction of two giant ground-based solar telescopes: the US *Advanced Technology Solar Telescope* (*ATST*) and the *European Solar Telescope* (*EST*), both of 4-m aperture and with the capability of resolving solar features with sizes of only 20–40 km. These are the proceedings of a conference held in Washington, DC, in 2011 November, which brought together about 100 participants to discuss the technical problems involved in these and other proposed large solar telescopes as well as the new research topics that the new-generation telescopes will address. The magnetic field in the atmosphere above the photosphere is an obvious focus since energy involved in heating the corona is likely to be channelled along field lines. But the long-established Zeeman effect used to find the photospheric magnetic field is fraught with problems when applied to the chromosphere and corona.

Much of the first part of these proceedings is taken up with recent results, with current instrumentation and ways of tackling the problems encountered as a guide to expected problems with the larger telescopes when they go on-line. Among the numerous contributions, there are impressive observations with spectropolarimeters of the tiny, moving, magnetic features streaming out from sunspots and the Evershed flow along penumbral filaments. Eventually, there should be a much improved understanding of their nature with the *ATST* and *EST*. Optical tools and techniques that will be used are extensively discussed in the second part. But the heart of the matter is taken up in the third part, with very informative descriptions of the construction of the *ATST* (Rimmele *et al.*, p. 377) and the *EST* (Collados Vera and the *EST* Team, p. 413). Other projects such as India's *National Large Solar Telescope* are discussed here also.

The nearly 500 pages of these proceedings give a very full account of the developments taking place in ground-based solar telescopes which will appeal not only to those working in this field but also those proposing spacecraft instruments. There are slight deficiencies — it was a pity none of the twelve editors considered going through some of the papers to improve the often ragged English — but this does not significantly detract from what is a rounded view of the present state of the subject. — KEN PHILLIPS.

Progress in Solar/Stellar Physics with Helio- and Asteroseismology (ASP Conference Series, Vol. 462), edited by H. Shibahashi, M. Takata & A. E. Lynas-Gray (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 546, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 806 0).

This book compiles the proceedings of the 61st Fujihara Seminar, held at Hakone (Japan) in 2011 March, and which brought together some of the most influential researchers in the fields of helioseismology and astroseismology. The main emphasis of these proceedings has been placed on the investigation of physical processes in stars and the Sun by means of our contemporaneous knowledge of helio- and astroseismology, and on the obvious implications for astrophysics in general. In this regard, there is an extensive series of contributions on the mechanisms responsible for the excitation of oscillations and the computation of theoretical instability domains, on the sensitive topic of solar-abundances determination and the investigation of the chemical stratification in stellar interiors, and on the latest developments from magnetohydrodynamics. Of the utmost interest is the discussion of the solar dynamo and solar activity from an helioseismic perspective, as well as the presentation of several instances of stellar activity across the Hertzsprung–Russell diagram. Finally, new observational findings obtained by current space-borne missions and ground-based campaigns are reported, and a series of observational challenges are identified. — TIAGO CAMPANTE.

Four Decades of Research on Massive Stars: A Meeting in Honor of Anthony F. J. Moffat (ASP Conference Series, Vol. 465), edited by L. Drissen, C. Robert, N. St.-Louis & A. F. J. Moffat (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 532, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 812 1).

The meeting recorded here is the fifth in the excellent series organized by the University of Montreal over the last two decades, the word 'recorded' reflecting the full reporting of the discussions that distinguishes the proceedings of these meetings. I started my reading of the present volume with the general discussions after each session and was not disappointed: there was real discussion, occasionally a little ratty, but genuine conversations and certainly illuminating.

The programme was divided into nine sessions ranging from stellar properties, winds, clumping, and mass loss to stars in binary systems and clusters, to surveys for particular types of massive star, especially WR stars — all topics in which Tony Moffat has been conspicuously active. Each session began with a review, was followed by several well-chosen contributed papers, often plus discussion, and was concluded with general discussion. The reviews and contributed papers, which are given enough space in the proceedings to say something, fit together well, *e.g.*, more on X-rays, η Carinae, and WR 140 following Gies's fine review in the binaries session. Other reviews I was particularly taken with were those on wind clumping by Sundqvist *et al.*, young star clusters by Davies, and the search for WR stars in the local group by Shara *et al.*, but the volume is much more than a collection of reviews by pundits.

My quondam editorial eye spotted a few errors. A horror was the confusing use of the symbol $\simeq$ for both similarity (*e.g.*, $L_X \simeq 10^{-7}\, L_{bol}$) and proportionality ($L_X \simeq L_{bol}$ instead of $L_X \propto L_{bol}$, *etc.*) in the contribution by Owocki *et al.* Another grumble concerns the figures: often they were so reduced that the scales are hard to read, illegible even with a magnifying glass in one case. Many of the figures were prepared in colour, and appear in colour in the on-line version, with the colours identified in the captions; but this information is often lost on the black-and-white page so it is hard to figure out what is what. Some authors have anticipated this by adopting different line styles and weights so that their figures 'work' in both colour and black and white, or by identifying the lines and symbols in their figures so that it is possible to distinguish the colours by their shades of grey on the printed page, but in other cases the reader is lost.

I was struck by the variety and impact, in some cases only beginning, of the new large, homogeneous datasets becoming available from large surveys and collaborative projects such as MiMeS, VVV, GOSS, NoMaDS, *VLT-FLAMES* Tarantula, RIOTS4, *etc.* Maíz Apellániz *et al.* include a glossary for the O-star spectroscopic surveys in their contribution, and perhaps the proceedings of a future meeting will include a dedicated index if the list keeps growing. The study of massive stars is going from strength to strength and this volume provides a very readable and useful account, both for those already in the field and those coming into it. I will continue to consult it for a long time and recommend it strongly. — PEREDUR WILLIAMS.

Binary Paths to Type Ia Supernovae Explosions (IAU Symposium No. 281), edited by R. Di Stefano, M. Orio & M. Moe (Cambridge University Press), 2013. Pp. 353, 235 × 18 cm. Price £76/$125 (hardbound; ISBN 978 1 107 01981 7).

There is general agreement that the majority of normal type-Ia supernova(e?) explosions are the result of accretion onto Chandrasekhar-mass CO white dwarfs. A major issue has been (and to some extent still is) whether this accretion is relatively slow, most probably from a non-degenerate companion (the single-degenerate channel) or fast, through the merger of two white dwarfs (the double-degenerate channel). The single-degenerate channel is attractive in that it naturally accounts for the relative homogeneity, or standardizability, of most SNe Ia; moreover, there are many long-studied classes of objects containing accreting white dwarfs, including classical and recurrent novae, and symbiotic systems. However, deep searches have yet to identify convincingly any non-degenerate survivors of historical SN Ia eruptions (direct 'before-and-after' imaging of recent SNe is handicapped by the rarity of nearby events, and the relatively low expected progenitor luminosities); this evidence of absence suggests that perhaps $\sim 80\%$ of thermonuclear SNe arise through the double-degenerate channel.

This view was becoming established around the time of this conference, attended by 140 or so astrophysicists. I use this label advisedly; the focus of the presentations is very firmly on stellar physics, not cosmological applications.

Furthermore, the contributions are quite strongly slanted towards observational endeavours, although a few theory papers are of course included, addressing, for example, white-dwarf evolution; issues of nuclear combustion, conflagration, detonation, and explosion; and implications for galactic chemical evolution. Some of the contributions seem to be rather tenuously connected to the nominal theme of the meeting, but overall, and not surprisingly, the impression is of a vigorous research field being pushed forward on a number of fronts, and a lively symposium.

What about the book? Bizarrely, discussions are recorded for only two papers, which appear to have been selected randomly from the 80 or so included in the proceedings without explanation or comment. The conference photographs are not captioned (and are not very clear). There's an author index, but neither object nor subject index. With this barely perceptible level of editorial effort (even the title is grammatically unconventional), it's hard to understand why publication has taken a relatively long time, with the inevitable consequence that many of the 'hot' results are already well established in the refereed literature. (It seems an age since the announcement of the award of the Nobel Prize in Physics to Perlmutter, Schmidt, and Riess, but the meeting that this volume documents was held in Padova some three months before that event.) As is standard with CUP publications of IAU Symposia, there is an on-line colour edition which is not mentioned anywhere in the bleakly monochrome printed version (wherein many figure captions refer to non-existent colour coding), and to which no access rights are conferred by purchase of the book. If, in the knowledge of all this, you still want to go ahead with buying this volume, fine, but if you have access to an institutional subscription I'd suggest you'd be better served by going on-line to Volume 7 of the CUP 'journal' *Proceedings of the International Astronomical Union*; or by conducting a bibliographic-code query on 2013IAUS..281 from the ADS 'Journal/Volume/Page' query form. — IAN D. HOWARTH.

Electromagnetic Radiation from Pulsars and Magnetars (ASP Conference Series, Vol. 466), edited by W. Lewandowski, O. Maron & J. Kijak (Astronomical Society of the Pacific, San Francisco), 2012. Pp. 282, 23·5 × 15·5 cm. Price $77 (about £50) (hardbound; ISBN 978 1 58381 814 5).

The core impression left by the proceedings of this conference held in Zielona Gora, Poland, in 2012 April, is that there are very many different kinds and manifestations of neutron stars, both single and binary, a significant subset of which fulfil the criteria for pulsars (rotation-powered, radio emission beamed by oblique magnetic dipole) or magnetars (soft-gamma repeaters or anomalous X-ray pulsars powered by drawing on magnetic fields of 10^{14} Gauss or more). There are at least the beginnings of a unified model, with the most important initial conditions being the dipole-field strength, rotation period, and presence or absence of a companion. Orientation of the source relative to us probably also counts, though not so much so as in unified models of active galactic nuclei.

The conference attracted 120 listed participants from 20 countries. The largest number (38) were from Poland, with Germany (12) second, and the US (10) third. The index lists 149 authors (not, apparently, all present), and the table of contents 74 contributions, from 8–10-page invited reviews down to 2-page short presentations. These are grouped by subject matter — multi-wavelength and radio observations, theory of emission mechanisms, theory of structure and evolution, magnetic fields, and pulsar-wind nebulae. A couple of talks looked forward to results expected from the *Cherenkov Telescope Array* (a follow-on from *VERITAS* and *MAGIC*) and a polarization-mapping code called POREC. One speaker suggested that all pulsars are born as magnetars, and one that no magnetars exist, averring that everything can be done with about 10^{12} G plus other energy sources.

My two favourite papers were, inevitably, first the concluding remarks from Ed van den Heuvel, who highlighted about a third of the presentations, some data, some theory, and, very importantly, some instrumentation (*LOFAR*, *ASKAP*, *QTT*, *FAST*, *etc.*). He noted that some of the most interesting talks had cast doubts on traditional wisdom about pulse profiles and times. Second was the history of pulsar observations in Australia remembered by Richard Wielebinski, who first observed CP 1919 on 1968 March 8 at Parkes, with colleagues from Sydney University and CSIRO, using receivers tuned to 85 and 140 MHz (well, they were Mc/s in those days). The *Nature* article announcing the first four pulsars found at Cambridge appeared in late February. Molonglo and the Mills Cross joined in later that year, finding the first southern pulsars and PSR 0833–45, with then the second shortest period, at 0·089 second, and the second association with a supernova remnant.

Yes, the Crab Nebula was first and was the topic of several talks on its radio and optical emission and polarization. Its nanopulses achieve an equivalent brightness temperature of 10^{42} K (1042 in van den Heuvel's summary, and we'll see how it comes out here).

The meeting marked the 60th birthday of Janusz Gil of the host institution and was in support of building a major planetarium there. I came to the proceedings volume with two questions: (*a*) where is the host institution, the University of Zielona Gora? A map answered that one — not very close to Warsaw or Torun or Krakow (the Polish cities I have visited) and indeed closer to Berlin than to Warsaw, suggesting that the city must have had an earlier rather different name*; and (*b*) is the senior editor, Wojciech Lewandowski, related in any way to the distinguished 19th-Century (1821–1894) composer Louis Lewandowski? In response to an e-query, he says not, at least in any recognizable way. — VIRGINIA TRIMBLE.

*Yes, it did, but who am I to spoil your morning Wiki-moment?

CORRIGENDUM

In the second paragraph below the informal table on page 167, lines 4 and 5, where it says 'radial velocity' it ought to read 'radial-velocity amplitude'.

Here and There

DIVINE OBJECTS

Variable stars are used to measure the dimensions and study the spiritual structures of remote corners of the vast Milky Way. — *The New York Times*, 1956 May 6.

A UNIVERSAL PROBLEM

… dissipation has been very important in bulge formation. — *QJRAS*, **21**, 444, 1980.